196/13.8.70

THE SCIENTIFIC MONTHLY

VOLUME LXXVI
JANUARY TO JUNE

PUBLISHED BY
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
1515 MASSACHUSETTS AVENUE, N.W., WASHINGTON 5, D. C.
1953

BUSINESS PRESS, INC.
LANCASTER, PENNSYLVANIA, U.S.A.

THE
SCIENTIFIC MONTHLY

JANUARY 1953

Some Aspects of Engineering Meteorology

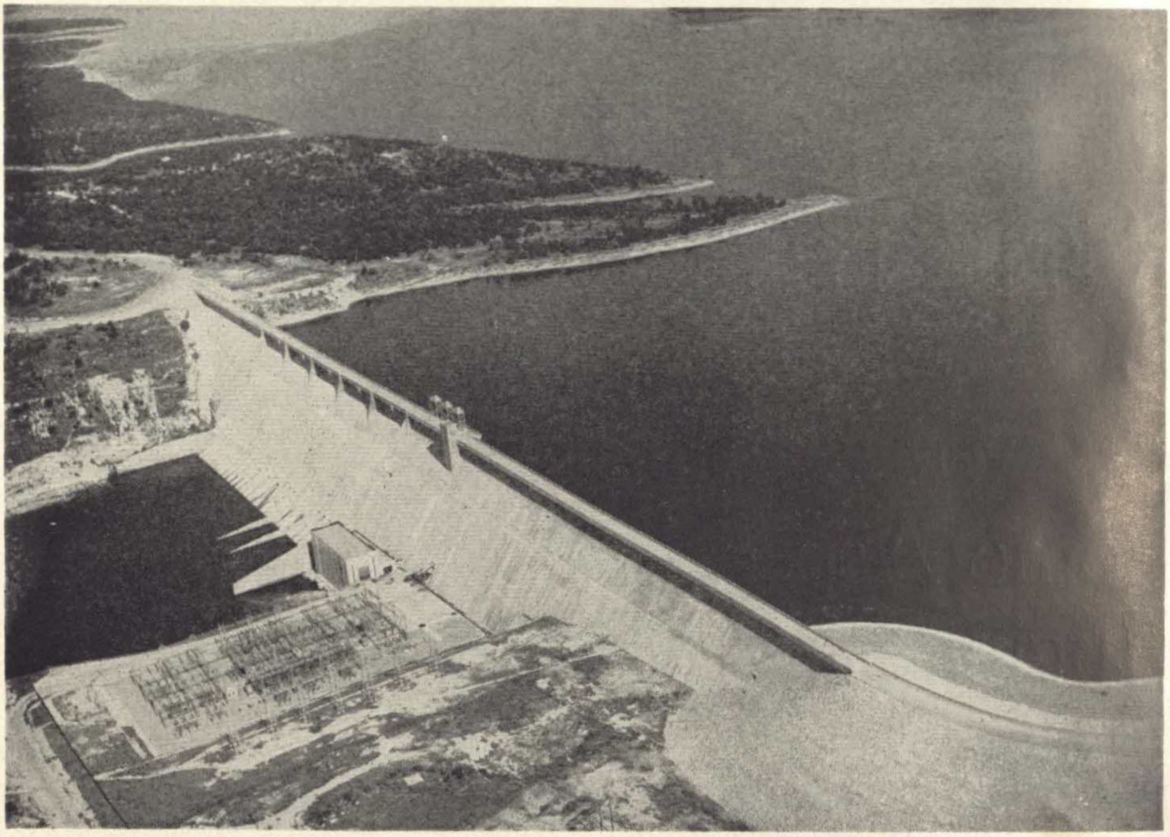
K. H. JEHN*

Kenneth Jehn received his training in meteorology at Rutgers, New York University, and the University of Texas. He was with the Air Force from 1943 to 1946. In the latter year he became meteorologist in the Electrical Engineering Research Laboratory of the University of Texas, Austin. He is now research meteorologist and assistant professor of meteorology.

METEOROLOGY, as the science of atmospheres, logically concerns itself with such analytical studies as the physics, thermodynamics, and fluid dynamics of the atmosphere, as well as with the spatial distributions of meteorological elements which, when integrated over an appreciable time interval, are incorporated into the field of climatology. Beyond what might be termed a study of the physical basis of meteorology, however, is the highly pragmatic consideration of the impact of the atmospheric environment upon living things, including man, and upon the various structures which man has erected upon the face of the earth. It is this very importance of the behavior of the atmospheric environment in the affairs of men which dictates the need for a consideration of meteorological factors in the solution of various engineering problems.

Engineering Meteorology Defined. The term "engineering meteorology" is used in the title of this paper and frequently throughout the text. In view of the rather widespread use of the term "meteorological engineering" in some of the literature cited, some justification for the author's choice will be offered. Engineering meteorology is best defined as the application of meteorological principles and practices to certain engineering problems, just as engineering mathematics is considered to be the application of mathematical principles and practices to certain engineering problems. It is in this sense that the term engineering meteorology is used in this article, and it is plain that many of the authors cited used meteorological engineering in this sense. The term meteorological engineering should be reserved, however, for the application of engineering principles and practices to meteorological problems; more plainly, meteorological engineering is engineering of the atmosphere. In terms of any accepted definition of engineering, this becomes a significant concept, and a practically virgin field of study. A beginning has been made, primarily in the field of applied cloud physics[1] and

* For a critical reading of the manuscript, the author is indebted to J. R. Gerhardt, R. C. Staley, and M. J. Thompson, all of the University of Texas.



Multipurpose dam. Aerial view of giant Mansfield Dam, key flood control structure of the Lower Colorado River Authority's chain of six dams, shows the massive mile-long concrete dam and the 2,000,000 acre-foot reservoir stretching behind it. The hydroelectric power plant is capable of producing 105,000 kw. (Photo courtesy Lower Colorado River Authority.)

in agricultural meteorology, but there are enough problems here to occupy meteorological engineers for centuries to come.

Meteorology Applied to Engineering Fields. During World War II, the application of meteorological knowledge and techniques to military and naval operations achieved a high degree of success. Jacobs[2] treats some of the aspects of climatology applied to war, and indicates the desirability of applying some of the war-developed techniques to civilian needs. These techniques fall into two major categories: the design or planning aspect of meteorology or climatology in engineering problems, and the forecasting aspect. An example will serve to illustrate the differences between them. Suppose that a civil engineer is concerned with the design of a multipurpose hydrologic structure. This design is determined in part by the magnitude of the maximum flood to be expected over the life of the structure. The estimation of this so-called design flood is a hydrological problem and contains meteorological factors that must be taken into account.[3] The best possible use must be made of available precipitation and other climatological data relating to this problem,[4] so that the mathematical probabilities of floods of various magnitudes are made available to the engineer in his calculations. This example represents the systematic use of appropriate climatological factors in solving an engineering design problem. No forecasting is involved, in the usual sense of a particular weather event anticipated at a given time.

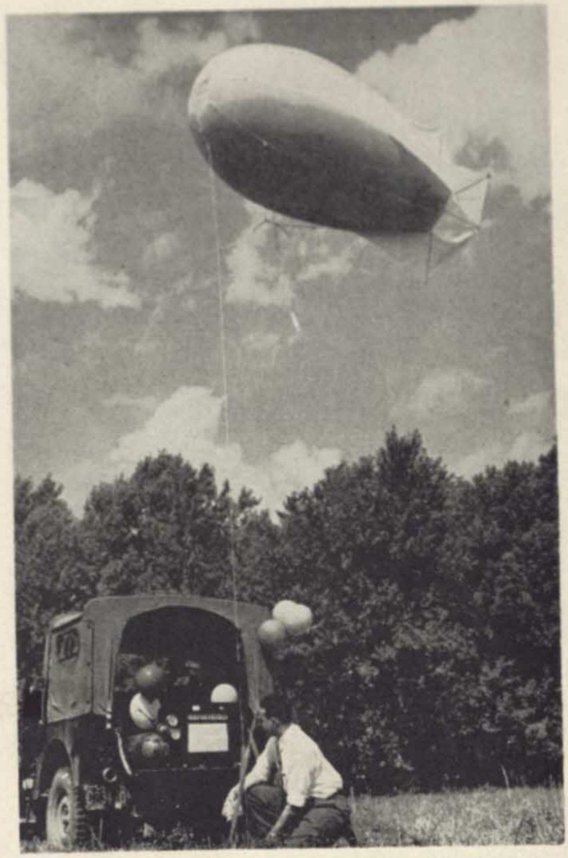
On the other hand, the electric company selling power generated later by this or another hydrologic structure may find that a foreknowledge of precipitation makes it possible for a hydroelectric plant to "increase its output, drawing down the pond, and thus conserving an appreciable amount of water that would otherwise be spilled over the dam."[5] If a drawdown operation could be followed by a return to normal pond elevation in a few days, a large saving in coal burned in the steam generating plant could be achieved. In this case, ac-

curate forecasting of weather conditions over a period of a few days is required.

Engineers have been required many times in the past, and will continue to be required, to solve problems involving the release of industrial wastes to the atmosphere.[6] Fortunately, the atmosphere has practically infinite capacity for the dilution of atmospheric pollution; the engineering problem arises when the rate of dispersion or dilution is insufficient to prevent excessive local concentrations. This fact alone is eloquent testimony of the need for an intelligent appraisal of the meteorological factors, because the diffusion of gaseous or particulate matter in the atmosphere is controlled in large measure by meteorological conditions. The meteorological problem again has two aspects: the forecasting aspect embodied in meteorological control, and the planning aspect involved in the selection of a site for a plant for which there is a difficult waste disposal problem. (The term "meteorological control" is used here in the limited sense of an operational control dependent upon meteorological factors either present or forecast. In a broader sense, meteorological control may be considered the practical equivalent of meteorological engineering. Meteorological control in this broader sense, with particular reference to cloud physics, has been discussed by Langmuir.[7])

With regard to meteorological control in the limited sense, then, which may involve complete or partial shutdown of offending plant operations when atmospheric conditions do not favor rapid dispersion,[8] the problem must be solved on the basis of the particular industry involved. Meteorological control may be used at a smelter, but it is impractical in the case of a steam power plant.[9] Here it might be necessary to remove offending materials before combustion, or to apply washing or precipitation techniques to the flue gases. It is hardly necessary to point out that the relationship of the meteorological factors to the particular operation must be known in order for these techniques to be effective. Moreover, accurate short-range forecasting will increase the effectiveness, permitting better planning of plant operation. The forecasting approach in meteorological control is being used by the meteorology group at the Brookhaven National Laboratory,[10] where an atomic pile has recently been put into operation.

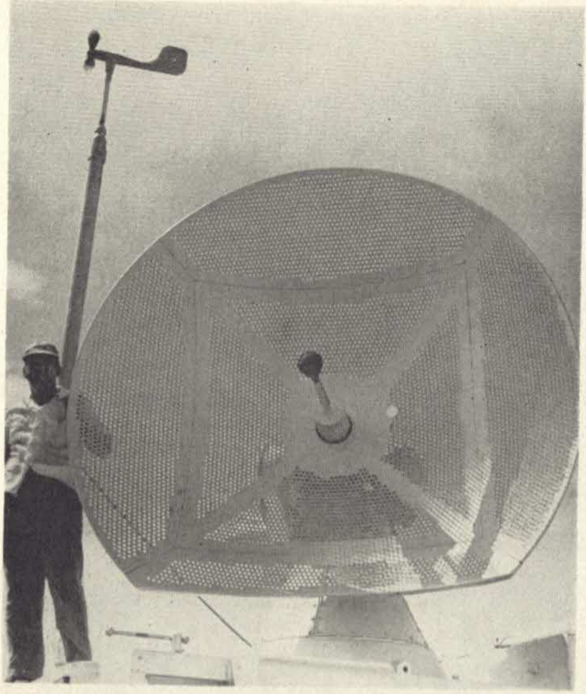
No engineering reference to atmospheric pollution would be complete without a mention of particular cases in which the slowness of atmospheric diffusion of gaseous wastes so built up ground concentrations as to bring disaster to the community near the industrial plant. Such was the case



Rigging KYTOON for measurement of vertical temperature gradient. Recorder in jeep. These data are used to solve site problems, such as setting heating or refrigeration loads. (Photo courtesy E. I. du Pont de Nemours & Co.)

in the Meuse Valley in Belgium in 1930 and in the more recent Donora, Pennsylvania, disaster.[11]

The oil industry, particularly in Gulf Coast locations on land or water, has discovered from experience that the weather enters into its operations in no uncertain terms.[12] One of the most complete treatments of engineering meteorology with particular reference to the oil industry in its Gulf operations is presented by Bates and Glenn,[13] consulting meteorologists, with headquarters in New Orleans and Washington, D. C. These men have profited from their wartime experiences in meteorology, hydrology, and oceanography to the extent of developing a successful meteorological consulting service in the Gulf areas. Particular problems involved here may include practicable conditions for surface or aerial transportation in geophysical exploration; wave and swell conditions during marine construction and drilling; choice of best working months for different types of operations; and the location and



Radar antenna at Grand Isle, Louisiana, used in detecting and tracking storms in the Gulf of Mexico near oil drilling platforms. (A. H. Glenn & Associates photograph.)

utilization of various installations influenced by such factors as surface temperature, precipitation, humidity, and surface winds.

These examples indicate some of the types of difficulties which the engineering meteorologist is likely to encounter. Among other problems is the providing of general weather information about locations being considered for new plant sites. The planning groups may need to be provided with sufficient data so that the potential cost difference between sites can be evaluated from the weather viewpoint. In the design of the components of an industrial plant, precise weather data must be supplied "so that the best estimates may be of the efficiency of different kinds of equipment, as well as the first and operating costs of that equipment."[14] Examples of the type of equipment affected by the weather are such things as the atmospheric cooling tower, heating and air conditioning equipment, and cooling ponds.

Plant location and design need not be industrial to be affected by the weather, of course, and the architect and architectural engineer can use the meteorologist's special skills to advantage. A consideration of architectural problems from the weather point of view introduces the concept of local climate, or microclimate. Significant variations of the microclimate occur over relatively short horizontal and vertical distances.[15] It is to the advantage of the architect and the people he serves to take the microclimate into account; an appropriately trained meteorologist is the specialist who can best assist in this problem. It is worthy of note that the editors of *House Beautiful* have made a significant contribution to a better understanding of this field in their series on climate control.[16]

The civil engineer may be concerned with the design and location of structures other than hydrologic ones. Highways, for example, pose engineering problems which depend for their effective solution on a knowledge of the state of the atmosphere.[17]

The electrical engineer may be concerned with the effects of severe weather upon power transmission; some power companies have worked out detailed studies of weather factors in their operations, with the result that load dispatching is more efficiently carried out.[18]

One of several waterspouts observed from a crewboat tied alongside an offshore drilling platform approximately six miles south of Grand Isle, Louisiana. The waterspout was associated with convective activity set off by a slow-moving cold front south of Grand Isle. (Photo by Mel Coston, Humble Oil and Refining Co.)

Some Research Aspects of Engineering Meteorology. The meteorologist also enters the engineering field in a more fundamental way. In the sense that the meteorologist attempts to discover the physical basis of atmospheric phenomena, he becomes a valuable member of the community of scientists engaged in research in various engineering (and other) fields. It is possible, therefore, to consider another aspect of engineering meteorology in which the engineer and the meteorologist work together. Again a few examples will serve to establish the nature of this relationship.

Practicing engineers concerned with air pollution are familiar with the semi-empirical equation of Bosanquet and Pearson,[19] which relates the various parameters in atmospheric diffusion. The British meteorologist O. G. Sutton[20] has developed a theoretical approach to the atmospheric diffusion problem based on G. I. Taylor's statistical theory of turbulence. Chemical engineers, using field data (concentrations of SO_2) from smelters in several locations in the United States, have found that the data from tall stacks agree well with the theoretical equations.[21]

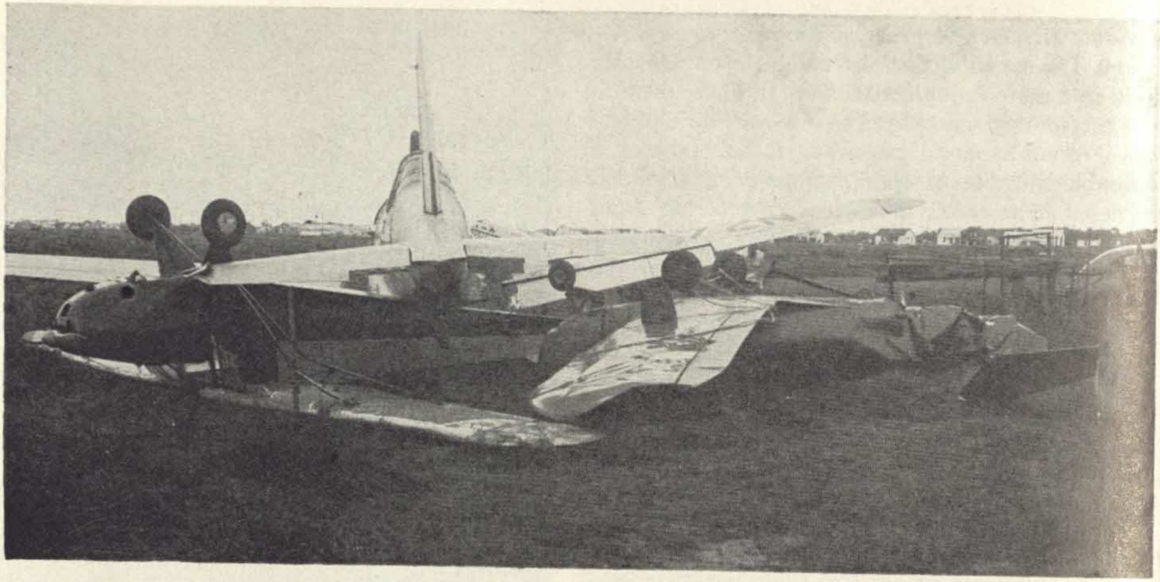
The design of an aircraft must of necessity be governed in part by the characteristics of the atmosphere through which the plane must eventually fly. The mean state of the atmosphere, represented, for example, by the NACA tables, is insufficient to specify the stresses a plane may undergo in the turbulent inner portions of a cumulus cloud, or even in landing or taking off in the rough air in the lowest few hundred feet of the atmosphere. The meteorologist and aeronautical engineer, therefore, have a common interest in the atmosphere, and particularly in the turbulent currents which occur in varying degrees of intensity and scale. As a consequence, a considerable aerodynamic literature has been developed, with contributions from both aeronautical engineers[22] and meteorologists.[23]

In the field of communication engineering, the effects of meteorological factors on transmission of short radio waves through the atmosphere are being studied intensively, and a considerable literature on radio meteorology has also been built up. The Electrical Engineering Research Laboratory of the University of Texas has pioneered in this work, and any one of several references[24] will serve as an introduction to the meteorology of radio engineering.

It is seen that a distinction can be made between what might be termed practical engineering meteorology and the research aspects. This distinction is in many respects similar to the distinction between applied and pure research; neither can advance significantly without the other.[25] It was to delimit this paper that primary emphasis has been placed upon practical engineering meteorology.

Equipment used in measuring short-wave radio propagation in the lower atmosphere on a 30-mile overwater path near Galveston. (Photo courtesy Electrical Engineering Research Laboratory, University of Texas.)

Features Common to Engineering Fields. Throughout these examples of engineering meteorology, certain features stand out. First, it is evident that many engineering problems contain certain weather factors, probability forecasts for which are needed in the solution of the problems. "Equipped with such knowledge [the engineer] can design to avoid failure without resort to blind extravagance; engage in sharp competitive bidding without jeopardizing his financial position; take advantage of Nature when she is kind, and protect himself against her when she engages in excesses."[26] Second, there are distinct differences between the design and forecasting phases of engineering meteorology; both the meteorologist and the engineer must recognize these differences in order most effectively to utilize meteorological and climatological data. It is plain that later accurate forecasting cannot make up for poor original design. Third, engineering meteorology is largely of a consulting



Views of hurricane damage at Freeport, Texas, following storm of October 3–4, 1949. (Photos courtesy The Dow Chemical Company.)

nature. This implies private meteorological consultants, selling their services to various industries and businesses, but certainly does not preclude staff consultants on the pay roll of an industrial firm. "The usefulness of the weather engineer is judged by his ability to provide the company with the means for more efficient operation . . . by utilizing a consulting technique to give advice to various divisions of the concern, for example, management, design, operating sales, or planning sections."[27] Fourth, the effective engineering meteorologist must acquire technical knowledge concerning the industrial or business operations with which he is working. The implication here is that more efficient use of the meteorologist's services can be made if he has a staff rather than a private consultant's position. Obvious exceptions exist, of course, and it becomes necessary for the company to decide whether to hire a full-time meteorologist or to pay a consultant for particular services.

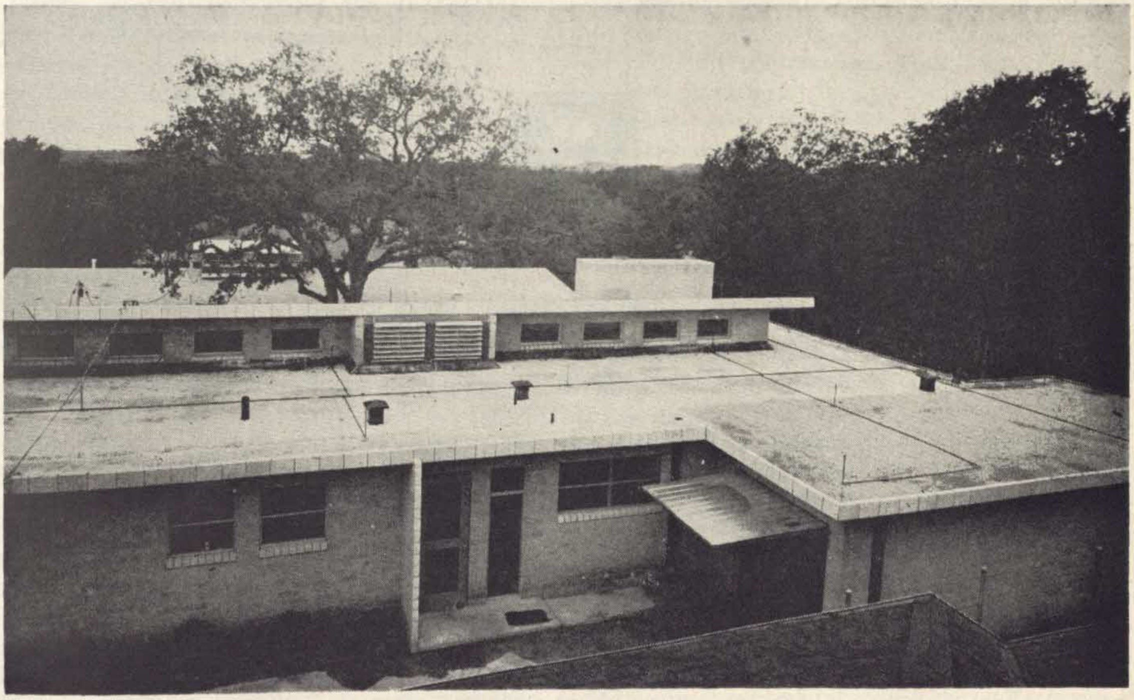
Training for Engineering Meteorology. For details of the recent (1949) status of applied meteorology in the United States, reference is made to Bates.[28] By and large, it appears that consulting and staff meteorologists are making significant

contributions to the solution of engineering and business problems. The development has been slow, in spite of the farsighted view of the situation offered by Rossby in 1945.[29] One of the important reasons for this slow development is the general lack of training in engineering meteorology. According to Bates, this problem might be remedied if engineering schools with meteorology in the curriculum would specialize in engineering meteorology. To the best of the author's knowledge, there is no such school at the present time. Jacobs first noted this lack in 1947.[30]

A realistic approach must involve a capable practicing engineering or industrial meteorologist, who would be invited to give a series of lectures or a full course of study as a visiting professor on the faculty of an engineering college. Such a course should be on the advanced undergraduate and graduate level, and would serve to pass certain fundamentals and practical knowledge on to meteorology students and faculty. Because of the diversified problems found in engineering fields, several lecture series would need to be arranged over a period of years. On the whole, the meteorological faculties at the various engineering colleges are potentially equipped to undertake such training, but need the infusion of experience which could come in part from the visiting lecturer and in part from the professional practice of engineering meteorology during leaves of absence from teaching. From a small beginning, and with adequate opportunities for students and staff to practice engineering meteorology on even a small scale, it is conceivable that a suitable course of study would eventually be evolved, and that eminently qualified graduates would enter the field.

It is the opinion of this writer that the approach used by the Illinois Institute of Technology[31] is not the answer to the problem of training in engineering meteorology. Obviously, to acquaint engineering students with meteorology as it affects engineering practice is a desirable thing; however, the primary need is for professional meteorologists, trained in basic engineering fields, who will extend and enlarge the scope of engineering meteorology through professional practice.

Engineering meteorology is still in swaddling clothes; perhaps it is not vain to hope that it will soon enter the "vigorous stage of advancement" now enjoyed by the field of industrial hygiene. According to Patty,[32] "Industrial hygiene is no longer seen by industry as the aimless effort of intellectuals collecting bottles filled with nothing so that they can prepare long and useless discourses that few read or understand, or, if they did understand, would know what action to take." In the sense that



Masonry dwelling utilizes a roof sprinkling system as a summer cooling device. (Acme Ceramic Home Program, Austin, Texas.)

meteorology and medicine enjoy similar problems of diagnosis and prognosis in the face of sometimes great uncertainty, so industrial hygiene has passed and industrial meteorology is passing through a stage of development which involves proving its worth to industry and the engineering fraternity.

References

1. Langmuir, I. *J. Meteorol.,* 5, 175 (1948).
2. Jacobs, W. C. *Meteorol. Monographs,* 1, 1 (1947).
3. McClendon, E. W. *Bull Am. Meteorol. Soc.,* 31, 238 (1950).
4. Bernard, M., *et al. Hydrology Handbook.* New York: American Society of Civil Engineers (1949).
5. Dryar, H. A. *Bull. Am. Meteorol. Soc.,* 30, 159 (1949).
6. Swain, R. E. *Ind. Eng. Chem.,* 41, 2384 (1949).
7. Langmuir, I. *Science,* 112, 35 (1950).
8. Hewson, E. W. *Ind. Eng. Chem.,* 36, 195 (1944).
9. Stern, A. C. *Heating and Ventilating,* 42, 64 (Jan. 1945); 73 (Feb. 1945).
10. Smith, M. E. Atomic Energy Commn. pub. AECU-702 (1950).
11. Brandt, A. D. *Heating, Piping, Air Conditioning,* 21, 97 (1949); Schrenk, H. H., *et al. Public Health Bull. No. 306.* (The need for an authoritative and complete annotated bibliography of the air pollution literature has been met through publication by the Am. Meteorol. Soc. of H. P. Kramer and M. Rigby's Cumulative Annotated Bibliography on Atmospheric Pollution by Smoke and Gases, in *Meteorol. Abstr. and Bibliog.,* 1, 46 [Jan. 1950]).
12. Fagin, K. M. *Petroleum Engr.,* 19, 82 (1948); 21, B65 (1949).
13. Bates, C. C., and Glenn, A. H. *Oil Weekly,* 125, 33 (May 19, 1947); 42 (May 26, 1947).
14. Gosline, C. A. *Bull. Am. Meteorol. Soc.,* 30, 119 (1949).
15. Landsberg, H. E. *Weatherwise,* 3, 7 (1950); Baum, W. A. *Ibid.,* 2, 75 (1949).
16. Siple, P., *et al. House Beautiful,* 91, 136 (1949). (This is a continuing project, and articles on the general subject of climate control have appeared in succeeding issues, the latest in July 1952.)
17. Landsberg, H. E. *Better Roads,* 17, 25 (1947).
18. Dryar, H. A. *Op. cit.*
19. Bosanquet, C. H., and Pearson, J. L. *Trans. Faraday Soc.,* 32, 1249 (1936); Bosanquet, C. H. *J. Inst. Fuel,* 8, 119 (1935).
20. Sutton, O. G. *Quart. J. Roy. Meteorol. Soc.,* 73, 426 (1947).
21. Thomas, M. D., *et al. Ind. Eng. Chem.,* 41, 2409 (1949).
22. Dryden, H. L. *Quart. Appl. Math.,* 1, 7 (April 1943).
23. Taylor, G. I. *Proc. Roy. Soc.* (London), A 151, 421 (1935).
24. Appleton, E., *et al.* Meteorological Factors in Radiowave Propagation (Report of a conference held in London in 1946 by the Physical Soc. and The Roy. Meteorol. Soc.), London: The Physical Soc.; Booker, H. G., and Gordon, W. E. *Proc. I. R. E.,* 38, 401 (1950); Kerr, D. E., Ed. *Propagation of Short Radio Waves* (1950).
25. Morse, P. M. *Am. Scientist,* 38, 253 (1950).
26. Bernard, M. *Bull. Am. Meteorol. Soc.,* 31, 194 (1950).
27. Gosline, C. A. *Op. cit.*
28. Bates, C. C. *Bull. Am. Meteorol. Soc.,* 30, 199 (1949).
29. Rossby, C.-G. An Appraisal of the Present Status of Meteorology and its Ability to Serve Industry. Mimeographed notes (1945).
30. Jacobs, W. C. *Op. cit.*
31. Malkus, J. S. *Bull. Am. Meteorol. Soc.,* 30, 310 (1949).
32. Patty, F. A., Ed. *Industrial Hygiene and Toxicology,* Vol. 1. New York: Interscience, 15 (1948).

INFINITUDE

Beyond my heart there is another heart,
Beyond these hearts, the world, and then an age,
And ages in themselves are infinite.
Beyond this world there is another world,
A hundred million solar systems that combine
To form a single galaxy, indefinite.
From galaxy to galaxy the universe
Expands, and ages that have been and yet will be—
What meaning have they for this shackled mind,
This microcosm of Intelligence,
That seeks, but ever binds the heart of me?

Rhoda Walton Leonard

Los Angeles, California

The Visa Problem

SIDNEY PAINTER, H. A. MEYERHOFF, and
ALAN T. WATERMAN

Dr. Painter, professor of history at The Johns Hopkins University, is a director of the American Council of Learned Societies and a member of the council's special committee on passport and visa problems. Dr. Waterman is director of the National Science Foundation, and Dr. Meyerhoff is administrative secretary of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ON OCTOBER 27, by formal request, spokesmen for the American Council of Learned Societies, the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, and the National Science Foundation submitted prepared statements before the President's Commission on Immigration and Naturalization. Although the commission is primarily concerned with problems affecting immigration, the three organizations were specifically asked to investigate the effect of the *McCarran Act* of 1950 on visits of foreign scholars to this country.

Representatives of the council, the foundation, and the AAAS conferred prior to the hearing to assure adequate coverage of the subject without serious duplication, and it was quickly discovered that each organization felt it had a specific and clearly defined function to perform, as the ensuing statements will demonstrate. The American Council of Learned Societies, with the cooperation of the American Council on Education, was especially concerned with exchange of scholars in all fields of learning. The AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE dealt with the problem as it relates to scientists, supporting its stand with cases supplied through the cooperation of the American Chemical Society, the American Institute of Biological Sciences, the American Institute of Physics, the American Psychological Association, the Federation of American Societies for Experimental Biology, the mathematical societies, the National Research Council, and the editorial staff of the *Bulletin of the Atomic Scientists*. The foundation gathered its background material chiefly from government agencies interested in, and concerned with, scientific research. Without minimizing the importance of measures designed to safeguard national security, the three organizations made a strong case for drastic liberalization of the *McCarran Act* of 1950 and, by implication, the *McCarran-Walter Act* of 1952, as well as of the administration of these acts by the departments of State and Justice.

The ACLS Statement

The American Council on Education has indicated its interest in the passport and visa problems affecting scholars in the humanities and the social sciences by consulting with the American Council of Learned Societies about this statement and has had occasion in the past few years to deal with various government agencies concerned with this problem.

Both these organizations and their constituent societies are vitally interested in anything that affects the international interchange of scholarly publications and personnel insofar as it concerns the development of their respective fields of scholarship. In every field of scholarship in the humanities and social sciences the advancement of knowledge and the promotion of research activities are dependent upon contributions made in many countries. For example, in the field of Biblical studies we depend heavily on the scholars in the University of Jersualem who are close to the source of new materials. Besides this routine day-to-day interest in the effective functioning of international interchange, the ACLS and its constituent societies support the general policy of international intellectual cooperation which they share with other groups of our citizens in the development of a peaceful and friendly free world. This policy has now received legislative formulation as a national policy of our government by several acts of Congress.

In passing the *Fulbright Act* the Congress of the United States clearly expressed its belief that the exchange of scholars between this country and others served the interests of the United States. Congress has confirmed its acceptance of this point of view by generous appropriations to carry out the Fulbright program, both by setting up agencies to administer it and by supplying supplementary funds.

Moreover, Congress in the *Educational Exchange Act* of January 27, 1948, made provision for interchange between the United States and other

countries of students, professors, and leaders in fields of specialized knowledge or skill. In that act Congress also provided for the establishment of the United States Advisory Commission on Educational Exchange to be appointed by the President, with the advice and consent of the Senate. This commission consists of five members, not more than three of whom may be of the same political party. Distinguished educators, including Harold W. Dodds, president of Princeton University, have served on that commission. Thus Congress has made clear that it regards the international exchange of scholars as a valuable contribution to the public interest.

This policy established by Congress has received strong support from all those who are interested in the advance of scholarship and the promotion of intellectual cooperation both here and abroad. The various foundations and institutions of higher education have brought foreign scholars to America and facilitated foreign travel and study for the scholars of the United States. The personnel of foundations and universities have devoted a large amount of time and energy to administering this international exchange of scholars. In short, it seems clear that the exchange of scholars is an important part of the policy of the United States, and large amounts of both public and private funds have been devoted to it. Anything that interferes with the effectiveness of this exchange should be a cause of grave concern to the country.

The American Council of Learned Societies is one of the organizations that have taken an active part in encouraging the international exchange of scholars. The learned societies that compose it have a vital interest in the exchange of ideas and knowledge between the scholars of the United States and those of other lands. They also value the effect of this exchange on international amity and understanding. The directors of the council have for some time been disturbed by reports that the policy of the United States in issuing visas and the administration of this policy have done much to nullify the effectiveness of the program. In its spring meeting the Board of Directors appointed a committee to investigate this situation and to recommend action if it seemed desirable and feasible.

The directors of the council are not as yet in a position to present statistics or to attempt to assess causes, but there is ample evidence of the seriousness of the situation. Foreign scholars invited to come to the United States are subjected to extensive and humiliating inquisitions and incredibly formidable questionnaires. This alone does much to prejudice them against the United States. But far more serious are the delays involved before the visa is received. In many cases the visa has come so late that the opportunity to visit the United States no longer exists.

These annoyances and long delays both hamper the arrangement of exchanges and create bad feeling which affects far more than the individual scholar concerned. Reports of these annoyances and delays spread rapidly, and the reputation of the United States is gravely injured. In England, at least, there is a serious question whether the damage done by reports of the humiliations inflicted on scholars planning to visit the United States has not overbalanced the good effects of the exchanges made.

The council is fully aware of the difficulties involved in enforcing the immigration acts passed by Congress. It has only one suggestion to make. Scholars are as a rule well known to their colleagues, and their opinions and activities are rarely secret. Most of them are attached to institutions of learning. Might not a statement from a university or research institution be accepted prima facie as adequate evidence of a scholar's suitability for admission to this country on a nonimmigrant basis?

Moreover, in view of the fact that visiting scholars are not numerous in comparison with the traveling public generally, and are usually coming to the United States to attend a particular conference or school term, with fixed and definite dates, it seems that possibly an expedited procedure for handling these cases could be established, if the consular authorities were appropriately instructed by the Department of State regarding the importance of promoting the congressional policy regarding educational exchange. They should also be informed of the possible detrimental effects to international goodwill which might be produced by failure to handle these applications promptly. On account of the status of the parties concerned, these consequences in such cases would be out of proportion to the number of persons affected.

The AAAS Statement

It is my understanding that the members of the commission are especially interested in securing testimony relative to the visa problem, which affects foreign scientists who wish to visit this country on scientific missions, rather than to immigrate with the intention of becoming American citizens. The information on immigration as it relates to foreign scientists is meager and difficult to assemble, and no effort will be made to cover this aspect of

the subject which, however, I believe has been dealt with by other witnesses. It should be noted that the implications of visitation are so drastically different from those of immigration that the two merit not only separate consideration but distinctive procedural handling.

The AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE and its affiliates have been interested in assuring relative freedom of movement for scientists and other professional people from one country to another, subject to necessary but reasonable security regulations, ever since the termination of the war in 1945. The Association's interest is based on the experience that science is international, discoveries in basic science may occur in any country, and the only way in which our nation can remain in the forefront of scientific and technological development is through unhampered intercommunication. The problems relative to international travel, except in the countries behind the Iron Curtain, did not become well defined until the passage of the legislation now in force. Up to the present time there has been no official consideration or action with reference to the implications of the new legislation.

The Association has taken official cognizance of problems encountered in administering existing legislation through its Council, which passed a resolution at Philadelphia on December 29, 1951. The AAAS Council is the policy-making body of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, comprising approximately 250 members, some of whom represent the 48,000 individual members of the AAAS, but most of whom are official representatives appointed by the 184 affiliated societies, which are entitled to one or two representatives, depending upon the size of their respective memberships.

The resolution that was adopted at the Philadelphia meeting of the Council was, naturally, concerned with the two-way movement of scientists; hence it deals with both passports and visas. Inasmuch as it sets forth a widespread, though by no means unanimous, reaction of scientists to certain provisions of the law and to its administration, it has pertinence in this testimony, and I quote it herewith:

> The Council of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE is profoundly disturbed over the present world conditions which so severely impede the free interchange of knowledge even among friendly nations. Danger to the future of our nation is implicit in such restrictions.
>
> The Council recognizes the need for measures which will effectively safeguard our security, but expresses its troubled concern over the manner in which such measures, in particular the *McCarran Act*, are being administered, to prohibit American citizens from going abroad and citizens of other nations from coming here to interchange knowledge of science which does not affect security.
>
> The Council strongly urges that the administrative procedures under the *McCarran Act* be reviewed and modified so as to minimize injustices and to increase both our internal strength and our prestige abroad.
>
> The Council further urges revision and improvement of the relevant portions of the act, to retain the objectives of necessary security, but with adequate provisions to maintain free interchange of knowledge that has no security implications.

The phraseology of the resolution indicates quite clearly that American scientists have, from time to time, encountered difficulty in securing passports for travel abroad and that foreign scientists have likewise encountered comparable difficulties in carrying out plans to visit this country. There has, of course, been no opportunity for additional experiences under the new legislation, but there is no reason to believe that the situation will change in any essential way. Inevitably the operation of any law becomes evident in its application to individual cases, and the only effective way to deal critically with a law is through the discussion of specific cases.

This type of procedure is fraught with dangers insofar as the cases presented may or may not possess validity. It will be readily appreciated by the commission that scientists and scientific organizations are not in a position to investigate, or to test the validity of, individual cases; yet it is important to recognize that the invalidity of any individual case does not in any way weaken criticism that would have applied had the case possessed validity. Fortunately, the experiences of many foreign scientists have been quite carefully documented, and twenty-six of them have been presented in the October 1952 issue of the *Bulletin of the Atomic Scientists.* I wish to enter this issue of the periodical in question officially into the record that will be studied by the commission. Despite the documentation, I wish to make no specific claim for the validity of any of the cases presented in detail, although I might state that the statement prepared by Michael Polanyi seems to merit careful study and analysis.

I am prepared to add to the twenty-six cases presented in this special issue of the *Bulletin of the Atomic Scientists,* if the commission so desires. The material that has been placed in my hands or that has come directly to the Association from individuals involved in visa problems provides information on eight additional cases, as well as supplementary documentation for some of the cases presented in the *Bulletin*. It seems of greater

importance, however, in this brief statement to extend the testimony in a different direction, and also to make some effort to classify and to systematize it.

Of very serious concern to the scientists of this country is the fact that, within the past twelve months, scientists in at least five different fields have definitely decided against holding international meetings in the United States. The most candid reaction has come from the psychologists, and a statement from the American Psychological Association dealing with this matter has been read into the *Congressional Record* (98, [88], 5920 [1952]). Here it was stated that, in deciding to hold the 1954 International Congress of Psychology in Canada, the psychologists agreed not to hold a meeting in the United States until and unless the existing legislation is "modified in such a way that visiting scientists will not be put through an inconvenient and embarrassing procedure in order to gain permission to visit this country."

Comparable action was taken by the International Congress of Genetics in voting to hold the ninth congress in Italy in 1953, and the tenth congress probably in Canada in preference to the United States. Although the astronomers of the International Astronomical Union were somewhat more reticent in official records regarding the locale of their next meeting, it is understood they declined an invitation to meet in the United States for the same reason. The 1954 International Federation of Documentation and the nineteenth International Physiological Congress will also meet outside the United States, notwithstanding invitations to meet here, but other reasons were given for the decisions in both these cases—specifically, the unfavorable rate of foreign exchange.

With respect to the individual scientists who have been denied entry or who have been delayed in entering the United States, analysis reveals that they can be classified into a comparatively small number of types.

1. In regard to foreigners who are active members of the Communist Party, the *McCarran Act* of 1950 is specific, and no particular issue has arisen, although the need to deal as rigorously with visitors as with immigrants has been questioned.

2. Foreign scientists who at any time past had admitted or alleged connections with the Communist Party have been given the same treatment as active Communists, yet their cases have possessed varying degrees of merit, to which adequate consideration has rarely been given. Enforced membership in the Communist Party without Communist sympathy, or temporary membership in the party for the purpose of combating Nazi occupation through "underground" activity, and voluntary membership that was ultimately renounced when full comprehension of the implications of Communism was acquired are three types of background cases encountered in this general category. Analogous situations have been faced in dealing with German citizens who had been forced into nominal membership in the Nazi Party in Germany. The new *McCarran-Walter Act* of 1952 sets up machinery for appeals by individuals in this category, but decisions are still discretionary and hence problematic.

3. Categories 1 and 2 are created by specific provisions of the act, but a much larger group of prospective scientific visitors is affected by the administration of the act. Foreign scientists whose political records are above reproach and whom the act was clearly designed not to exclude commonly find themselves involved in the operation of machinery that has not been satisfactorily geared to the volume or the kind of business that must be processed. A few sensitive individuals have resented some of the questions that are asked in conformance with the requirements of the law. Many scientists have experienced such long delays in action on their applications for visas that the events in which they had planned to participate in this country were things of the past before permission to enter the United States was granted. A disconcertingly large number of scientists who were planning longer stays —in some instances as visiting professors in educational institutions—have been delayed or even precluded from entry by consular insistence on additional evidence regarding the adequacy of financial support. Only rarely have the personnel in U. S. consular offices been aware of the dignity and esteem attached to scholastic distinction abroad, and the treatment that has been accorded several foreign scientists by our consular agents has generated, and is generating, an atmosphere of international ill will that has prompted scientists in some of the Western democracies to bracket the United States with the USSR in respect to attitude toward foreign visitors. Inaccurate and unjust as such a comparison may be, its causal relation to the visa problem is ample evidence of defects in the existing machinery of administration that demand correction.

The visa situation should be viewed, and reviewed, against a background in which national security is a dominant factor. As the Association's resolution of December 29, 1951, clearly demonstrates, the scientific profession does not minimize the need to protect our democracy against subversive influences and to guard our scientific and technological secrets. Scientists, however, are only too acutely aware of the fact that there is no protection against the independent discovery of supposed secrets; that scientific progress has international roots, that draw upon basic discoveries made in many different countries for sustenance; that free intercommunication, which will give American scientists quick access to new scientific developments in other countries, is not only vital to the national welfare but crucial in preserving our national security. Scientists heartily endorse the kind of caution that excludes the subversive who seeks to obtain and to export our technological secrets and to import propaganda that aims at the overthrow of our democracy, but they consider any

barrier to the free flow of information into this country as an even greater threat to national security. There is such a threat—inadvertent, to be sure—in certain of the provisions of the present law and in its administration, and it is only this threat that the profession seeks to remove. The importance of the international exchange of scientific information has been partially set forth in an article under that title, prepared by Wallace R. Brode, associate director of the National Bureau of Standards, and published in Vol. 28, No. 50, of *Chemical and Engineering News* (pp. 4332–38) on December 11, 1950, and a reprint of this article is herewith submitted for entry into the record. Although certain of the problems outlined by Dr. Brode have been accentuated during the twenty-two months that have elapsed since the article was printed, his remarks still provide essential background material in dealing with the movement of foreign scientists into this country and of American scientists into foreign countries.

In the preceding remarks, the impression may have been given that the mesh that screens foreign scientists seeking to enter the United States is too fine, and in general this is true. American scientists, especially those working in government laboratories, where highly classified research is being carried on, have, however, been perplexed by statements from the Department of State to the effect that the possession of a visa on the part of a foreign scientist is no guarantee that he is a good security risk, and that possession of the visa does not entitle him to visit laboratories in which classified research is in progress. We are thus confronted with the fact that the task of screening scientific visitors is being carried on so imperfectly that many who are entitled to enter this country are excluded, whereas others, who are admittedly poor security risks, obtain entry. It is understandable that the Department of State should be unwilling to assume responsibility for permitting foreign visitors to have access to classified information and operations; but this very fact indicates that screening cannot be done in the consular offices and should not be attempted, and that more uniform and somewhat more liberal policies should be adopted in giving visas to scientists who are planning comparatively short stays in the United States. The task of screening these visitors for possible access to classified projects and information can more effectively be conducted in this country by agencies that are staffed and equipped for such investigation as may be appropriate.

This rather brief and incomplete summary indicates that the existing legislation—and presumably impending legislation—should be further reviewed and should be revised on the basis of experience that has been acquired since 1950, which was not available when the legislation was drafted and passed by the Congress. From the cases that have been studied and classified it may be concluded that:

1. The provisions of the current law have been responsible for several grave injustices to individuals, whose exclusion from the United States must be viewed as a loss to American science, as well as a setback to American prestige and international good will.

2. The administration of the existing legislation is unsatisfactory, in part because;

a) the facilities for handling the volume of business are inadequate;

b) the personnel upon whom the primary responsibility of administration has fallen only exceptionally have the background and training to handle it judiciously;

c) insofar as current legislation requires investigative procedures, this responsibility cannot be assumed or handled expeditiously by those to whom it has been delegated;

3. Although the new legislation provides machinery for appeals from adverse and presumably unfair decisions in certain cases, it does not solve the major problems or remove the causes for growing international friction and ill will;

4. Without relaxing vigilance against the infiltration of Communists, the law may appropriately and profitably be liberalized and some of the problems solved by distinguishing between those who propose to immigrate into this country and whose qualifications for American citizenship should be critically scrutinized, and those who merely plan short visits and whose scientific knowledge is potentially of value to American institutions and scientific organizations.

I have no wish to leave with the commission the impression that scientists have a distorted perspective on the visa question. The colleagues who have supplied me with information have mentioned as many cases in which foreign scientists have entered this country without trouble or delay, as they have cases where unwarrantd difficulty or outright refusal was experienced. They are rightly concerned, however, with the imperfections of the law and its administration, because these imperfections are creating ill will that is being reflected in the increasing number of decisions on the part of international scientific bodies not to schedule meetings in the United States. If this trend continues, American science faces the threat not merely of becoming provincial but also of becoming atrophied to the point where the national welfare and national security will suffer. Security and welfare are founded on knowledge, only part of which originates within the confines of the United States.

THE NSF STATEMENT

The invitation to the foundation was to testify concerning the impact of the immigration laws

upon science. For the most part the effect of the immigration laws upon science is not substantially different from the effect upon other professional and scholarly activities. In matters concerning the admission of foreign scientists as visitors, however, experience has demonstrated the existence of a problem of special concern to science and one in which the stake of the country is large. It is, therefore, to this special problem that I shall confine my attention.

I should like to place my remarks in perspective by indicating the nature of the interest and the competence of the National Science Foundation in this field. The creation of the National Science Foundation by Congress in 1950 was itself recognition of a fact to which the national and international events in the first half of this century bear witness: the emergence of science and technology as a crucial and sometimes decisive factor in the rise and fall of nations and the personal destinies of all men. The nations of the free world are now engaged in a grim and seemingly endless struggle to maintain the precarious balance for peace and security. In this struggle, the decisive edge in military strength or, if our hopes are realized, in the peaceful development of the economic resources of our world, is likely to go to that nation or group of nations which most successfully supports and develops its scientific and technological strength.

Since the late 1930s, when the magnitude of this country's stake in vigorous scientific research and development began to be apparent, the resources of the government have been marshaled in support of science. Today the federal government's annual budget for scientific research and development is of the order of 2 billion dollars, to which private enterprise and the universities add perhaps 50 per cent more. Nine federal agencies, in addition to the foundation, pursue major research programs covering widely the scientific fields known to man. The National Science Foundation, however, was devised in the years following the end of World War II, "as a much-needed keystone in the structure of the national research program"—to use the words of the President in transmitting the foundation's first annual report to the Congress. One of its principal tasks is to appraise the rapid growth of research activity, both public and private, and to recommend the broad goals toward which this effort should be channeled. The foundation is also directed by the *National Science Foundation Act* to cooperate in international research activities. It is principally in these capacities, then, as the adviser to the government on national policy with respect to scientific research, and as a major agency concerned with international cooperation in scientific research, that the foundation has approached the problem of foreign scientific visitors under the immigration laws.

In assessing the problem of the federal government, the foundation has drawn upon the experience of other government agencies and, through the wide contacts of the foundation with the scientific community in this country, upon the experience of scientists themselves. Upon the basis of information available to the foundation through these channels, it is clear that the provisions of the present immigration laws governing the temporary admission of aliens to this country, and the administration of these laws, have created a problem. If the solution to this problem is long delayed, a seriously detrimental effect on the strength of science in this country may be expected. Any such handicap to our progress in science will in turn unquestionably react adversely on our welfare and security in the years ahead. A further consequence would be a weakening of cooperative relationships with friendly countries in an important component of our common defense—namely, scientific research and development.

The problem arises in the restrictions on temporary admission of an alien visitor, now stated in Section 137 of the 1950 law and retained in the *McCarran-Walter Act*. Since these restrictions have been in effect since 1950, we have had an opportunity to observe their consequences for science. Opinion among scientists is practically unanimous that they have brought about deterioration in the relationships of American scientists with their opposite numbers in countries friendly to the United States, particularly in the United Kingdom and Western Europe.

Effective scientific research calls for creative ability of an outstanding order. Such ability is no respecter of national boundaries. At a given time in a given field of science the leaders in the field are usually found in at least several countries in the world, and the researchers in the field in practically all. Much of the progress in science is achieved through the inspiration and guidance of the few individuals of outstanding competence and experience. For progress on the frontiers of science it is especially necessary that these leaders have opportunities to discuss their ideas and plans with each other and with the large group of research workers who are providing the body of research which comprises that field of science. Observations and conclusions reached by competent scientists in any one country are invaluable to the research of scientists in other countries working on the same or similar

problems. Although I am speaking here primarily of basic fundamental research—i.e., research on a frontier of science—the importance of this exchange of information is no less for our applied research and technology. There is overwhelming evidence on this score. Until well into the twentieth century this country advanced its technology and standard of living to the highest level the world has seen. Yet it is universally admitted that in so doing we drew heavily on the findings and accomplishments in pure science abroad. Without ready access to this foreign stockpile of scientific information, this progress would have been impossible.

Now that we are among those in the forefront of progress in basic scientific research, it is common sense and in the interest of economy to insure that loss of critical time and needless duplication do not arise through failure of ready communication. Without opportunity for exchange of views and information, delay and unnecessary duplication will inevitably occur. It is for this reason that from the very beginnings of science scientists have put a very high value on good channels of communication. The value of direct communication in speed, in dollars, and in ultimate accomplishment is great.

The bulk of international scientific communication is carried out continuously through written media. It is common knowledge, however, that there are limitations on the capacity of the written word to convey complete information that can be useful to cooperative effort or to the work of an individual which requires an intimate knowledge of the work of others. It is hard to imagine this commission or a legislative body attempting to draft legislation by correspondence, or a court reaching a just and impartial decision without having seen or heard the opposing witnesses in person. As in all human affairs, there is no substitute for informal discussion face to face.

This is exemplified in a more formal manner by the existence of a large number of international professional organizations, concerned with particular scientific fields or subjects and comprised of the leaders in these fields. These organizations periodically bring together outstanding scientists for exchange of ideas, mutual criticism, and marking out of new lines of research along the frontiers of science. Much is owed to them for continued work on such great worldwide problems as tidal waves, sea level and its variation, maintenance of international standards of measurement, long-range radio transmission, epidemic control, health and disease, sanitary engineering, meteorology, and hundreds of other matters of concern to modern civilization and to our national defense. Agencies of this government have also recognized the value to this country of direct, personal interchange of scientific information by convening special *ad hoc* conferences to focus the best minds in science on a problem of particular significance. Of at least equal importance are the contributions of individual foreign scientists to the progress of science in this country through visits to laboratories for periods of research and to universities for lectures or seminars.

Estimates of the number of scientists coming to international meetings or to laboratories and universities in the United States are difficult to make. Compared to the stream of visitors to this country for all similar purposes, including pleasure, which in the fiscal year 1951 comprised more than 300,000 persons, the number of scientific visitors (excluding students) is small, perhaps less than 3000, or 1 per cent, each year. But the scientists who do come here are important to our scientific strength out of all proportion to their number, for they consist, generally speaking, of the best scientific minds of the free world outside this country.

I should point out that the exchange of scientific information with which we are here concerned does not include classified security information. No one questions the necessity of safeguarding such information. Classified research necessarily proceeds without the full benefit of communication in this manner. From the standpoint of progress alone there is no question that this is a handicap, but one agreed to be necessary.

The difficulty with the present system of visitor control has been aptly summarized in the *Bulletin of the Atomic Scientists* (**8**, 210 [1952]) in the following terms:

> In the past few years a very large number of distinguished European scientists, almost all of them anti-communists and deeply devoted to the freedom in which scientific truth is sought and discovered, have been frustrated in their efforts to come to the U. S. to share their knowledge with their American colleagues. Their applications for visas have in many cases been refused, usually after long delay; in other cases the visas have been finally granted, but only after delays so long that scientific meetings to which they had been invited had taken place, or the teaching appointments for which they had been engaged had lapsed through their failure to arrive in time to fulfill them.

It has been estimated that under the existing statutes at least 50 per cent of all foreign scientists who apply for entry meet difficulties or serious delays. This does not imply that the number of actual refusals to foreign scientists of permission to enter is very great. The principal damage appears to occur in a small number of cases involving seemingly unjustified refusals to outstanding persons,

coupled with the tedious, cumbersome, and uncertain process experienced by those who do pass through the screen. The foundation is, of course, in no position to conclude that in any particular case the decision has been unwarranted. In some cases it is difficult to understand, from the public record, why admission has been refused. However, it is not so much the final outcome in any one case as it is the total effect of the system on our science and upon our scientific relations abroad which is harmful.

The impact of the present situation on the opinion of scientists is evidenced in editorials from leading periodicals in this country and abroad, as well as in published correspondence. A brief bibliography sampling these materials is appended to this statement for the convenience of the commission.

This and other evidence demonstrate a widespread opinion that the system operates in so cumbersome and hostile a manner that many foreign scientists would prefer not to become involved with it. To the degree that this opinion spreads and becomes confirmed, U. S. science is cut off progressively from the contributions of British and Western European scientists and those of other friendly foreign countries. These have, many times in the past, been of great value to progress in scientific fields important as the basis of our progress and security.

We must not imagine that America does not need information and inspiration, and cooperation, from outstanding scientists in friendly foreign countries. We do not have any monopoly on scientific talent or the emergence of new discoveries in science. As I have stated, we benefited perhaps more than any other world power from scientific discoveries made elsewhere. The development of some of the most vital weapons in our armament stems from open, unclassified fundamental scientific research abroad. Radar, the atomic bomb, jet aircraft, and penicillin were perfected in the United States on the basis of discoveries and research in foreign countries to which we were given ready access.

The extent to which the United States needs to draw scientific knowledge from abroad is indicated by an analysis of the nationality of scientists awarded the Nobel Prize. During the first twenty years of this award, 1901–20, forty-three awards were made in physical sciences—fifteen to Germany, twenty-six to other European nations, and only two to Americans. None of the seventeen awards in medicine and physiology went to Americans. Of the sixty awards in the physical sciences in the years 1921–49, forty-four went to European scientists, two to Asian scientists, and fourteen to Americans. Although a considerable number of American scientists have received Nobel Prizes, the fact remains that to date three out of four of these scientific awards have gone to scientists outside the United States.

I am sure that it was not the intention of the Congress, in refining and redefining the security provisions of the immigration and naturalization laws, to impede the progress of science or decrease the military security of this country by adversely affecting scientific research programs. I am just as confident that, once the special problem of science is made known, constructive changes can be expected. I also do not wish to claim that the difficulties we have experienced have been, or are likely to be, catastrophic in their effect on the progress of science in this country—although this is a possibility. My judgment is, however, that the effects of the present policy, if continued for long, can be substantial in slowing down the progress of this country on many important scientific frontiers. The implications, for international relations generally, of alienating a substantial number of the distinguished citizens of friendly foreign countries I leave to those more experienced in political affairs than I. I can say, however, that the implications for science in this country of alienating the foremost scientists and leaders in scientific thought in friendly countries are indeed serious. What has happened, thus, is sufficiently important to the research effort of this country to merit the attention of this commission and, I hope, eventually of the Congress.

What shall be done?

Our survey of the problem, although not an exhaustive one, indicates that there is room for improvement both in the law and in its administration. We are encouraged by the fact, of which we have been informally advised, that the Department of State has been actively investigating all aspects of the visitor visa problem. It seems likely that a satisfactory solution from the point of view of science will require not only improvement in administration by the State and Justice departments but also some revision of the law. For specific constructive recommendations in this field the foundation looks with confidence to the work of this commission and, ultimately, the Congress. We would like, therefore, to suggest some approaches for consideration.

First, let it be said that the foundation recognizes

that rigorous and effective security measures are required under present world conditions to preserve the integrity of our government and our country. We must be protected by adequate safeguards against admittance of undesirable or dangerous individuals on either a permanent or a temporary basis. The foundation believes at the same time that our people can understand that overemphasis on the mechanics of measures for security can seriously compromise security when it cuts us off from access to information vital to our strength. The question is frankly one of proper balance between security by isolation and security by technological achievement.

An important first step toward this end could be taken by making a distinction in the statute between requirements for temporary admission of a nonimmigrant alien and requirements for admission of an alien who intends to become a permanent resident of the United States. Complicated administrative procedures, extensive security checks, exhaustive questionnaires, and careful interrogations should be acceptable as part of an application for permanent entrance and ultimate citizenship in the U. S. The same administrative procedures and criteria are not easily understood or accepted in the case of an application for a visit of a few weeks or months. It is implicit in this suggestion, of course, that strict measures be employed for screening out foreign agents, *saboteurs,* and secret couriers.

The next suggestion is that the criterion requiring exclusion of an alien visitor might rationally become *present, sympathetic* association with a foreign subversive organization rather than, as now, affiliation, in an extremely broad sense of the word, at any time in the past with such an organization. It is encouraging that the Congress has already taken a step in this direction by providing exceptions for persons who in the past were so affiliated but who have terminated such affiliation and for five years prior to the date of application for a visa have been actively opposed to the program of the subversive organization. The change from past to present association might be coupled with a requirement that there be developed a definitive listing, similar to the attorney general's list under the Federal Employees Loyalty Program, of subversive organizations whose character as such has been publicly identified by an authoritative body or officer after due investigation. This would do much, the foundation believes, to assist administrative officers in evaluating the nature of organizations with whom foreign scientists have been associated in one manner or another during the confused and troubled years of the past two decades in Europe.

The foundation's third suggestion grows out of recognition that our government has been accumulating a wealth of experience with security programs in which a balance must be struck between security by isolation and security by technological achievement. In order to ensure that this balance be safeguarded and maintained, it is suggested that consideration be given to providing for selective audit from time to time of applications for temporary admission, by a competent, reliable, and disinterested group with appropriate experience both inside and outside of government.

There is one further possibility that should be considered, particularly if the other suggestions prove to be impracticable. It is a possibility that the foundation advances with some reluctance because it appears to set apart from other alien visitors a separate class—one having outstanding records of achievement in the professions, such as science, scholarship, and technology, and to accord to this class of persons special treatment. The suggestion seems worthy of consideration because it is among this class that the stake of this country in granting prompt admission is often demonstrably the greatest. I have in mind a separate section of the immigration law which, if established, would create a much-simplified and expeditious system for admitting such persons, perhaps defined in terms of those eligible for reciprocal exchange under the *Smith-Mundt Act*—"students, trainees, teachers, guest researchers, professors and leaders in fields of specialized knowledge or skill"—who have applied for admission to this country for a purpose directly related to the activities of a government agency, an accredited institution of higher learning, or a scheduled meeting of an accredited international professional organization.

In giving my views, I speak for the foundation, as director, but I should note that it has not been possible for me, within the limits of the time available, to obtain from the twenty-four members of the National Science Board a direct expression of their opinions. I feel confident, however, that my position is shared substantially by all members of the board. . . .

Geographical Elements in the Toponomy of Mexico

PAUL F. GRIFFIN

The author has been a lecturer in geography at Stanford University and an instructor in the same subject at Columbia University. After receiving his Ph.D. from Columbia last February he was appointed to the staff of the Department of Geography at Stanford as an assistant professor.

NEARLY 100,000 inhabited places, each with an average population of less than 300, dot the face of Mexico. Most of these bear Indian names or toponyms. Translated into English, these place names sound strange, exotic, and at times incredible to the foreign ear. Imagine living in a place the name of which signifies "I'll break your jaw!" (Chalco), "In the umbilicus" (Xico), "Where they go downhill" (Temoayán), or "Place of the squashed serpent" (Coapatongo)! In an extraordinary fashion these and thousands of other Mexican place names spell out distinctive facets of the primitive Mexican's attitude toward life, and are evocative of his cultural patterns, beliefs, devotions, his preoccupations.

For centuries his chief preoccupation—and occupation—have been survival. In stoic spiritual and physical devotion the Mexican has had to scratch and coddle the earth to sustain him. Countless tributaries of place names flow toward this central stream of thought. A basic theme directly related to survival is that of the soil and its capacity to yield fruit, or its sterility, as suggested by:

"Land where the soil is humid"	Tabasco
"Stony lands"	Tequemecán
"Sandy lands"	Jajalpa or Xalpan
"In the mud"	Zoquipa
"Where black soil abounds"	Palla

There is a logical profusion of toponymic allusions to agriculture:

"Near the ears of corn"	Miahuatlán
"Place of husked and dried maize"	Tlaollan
"Where there are spicy tomatoes"	Tomacoco
"Where there is much manure"	Cuitlapán
"On the mountain of maize ears"	Xilotepec
"Cotton temple"	Ichcatcopan
"In the land of cornfields"	Centlalpan
"Among fruit trees"	Xocotitlán
"Near the spinach"	Huitznahuac
"Place of many little red peppers"	Chiltecpintla
"Where acid fruits abound"	Xochocotla
"Smooth white pumpkin"	Tzilacaapán
"Where there are many spiny pineapples"	Huitzannaola
"Where the yucca is planted"	Yucatán

A substantial counterpart of the agricultural theme is that of water, which always has been a crucial factor in the Mexican's survival. Much of Mexico is arid, semiarid, or subhumid. As a result, the hydrographic element is conspicuous in Mexican place names. References are made to still and running waters, coloring and taste of water, means of handling it (e.g., damming it for reserve purposes), negotiating it (e.g., with canoes), praising it, noting its scarcity or its plentifulness. To illustrate:

"In the water of shrimps"	Chacalapa
"Where the water divides"	Amaxtlán
"Where houseowners have water troughs"	Apancalecán
"Where there is good water"	Cualac
"Black water"	Olac
"Place of sweat baths"	Temazcalapán
"Where fountains flow"	Axotla
"Near the blue water"	Axotlán
"At the mirrored waters"	Atexcapán
"Where waters gush forth"	Atlacholoayán
"Where there are spiders in the water"	Atocan
"Treacherous waters"	Dexcadi
"Where canoes sink"	Acalaquian
"Where the water can be drunk"	Tacubaya
"The people's water"	Altepetlac
"Where the water is imprisoned"	Altenango
"Where waters divide into many parts"	Atlamajac*

Through place names an observer can readily reconstruct the important part that hunting and

* It is probable that this toponym signifies irrigation ditches.

fishing have occupied in the Mexican scene. Typical of this category are:

"Field for bird hunters"	Totomaixtlahuacán
"Great god of fishing"	Hueipochtla
"Where they fish"	Michmaloyán
"Where they hunt with crossbow"	Atlamaloyán
"Where they hunt for birds"	Netotomaloyán
"Where they capture birds"	Totomaloya
"Place of fishermen"	Michoacán (Michhuacán)
"At fishing waters"	Michapán

A wide variety of faunal references is found among Mexican toponyms—including tigers, deer, javelina hogs, skunks, rabbits, serpents, worms, centipedes, scorpions, bees, flies, sparrows, frogs, fish, and monkeys. As a rule, the organism is simply mentioned, without a modifying adjective. Some exceptions to this are: "Place of the skinny coyote" (Coyoacán), "Place of the mashed serpent" (Coapatongo), and "Where armadillo excrement accumulates" (Ayotochcuitlatla):

"Where rabbits abound"	Tuxtla (Tochtla)
"Where monkeys begin"	Usumacinta
"Where turkeys are plentiful"	Huexolotlán
"Place of the wild pigs"	Ixcoyamec
"Place of rabbits"	Tuchtlán
"On bat hill"	Tzinacantepec
"Where snakes abound"	Coatlán
"Woodlands of wild beasts" (or "Hill of . . .")	Tehuantepec
"Where lobsters are many"	Chapola
"Where worms are plentiful"	Ocuila (Ocuilán)
"Cloud-snake place"	Mixcoac
"Where fierce beasts prey"	Tecualoyán
"The mountain of rabbits"	Tochtepec
"Where there are bats"	Tzinacantlán
"Tiger hill" (or "Ocelot hill")	Otzolotepec
"Near the pigs"	Pizotlán
"Land raised by ants" ("Anthill")	Atzcapotzcalco

The Mexican's concern with health since primitive times is reflected in many of the nation's place names. Although it is highly improbable that the Mexican formerly associated flies and mosquitoes with health conditions, they are included in the representative list below because of their direct bearing upon health:

Where there are hot healing waters"	Atotonilco
"Where there is much excrement in the waters"	Acuitlapán
"Where there are many mosquitoes" (flies?)	Moyotlan
"Wall against mosquitoes"	Moyocalco
"Place of the itch"	Cimatán and Zahuatlán
"Place of medicine preparation"	Oxitipán
"Woman's medicine"	Soapayuca
"Place full of dead people"	Mictlancuauhtla

With respect to war, Mexicans of ancient times were quite up to date: it was an almost constant affair in their world. One of the main motives for the belligerent practices of the Aztecs and other more aggresive tribes was to capture prisoners who might serve as sacrifices to the many gods who were constantly in need of appeasement. Mamatitla ("Among the prisoners") is one place name that tends to confirm what historians have recorded. Tzompanco ("Place where victims' skulls are saved") adds a macabre and curious footnote to the historian's picture. Still other place names tell the story: Quimichtepec ("Hill of the spies"); Mitepec ("Village of war arrows"); and Yautlán, which means simply "Battlefield."

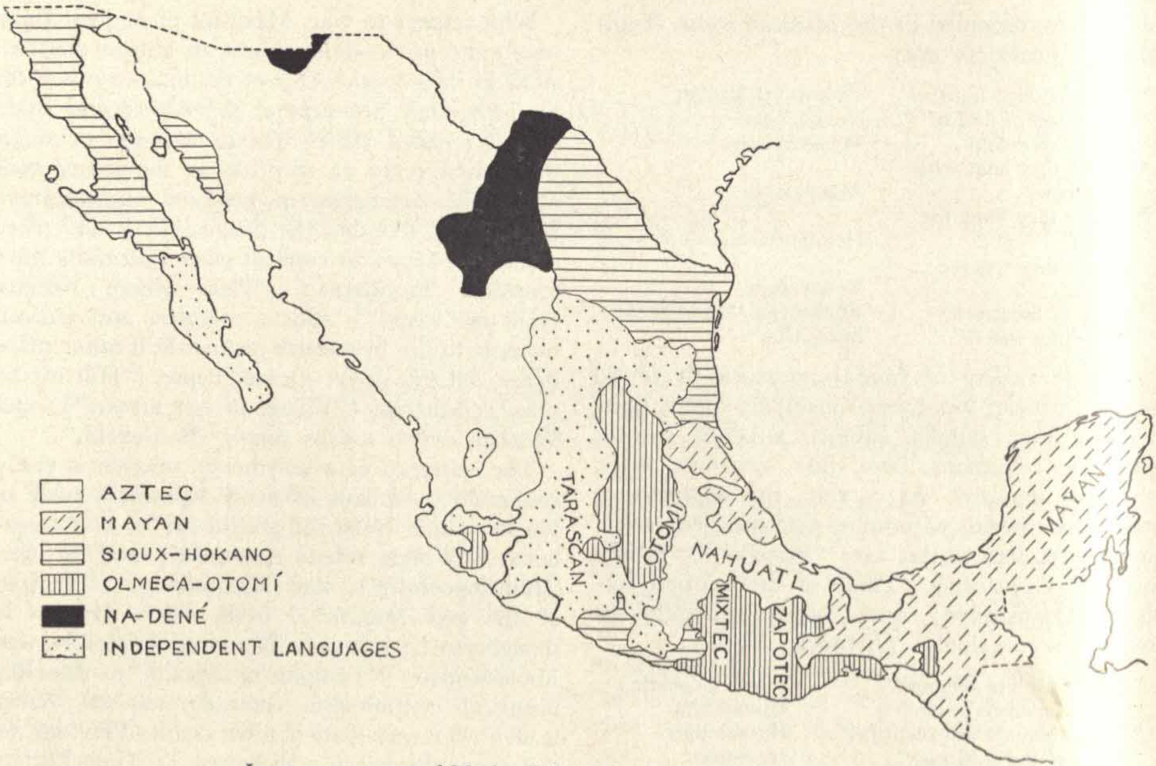
The existence of a polytheistic religion is easily confirmed by means of many toponyms, such as Huehuetlán ("Near the god of fire"), or Churubusco ("Where reigns the sinister god of war [Huitzilopochtli]"), and Mexicalcingo ("Temple of the god Mexitli"), from which *México* is thought to be derived. The word originally was Mexicatzinco ("Temple of Mexitli"). Mexitli, historical etymologists contend, was an Aztec leader who was deified after death. Previous to this time, the place was known as Tenochtitlán ("Place ruled by the god Tenoch"), according to one widely accepted etymology. A date given for the renaming of Tenochtitlán as Mexicatzinco, now called Mexico for short, is July 18, 1327.† Mexicans whimsically advise us to accept the date as correct: " 'Tis better to believe it than to try to prove it."‡

Although the ancient Mexican was greatly concerned with deities, he did not neglect terrestrial affairs. He established many inhabited places on or near mountains. A conspicuous frequency of the *-tepec* (mountain or hill) ending tells us as much: e.g., Ixtepec, Tehuantepec, Miztepec, Ozelotepec, Chapultepec, Pipioltepec, Cuicuitzcatepec, Xocotepec, Xochitepec, Moyotepec, Guaquitepec, Tecpatepec, Tezcatepec, Xaltepec, Xilotepec, Quetzaltepec, Sultepec—to mention but a few of Mexico's jaw-breaking place names in this group.

Mexico's volcanic area is amply strewn with appropriate place names. The name of the latest volcano, Parícutin, is itself a coined word, abbreviated from San Juan Parangaricutiro, the name

† Cecilio A. Robelo. *Nombres Geográficos Mexicanos del Distrito Federal: Estudio Crítico-Etimológico.* Mexico: F. Díaz de Leon Sucrs., 56–62 (1910).

‡ The expression is a proverb, typical of many picturesque Mexican sayings: *"Más vale creerlo que averiguarlo."*



Language groups of Mexico (after Mendizabal Jimenez Moreno).

of the village near which Parícutin erupted. Notable among Mexico's volcanic place names are Popocatépetl ("Mountain that smokes"), Ixtaccihuatl ("Sleeping woman," which graphically describes the mountain's contours), Poctlán ("Place of volcanic eruptions"), Yebusibi ("Mountain of fire that rains down"). Likewise, many places are named after earthquakes: Olinalán ("Place of the trembling earth"), or Olintepec ("Trembling mountain").

Mining long has been an occupation vital to the Mexican. Many localities are labeled with the names of rocks and minerals. A singular conception is embodied in the place name which signifies "gold": Cozteocuitlatl (literally, "heavy yellow excrement of the gods"). By the same token, "silver" is Teocuitlatl. *Coz,* denoting "yellow" has been omitted. Other place names alluding to rocks and minerals are:

Istapangojoya	"Place over salt"
Iztepec	"On the obsidian mountain"
Xiuhuacán	"Place of turquoise"
Tepuzcululán	"Where copper is worked"
Tequesquitenango	"Natural salt waters"
Tepoztitlán	"Among the copper"
Tecpatzinco	"Among small flints"
Tehuitzco	"Place of sharp stones"

Another environmental consideration that has generated numerous toponyms is the climate. Some self-explanatory examples are:

Yoalán	"Where night falls early"
Ecatitla	"Among the winds"
Ecatepec	"On the windy hill"
Ixtapalcalco	"In salty, damp houses"
Tlacipehualco	"Barren place"
Teciuhtlán	"Place of stone rain"
Apipilhuazco	"Where the water hangs freezing"

Contrary to the beliefs held by some tourists that "Cuernavaca" is derived from *cuerno* ("horn") and *vaca* (Spanish for "cow"), the word actually is Nahuatl, a combination of *cuahuitl* ("tree") and *nahuac* ("near"). Dominant Spanish interests during the sixteenth-century conquest of Mexico brought about a change in the original word, Cuahuitnahuac.

A curious mixture of the Spanish and Indian languages is found in some Mexican place names. For example, Minatitlán is named after a General Mina (a Spanish name), with the Nahuatl suffix *-tlán* ("place of"). Similar are Barragantitlán and Polotitlán (after General Barragán and one Señor Polo).

From the foregoing, certain generalizations about Mexican toponyms may be made. Most place names end in a suffix which designates "place of . . ."—e.g., *-co.* Ordinarily not more than three

elements are compounded into any one place name. Frequently one of these elements is a qualifying adjective, which constitutes an index valuable in the study of pre-existent attitudes of the aborigines toward given phenomena. Since the student of the civilization does not have access to the Indian himself, he must study what the Indian has left. This amounts to a content analysis of cultural products.

From a grammatical standpoint, it can be seen that the adjective precedes the noun in compound Indian place names. Some frequent prefixes and suffixes in Mexican toponomy are: *-icpac* ("on," "over"), *-itec* or *-itic* ("within" or "belly", as in Xalitic, meaning "on the sand"), *-nahuac* ("near, around", as in Anáhuac, "near the water"), *-pan* ("on," "over", as in Tlalpan, "on the earth"), *-can* ("place", as in Michoacán, "place of fishermen"), and *-tlan* ("near," "among," "under" as in Cuauhtitlán, "among the trees," or Tenochtitlán, "under the god Tenoch"). *Tla-* means abundance of the object expressed in the name, and *-yan* means place in which the verb of the toponym carries out its action.

The enormously complex language and dialect mosaic underlying the toponymic pattern of Mexico makes any accurate analysis difficult, even for those to whom indigenous dialects and languages are familiar. It has been established that Mexico has used more than 150 languages and dialects.§ Among these, Indian tongues—Nahuatl, Otomí, Mayan, Zapotec, Mixtec, and Tarascan—predominate, although other languages are used by fairly large numbers of people.‖ Nahuatl, the language of the Aztecs, is at present, and was historically, the most widely spoken. It is used principally in portions of Jalisco, Michoacán, San Luis Potosí, Hidalgo, México, Morelos, Guerrero, Veracruz, Oaxaca, and Tabasco, by more than 700,000 Mexicans. Otomí is spoken by some 450,000 persons in Michoacán, Guanajuato, Querétaro, San Luís Potosí, México, Hidalgo, Morelos, Puebla, Veracruz, and the Distrito Federal. Mayan is spoken by about 350,000 people in Yucatán, Campeche, and Quintana Roo. Zapotec is used in Oaxaca by about 250,000 persons. The Mixteca language group is used by more than 200,000 in the states of Oaxaca and Guerrero.¶ Further to complicate the picture, there has been extensive language borrowing and constant linguistic erosion. Both these processes have been accelerated by the rapid development of the modern press, radio, education, and travel, so that with each passing decade it becomes increasingly difficult to establish exact etymologies of Mexican place names. To declare with Orozco y Berra that languages in Mexico have lost their *"pureza primitiva"* is an understatement of the problem.**

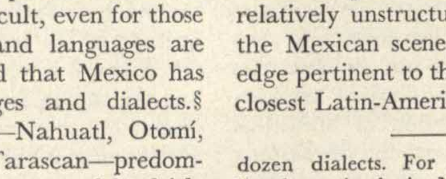
Yet with all the accompanying complexities of Mexican place name meanings, the field in its relatively unstructured stage offers the student of the Mexican scene a tremendous store of knowledge pertinent to the long and colorful story of our closest Latin-American neighbors.

§ Jesús Galindo y Villa. *Geografía de México.* Barcelona: Labor, S. A., 89–90 (1930).

‖ Manuel Orozco y Berra pioneered in studying the geography of Mexican languages; Francisco Pimentel expanded this study and emerged with a fairly comprehensive picture, listing by name 108 languages and several dozen dialects. For English-reading observers, this is listed succinctly in Matias Romero's *Geographical and Statistical Notes on Mexico.* New York: Putnam, 86–88 (1898).

¶ Jorge A. Vivó. *Geografía de México.* México: Fondo de Cultura, 134–5 (1947).

** Manuel Orozco y Berra. *Geografía de las Lenguas.* México, 13 (1886 [?]). This idea is enlarged by José N. Rovirosa in *Nombres Geográficos del Estado de Tabasco.* México: Tip. de la Secretaría de Fomento, 5–8 (1888).



Social Science and Purposive Behavior*

LOUIS O. KATTSOFF

The author is professor of philosophy at the University of North Carolina, where he has also served as chairman of the department. He is the author of Philosophy of Mathematics *(1948) and of articles ranging from methodology in psychology and sociology to problems in metaphysics and logic. He writes, too, on education and politics and is the editor of the* Southern Philosopher, *a newsletter. His "Elements of Philosophy" will be published by Ronald Press in the spring.*

IT IS not without justice that present-day scientists are still fearful of teleological concepts, for we are not so far distant from the time when the universe was purported to have been created for man and when the doctrine of "proper places" was an accepted dictum. The controversy that raged about instincts is still fresh in the minds of many, and the difficulties of scientific investigation that ensue when one establishes previously conceived ends are still present.

The mechanistic interpretation in physics brought about tremendous achievements in the physical and biological sciences, and behaviorism in psychology impelled investigations of tremendous significance. The application of statistical concepts to the social sciences and the developing application of nonteleological concepts to sociology aided in the development of the sociological sciences. But with all this an important element was being omitted in investigations, one which partially differentiates human behavior from physical events. This element was tied up with emotional valuations which it was advisable to omit from consideration for the time being. But whether we like it or not, human behavior (or even animal behavior) indicates direction. The new fields of psychoanalysis and psychiatry have made it increasingly more difficult to ignore the purposes behind behavior. In addition, problems of learning were seen to be influenced by the state of mind of the individual. Motivation has become an important consideration in many types of problems. The mechanistic interpretation required such stretching to cover so many cases that it began to look as though the mechanistic conception might be inadequate.

* This is a modification of one of the chapters in the author's forthcoming book *The Design of Human Behavior* (St. Louis: Educational Pubs. [1953]).

The development of the social sciences came as a result of the drive to gather facts concerning present conditions in society. In an attempt to avoid premature judgments concerning the meaning of the data, all valuational attitudes were severely repressed. Invaluable results were accumulated and stacked away in libraries and introductory texts. The data were used primarily for the purpose of disproving certain current theories, and no attempt was made, at least for a time, to organize and systematize the information acquired. The attitude which led to objectivity in the social sciences was so intense that it tended to overdo itself, and the source from which the data were derived gradually became overshadowed by the mountains of information about it. The application of statistical techniques induced a search for uniformities in the data, a search which was in line with the current definition of scientific law as a description of uniformities.

Up to this point what was happening was extremely valuable and desirable. But two mistakes were made. First, a careful distinction was not drawn between the emotional valuations of the scientist as a person and the valuations of the persons whose behavior was being studied. In order to avoid subjective interpretations, the social scientist disregarded the existence of objective values in the sense of values which conditioned behavior. Then when a consideration of values was forced upon him, he tried merely to describe them without noting their effect upon the subject who held them. The second error lay in the increasing failure to take into consideration the behaving individual, a failure which gradually began to lead to the hypostatization of mental constructs. The tendency was further illustrated in the introduction into social analysis of "mob minds," "social forces," "organismic theories," etc. Such theories as those de-

veloped by Spengler, Pareto, and in recent days by Sorokin, are the result of treating data in abstraction from the individuals who give rise to those data. Such an attitude makes it difficult, if not impossible, to comprehend social phenomena. For all the laudable effort put into Dodd's *Dimensions of Society,* it is difficult to recognize the results as descriptive of *social* phenomena. Dodd's attempt to quantify is very ingenious. But his variables themselves often involve variables. Until there can be a quantification of values, attempts such as Dodd makes cannot do more than break ground.†

The reasons why individuals behave differently in mobs from the way in which they behave elsewhere cannot be explained in terms of a new "mind," but rather in terms of the motivating forces within the individual. It is perfectly true that individuals act differently under different circumstances; but, again, this is not because some external forces compel them so to act, but rather because of the interplay of values and possibilities. As important as the data concerning crime, tenant farming, education, etc., are, these data acquire meaning only in terms of the values held by the individuals participating in each phenomenon. It is interesting to note that, although the social scientists often speak of the necessity of eliminating purposive considerations in the discussion, for example, of institutions, the leaders in these institutions always act in terms of desired goals.

A consideration of existing institutions and contemporary social phenomena cannot fail to indicate the tremendous importance for any developing society of the values sought by the members of that society. People are not concerned with some mysterious "telos," but with their direct needs and wants. This means that in society, as anywhere that individuals live, certain end states or end objects that the individual conceives as necessary for his (or her) continued existence in a healthful state are the ultimate determining factors in what will happen and what the individual will do. The additional fact that the goals toward which behavior is directed are subject to evaluation in terms of the health of individuals makes a true social science possible. By means of these two complementary concepts, social science can organize its data to indicate desirable directions and to implement social processes in the direction of human well-being.

Perhaps the introduction of the concept of purposive behavior will not give rise immediately, if at all, to laws according to which we can predict absolutely what individuals will do under certain circumstances. But neither can we predict absolutely what will happen when a patient takes one of the sulfa drugs. We know that there will be a reaction, and we know how it will affect the patient if the patient does not react in certain ways to the drug. The concept of purposive behavior in the social sciences will indicate general trends. To take an obvious illustration: if an individual is hungry, then he will seek food, if there are no other interfering motives or conditions. In general, an individual will seek to maintain or to attain health, provided there are no other conflicting motives or circumstances. The reason why an improvement in economic conditions may give rise to an improvement in social behavior lies in the fact that improved economic conditions make available the means for the satisfaction of the needs of individuals. On the other hand, hunger alone will not in itself lead to revolt; nor, conversely, will a sufficient amount of food guarantee a satisfied citizenry. Most people not merely want food, they want it in a certain way and accompanied by certain other values. Some people might starve to death rather than be fed under certain conditions. We know, for example, that many poverty-stricken people would rather go hungry than accept charity. But they might accept the same money or food if they were somehow made to feel that they had earned it. Again, there are individuals who would sooner surrender all dignity and all rights rather than starve to death. The difference lies in the types and the relevant potency of the values accepted by the individual. Ultimately, the significance of statistical frequencies lies not in their correlation with other frequencies of the same type, but rather in their ability to indicate the strength and quality of the values of individuals. There is, then, an element of indeterminacy in all social science. The very attempt to modify a person's values may cause a modification in other values. So, too, a prediction concerning the outcome of a given behavior pattern may cause the person involved to modify that behavior and thereby falsify the prediction.

This analysis goes counter to the complete reduction of social phenomena either to statistical tables or to the effects of forces or movements considered independent from the individuals concerned. Very little is gained if we are told that the present crisis is caused by the fact that man had made machines and had lost control of them. Expressions which indicate that the machine has conquered man or that machines have given rise to social forces beyond the control of human beings

† The literature on method in sociology and psychology is growing rapidly, and no attempt will be made to cite any of it. The reader is referred to the various sociological and psychological journals.

lead us nowhere. No machine can control a human being; the machine ultimately remains a machine. Expressions of this type are either admissions of ignorance, efforts to evade responsibility, or attempts to hoodwink people. The construction of machines and the development of technology have placed in the hands of human beings implements that make the attainment of values much easier. The same machine, if used in one way, can give individuals economic security; if used in another, it can provide profits; and if used in a third way, can give power to the user. The way in which the machine will be used will depend upon the values held by the person who controls it.

In the same way, the appeal to social forces or social energy or to a social organism can lead nowhere. This is not to deny that meaning can be given to such a term as *social force*. A social force may be viewed as an abstraction from individual behavior. The effect of an idea upon masses of people, for example, cannot be denied. But the idea has arisen in the minds of individuals because of the intersubjective system in which they find themselves, and its effect will be upon individuals.

When a craze or a fad or a style sweeps the country these things cannot be treated as social forces, independent of people. The chief reason why a fad takes hold is that it appeals to individuals. Or it may appeal, not directly, but through other motives, such as the desire to mimic celebrities or to be considered up-to-the-minute. It is difficult to stop a fad, not because the fad takes control of individuals, but because of the complex of motives in individuals which causes them to accept that particular fad. In this sense, the abstraction of "social forces" can be accepted. But in no other sense is it a valid conception. The spread of new values, the criticism of old ways of behaving, might induce people to change their values and hence their modes of behavior, but the radio, as an instrument for the propagation of ideas, released no new forces in itself. It is only those who speak through the radio and control the means of communication who may so direct the ideas of their listeners that they will tend to adopt different values and hence change their mode of behavior.

Far more important than the accumulation of statistical data on such social facts as divorce, criminality, and birth rate is the consideration of the values sought. The statistical data themselves are significant as they indicate (1) a modification, (2) a frustration, or (3) the successful attainment of values. Correlations of these rates with climate or geography or economic factors may be significant. But if they are significant it is only because these factors have an effect upon the motives of individuals. That climate does have an effect upon activity cannot be denied; but this does not mean that the climate causes the activity. A great deal of the variation in civilizations can be traced to climatic conditions, not because of any mechanistic influence of the climate upon people, but rather because individuals must adapt to the conditions under which they live. Any attempt to reduce human beings to mere pawns in the struggle of natural elements is an extreme oversimplification of the problem. The recent attempt to reduce history to geopolitics falls into the same category. Actually, geopolitics assumes the validity of the goals of a certain small group of people and, further, that they are superior to the goals of all other individuals. Having made this assumption, the geopolitician considers what resources are needed to achieve the values of that particular group. Geopolitics, as with all so-called realistic theories, begins by decrying values as idealistic or metaphysical or theological, but it ends by smuggling in a set of values accepted by small groups. These realistic theories are propounded in the interest of specific groups of individuals, and are used in an attempt to befuddle other individuals and induce them to surrender other values in the interest of the special groups.

The process of social evolution is one of development and change in the means of acquiring values as well as in the values themselves, for the achievement of values gives rise to the development of other values. More successful modes of behavior, whether discovered as a matter of trial and error or reason, tend to replace out-moded types of behavior. What actually occurs is the individual's adjustment of his values to conditions. The effect of environment upon behavior is conditioned by his achievable and approachable goals. The continual dynamic change which goes on gives rise to continual modification of the means of achieving values, as well as a continual modification of learned values. It is not possible, therefore, to lay down a set of absolute categorical dicta that will hold for any and all circumstances. But this variation cannot be used, as it has been, as an argument against the reality of values or the existence of means for judging them. Ultimately, all goals are to be judged in terms of their positive or negative potentialities. Is a particular mode of behavior conducive to the health of the individual or not? This question can be answered only in terms of the given situation. Although we cannot say that polygamy is an evil in abstraction from the situation in which polygamy occurs, this does not permit

us to conclude that polygamy cannot be said to be either good or bad. The judgment must be made in terms of the individuals concerned, as well as the conditions under which they live, but where the health of the individuals is concerned, judgments must be made.

It is evident from what we have said that moral systems must vary. The only conditions under which moral systems could be said not to vary would be conditions of absolute stasis and complete uniformity, conditions that are impossible to achieve. The health of an individual may not at all be reached by those modes of behavior which the individual believes will attain it. Whether or not an individual is healthy is only slightly related to his own opinion. An individual who insists that a given act is indispensable for his health may be completely mistaken. Only where the individual has sufficient knowledge and information concerning himself and his environment is he capable of making proper judgments. Ethical judgments depend upon knowledge and not upon mere opinion. The situation is made even more difficult by the fact that we do not have at present as much information as we would like. Hence, moral judgments are in many cases tentative. Even the physician does not always know what medicine is best for a given ailment. In the case of moral questions, where the individual's health both in its physiological and psychological aspects is to be considered, the problem is even more difficult. But the difficulty present in a great many instances does not necessarily spread to every instance.

It is evident on the basis of what we have said that ultimately all social sciences center around moral questions, questions of "ought." This does not introduce into the social sciences any factors that are not already present, but it is a specific recognition of the basic factor in all human behavior. The reason for the lack of success of various social experiments lies in the failure to recognize this. People who unite to form groups dedicated to certain values should make sure that in no important set of values do they have basic differences. This is, unfortunately, the case in all attempts to set up international agencies for peace while retaining the notions of national sovereignty and national interests. Social experimentation must recognize the existence of values and the concomitant fact of purposive behavior. It is quite possible to study environment or to study psychological processes in abstraction, but this abstraction must be recognized and repaired if inferences are to be drawn from these studies. The social situation is an intersubjective field, consisting in a unity of environment, behaving person, and end state. We have called the social situation an "intersubjective system" to emphasize the fact that there is interaction, not a superentity.

Social therapy will direct itself against one of these three aspects, always keeping in view the values involved and their relationship to the health of the individuals. To improve slum areas will not in itself lead to an improvement in the lives of slum inhabitants. As long as the values which the slum dwellers hold are slum values, no amount of improvement of conditions will be sufficient. The effect which has been found—that is, that with slum clearance there has come a decrease in criminal behavior and an increase in the quality of the actions of slum dwellers—is the result of the fact that improved living conditions make it easier to achieve basic needs, and hence to develop new values. The improvement in the lives of people as a result of slum clearance, housing projects, etc., is an indication of the ease with which values can be modified to fit the environment as well as evidence that the slum values are not innate but learned. The psychological effect of a new suit of clothes is frequently far beyond the value of the clothes. What happens, in fact, is that the new suit of clothes changes the attitudes of others toward the wearer, who then begins to think in terms of a different set of values. Environment has a definite effect upon the behavior patterns of individuals, but this effect is the result of the relationship that exists between environment and the achievement of many values. Where the environment is such that only a few values can be achieved, and all the energy of the individual must be directed toward maintaining himself, very little social progress is possible.

In the same way social therapy will be directed toward behaving persons. Where the individual can no longer achieve his desires, and for one reason or another does not change his behavior patterns, a re-education of the individual must be undertaken. Here, again, the important element is to educate the individual to act in those ways that will enable him to achieve the greatest amount of values conducive to the greatest amount of health for himself. Where the individual cannot be educated into a new mode of behavior, then he may be placed in an institution where he will be prevented from injuring himself and others. In every case we are concerned primarily with things that ought to be sought and things that ought to be done to achieve a state of health. The existence of social problems means the existence of things sought for and things done that are not conducive

to the health of the individuals. The concern which sociologists show for social problems is based upon the destructive features of many aspects of the social situation. It is desirable, therefore, that the social sciences recognize these facts and define both the health of individuals and the conditions conducive to that health. In those cases where social conditions are detrimental, it behooves the scientist to describe the conditions and to propose remedies for them. Social scientists are the physicians of the social system. That they are not accepted as such is due as much to their own mistaken values as to the refusal of people to value them.

Such a point of view will make social experimentation possible. If values are the basic features of social behavior, then clearly the manipulation of these values will be the essential feature in the study of society. The difficulty with any present attempt to experiment on a social basis lies in the difficulty in controlling the innumerable factors that are constantly affecting each other in the social situation, plus the inability to determine what constitute the important factors in a given situation. Statistical correlations indicate interrelationships, but not causation. Recent developments in statistical analysis, known as factor analysis, enable the student of social phenomena to factor out the basic elements in a given set of data, to sift out those elements common to the various sets of data. Obviously, what is not in the data could not be derived from the data. Hence, the elements which in the individual give rise to the phenomena that are measured may frequently be hidden from the view of the investigator. In any case, the obvious fact to which we have referred frequently, that a change of environmental factors does not in itself necessarily bring about new situations, is the keynote of the difficulty of social experimentation. What gives rise to behavior is a value, and if we wish to change behavior and hence social phenomena, we must therefore change the values maintained by the individual. Social experimentation can consist, therefore, in setting up situations in which individuals are given the opportunity to obtain the objects of their behavior and others in which their achievement is frustrated. Or, keeping certain values as constant as possible, environmental factors may be modified to a greater or less degree and their effect upon the values and hence the behavior of the individual noted. A given social situation can be modified and the effect upon the health of individuals noted. Control becomes possible because the control is directed toward the means of achieving values as well as the values themselves. Whether or not a given social policy is desirable, therefore, can be determined by viewing its effect upon the health of the individuals. A great deal of such experimentation does seem to be developing gradually. The experiments on neuroses in animals, and the Iowa experiment on children to determine the effects of group organization, are indicative of the interest in these problems.

We must never swerve from the basic questions that need to be decided in the case of social policy: not a monetary cost or consistency or inconsistency with a given set of institutions or a tradition, but the effect upon the health of individuals. Certain laws are immoral not because lawyers say so or because the Supreme Court decides so, but only because they lead to unhealthy results in individuals. Laws that give rise to inequalities or to injustices, or set up situations in which individuals are compelled to violate them, are not conducive to the health of the individual. Moral standards no matter how ancient, traditions, folkways, institutions, no matter how venerable, are not acceptable if they no longer effect the purposes toward which they were originally aimed, and have not acquired new values. Unless it can be shown that greater ill health would result from abolishing them than from maintaining them, they should be abolished. Social science cannot divorce itself from its applications any more than any other science can. The findings of the social scientists, therefore, as Lynd has pointed out in his excellent book, *Knowledge for What?* are to be tested in their applications to social systems. The laboratory within which the social findings are tested is the social system itself. Once the social scientist recognizes this fact and courageously makes his decisions in the light of the best knowledge available to him, social science will become probably the greatest tool for the improvement of civilization that the world has ever found. But this will necessitate the clear-cut recognition of the existence of values and of the fact that it is the business of the social scientists do determine what is a positive value, as well as the best means of achieving that value within the existing limitations of the intersubjective system.

Some Aspects of Urban Zoology in Great Britain

COLIN MATHESON

The author, head of the Department of Zoology, National Museum of Wales, Cardiff, is a graduate of the University of Aberdeen. He is a member of the Nature Conservation Committee of the National Trust for Places of Historic Interest or Natural Beauty and since 1952 of the newly constituted Welsh Committee of the Nature Conservancy. He is past president of the Cardiff Naturalists' Society.

IN HIS paper "Biology and Urban Areal Research" in THE SCIENTIFIC MONTHLY (July 1951), Francis C. Evans, of the University of Michigan, comments that "the rise of the city has been possible only through man's domination of his surroundings," and adds, "Behind the disharmonies of the modern urban community there seems to be a lack of effective regulation." The study of the modern city, and of its inhabitants human and animal, as an ecosystem, still awaits comprehensive treatment. It may, however, be of interest to submit the following brief synthesis of investigations made at different times on some of the zoological "disharmonies," and on measures for their effective regulation that are being or might be undertaken by urban man in his efforts toward still more complete domination of his surroundings.

Many of these disharmonies, both physical and mental, are associated with the prevalence of various animal pests, the multiplication of which has been favored by obsolete buildings and building methods. Among the most serious insect pests of present-day cities is the bedbug, stated in 1937 to be the source of greater or less discomfort to over four million inhabitants of Greater London; and it is in the larger cities that it occurs in greatest concentration. No definite figures were available until, from 1936 onward, the Medical Officers of Health of a number of British towns, in accordance with a recommendation from the Ministry of Health, published in their annual reports particulars of the number of houses found to be infested with *Cimex lectularius,* and of those disinfested. From these and other figures, it was possible to prepare tables[1] giving the number of houses known to be infested in each town as a percentage of the total number of inhabited houses. In twenty-three towns for which figures were available, it was found that the known degree of infestation increased, in the great majority of cases, along with the order of magnitude of the town. Thus, of the six smallest towns, with under 25,000 houses each, five had a percentage of infested houses ranging only from 0.12 to 0.30; of the six largest towns, with over 100,000 houses each, five showed percentages ranging from 0.79 up to 2.2; of the towns of intermediate size, the majority lay in the classes of intermediate infestation (between 0.31 and 0.70 per cent). But bug infestation, not being a "notifiable disease," was no doubt commoner than indicated by these figures, which are of interest as suggesting the *relative* conditions in large and small towns, and not as criteria of the absolute degree of infestation or as a basis for comparison between one town and another.

It may be mentioned, however, that the town showing the highest degree of known infestation (2.2 per cent) was characterized by its many rows of back-to-back houses; that is, each house was structurally united to and continuous with three others, one on either side and one behind. Such houses still constituted at the time almost half the total houses in the town, although this proportion was decreasing as a result of large-scale civic replanning. Indeed, the part which improved building design can play in reducing this pest is so important that the Report of the Committee on Bedbug Infestation, issued in 1942 under the auspices of the Medical Research Council, included a detailed section on "Building Design in Relation to Bedbug Infestation." The recommendations deal with the reducing of harborage to a minimum by appropriate attention to foundations, floors and skirtings, walls, roofs, picture rails, etc; and also

with facilitating disinfestation by planning buildings in simple, rectilinear, accessible compartments. Application of the methods recommended will, it is hoped, save future urban dwellers from the experience of housewives living, as recorded by Margery Spring Rice, in houses where "The bugs . . . in the rotting woodwork cause endless extra work in an endeavour to be clean. It has been necessary to sit up at night to keep the bugs off the small baby."

Measures for the control of the chief mammalian pests of urban man, the house rats and mice, may also contribute incidentally to the reduction of the bedbug, which, in the absence of human hosts, may derive sustenance from them. Cases are recorded of bugs surviving in houses that had been empty for over two years, and it is known that they can feed on the blood of rats and mice; though how far they do so in nature is not yet established. In the laboratory they can and do produce a normal number of fertile eggs on such a diet. But, apart from this possibility, the rodent population in itself is well known to constitute, at the least a source of annoyance and anxiety, and at the worst a grave danger to health, for city dwellers. Much research on the numbers and habits of the brown rat (*Rattus norvegicus*), the predominant rat in our towns today, has been published both in Great Britain and in America, and need not be enlarged upon here. The writer may simply mention that in the course of routine examinations of rats undertaken over a period of years for the Cardiff Public Health Department, he inspected in one year 1108 brown rats from a single city refuse dump (now filled in and converted into a sports stadium).

The routine investigations mentioned were, however, mainly concerned with the examination of black rats (*Rattus rattus*)—the rat chiefly found on seagoing ships—for fleas and other ectoparasites. The plague flea, *Xenopsylla cheopis,* the potential carrier of bubonic plague, was found during the late 1920s to constitute 78 per cent of all fleas found on rats from seagoing ships, the remaining 22 per cent being almost entirely *Nosopsyllus fasciatus,* the species associated with the brown rat. Away from the docks, in the city area where most of the rats are of the brown species, plague fleas constituted only 3.5 per cent of the total. The situation in shore premises at the docks was intermediate; though black rats were more numerous than brown, the plague flea numbered only 21 per cent of the total fleas.[2]

It was later found, however, that among rats from shore premises at the docks the percentage of plague fleas decreased considerably from about 1930 onwards—from 21 per cent to 5.5 per cent.[3] Statistical analysis of corresponding data (which unfortunately are not available) from other ports would be necessary before one could base any conclusion on this; but it may be noted that this decrease began soon after the coming into force in 1930 of the Public Health (Deratisation of Ships) Regulations in 1929. Since then it has been obligatory on vessels coming from a foreign port to produce a deratization (or exemption) certificate issued at an approved port within the previous six months.

Percentage of Houses known to be infested	Size of Town			
	Under 25,000 Houses	25,000–50,000 Houses	50,000–100,000 Houses	Over 100,000 Houses
Over 1.10				●
0.91–1.10	●			● ● ●
0.71–0.90				●
0.51–0.70		● ●	● ● ●	
0.31–0.50		●	●	●
0.11–0.30	● ● ● ● ●	● ●	● ●	

Diagram showing how the proportion of houses known to be infested with *Cimex lectularius* in twenty-three towns in England and Wales generally increased with the order of magnitude of the town. (Based on Matheson, C. *Bull. Entomol. Research,* 32 [1941]).

These investigations suggested an inquiry into the position of the black rat at British seaports generally; and this was undertaken, with the cooperation of the medical officers of health concerned, just before the outbreak of war in 1939. The results[4] indicated that the average number of rats (almost always black rats) per ship fumigated, and the percentage of shipping requiring fumigation, had declined considerably; that, despite this and precautions taken to prevent passage of rats between ship and shore, several ports still showed an undiminished black rat population on shore—at the docks and sometimes in other places as well; and that in ports which did show a decrease on shore, comparison with the figures for brown rats usually indicated that the decrease applied to both, and was not necessarily related to ship-deratization measures. The black rat seems capable of maintaining itself in some numbers at British seaports without receiving any large recruitments from ships. Commenting on this, the chief medical officer to the Ministry of Health stated in his *Annual Report on the Health of the Nation* that the survey indicated the necessity for

measures against the species not only at docks but at a distance therefrom. The same is true of some seaports in the United States; of the rats taken by the trappers in San Francisco during 1936 and 1937, almost one quarter were black rats, nearly all from the area rebuilt after the great fire of 1906.

This last point emphasizes that special attention to the construction of new buildings is among the most important measures to be taken against the black rat in modern cities, since it appears that the efforts of this adept climber to recolonize seaports have been aided by such developments as the extension of the telephone system and the removal of kitchens from basements to roofs. The sealing-off of floors, ceilings, etc., from upper floors and roofs, and the provision of really effective rat-guards where there is any connection between one roof and another, are among the measures recommended. The brown rat, on the other hand, is of course a burrower, and it is instructive to notice the detailed analysis made at Manchester of the causes of rat infestation. During the three years 1934–36, 5791 premises were found to be infested, and in 67 per cent of these the infestation was due to, or associated with, defective or disused drains or sewers. Similarly at Glasgow, "rats were found to be gaining access . . . through apertures in the walls under the ground where service pipes such as gas, water, drains, etc., entered the buildings. . . . More care should be taken to seal all openings in walls."

The part played by domestic cats and dogs in the destruction of house rats and mice is difficult to assess* and is probably overestimated; though it may be suggestive that of a representative sample (about 3000) of Cardiff households from which information was obtained the percentage which kept a cat ranged from only 30 per cent in recently built housing up to 75 per cent in older dilapidated districts, where rodent pests are likely to find suitable harborage. Some breeds of dogs, such as terriers, may also contribute to rodent control.

On the other hand, both species are the hosts of a number of animal parasites, some of which are transferable to man, and the dog as a factor in street accidents receives considerable publicity in the press of Great Britain, where the animals are often allowed to wander at will. Actual figures supplied by ten towns of varying size in Britain indicate that dogs are concerned, as victims or in some other way, in rather under 200 out of every 1000 street accidents in these towns, and that, of these, four or five may result in injuries to human beings. Records of people bitten by dogs in these towns indicate (even allowing for unreported cases) that in proportion to population this is a negligible trouble. In any case the eradication of rabies in this country by stringent quarantine regulations has banished the danger once so commonly associated with dog bites, although the danger is still appreciable in towns in the United States and elsewhere. (These and other aspects of the influence of the dog in urban communities are discussed in a paper awaiting publication.[5])

The keeping of domestic animals, however, should probably be associated less with the health aspects of urban zoology than with the less easily assessable psychological needs that are due to the almost complete severance from nature of many modern city dwellers. One need not be a psychologist to appreciate the significance of the contrast between the early medieval town, which Mumford in *The Culture of Cities* describes as "adequate . . . on the biological side," with its cows and horses, its houses where "one awoke . . . to the crowing of the cock, the chirping of the birds nesting under the eaves," and the twentieth-century towns where, for example, "most of the inhabitants of modern Scotland . . . grow up out of touch with nature, the rhythm of the seasons, and the relationship of man to the soil. Their mental conditioning by a man-made environment of concrete and steel, stone and brick, must profoundly affect their outlook."

Robert Sinclair, in his *Metropolitan Man,* mentions that, of a class of London senior schoolgirls before the war, five had never seen a live cow. Even the horse, familiar for much longer on city streets and quaysides, has in many towns practically disappeared.

> Among all the sights of the docks [wrote Herman Melville† of his visit to Liverpool a century ago], the noble truck-horses are not the least striking to a stranger. They are large and powerful brutes with such sleek and glossy coats, that they look as if brushed and put on by a valet every morning. They march with a slow and stately step. . . . The truckmen themselves are almost as singular a race as their animals. Like the Judiciary in England, they wear gowns—not of the same cut and colour though—which reach below their knees, and . . . hobnailed brogans. . . . They are a reserved, sober-sided set . . . spending so much of their lives in the high-bred company of their horses. . . .

Even in 1921 there were in Liverpool 2078 stables containing 9940 horses; by 1934 there were only 1066 stables with 4168 horses.

* See, however, Jackson, W. B. *J. Mammal.,* 32, 458 (1951).

† In his partly autobiographical novel *Redburn.*

The majority of carters [observes the *Merseyside Social Survey* carried out by the University of Liverpool] are engaged about the docks, carrying goods from the quayside to the railway or warehouse. . . . Today, not only are goods generally loaded into or unloaded from railway wagons at the quayside, but motor transport has in many cases replaced horses . . . as might be expected in a declining trade, there is little recruitment of new entrants. . . . With the decline in importance of the trade, there seems to have gone a certain decline in the professional pride of the carter. A generation ago, we were told, the carter had his own uniform of white moleskin trousers, which were always clean on Monday morning, blue coat, blue socks, and heavy boots, with a special kind of apron. Today these have been replaced by corduroys and even by fancy socks and shoes.

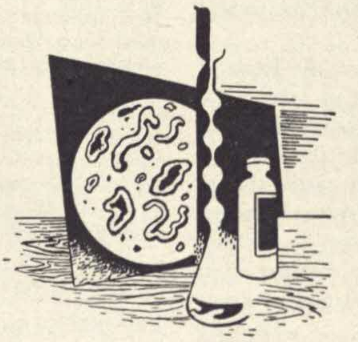
For many townspeople today, therefore, the domestic cat or dog may represent their only contact with the world of four-footed creatures familiar to their ancestors; and further reduction of such contact by restrictions on keeping cats and dogs, which various corporations (no doubt rightly) have judged necessary both in old tenements and in new blocks of flats, has aroused protests from tenants in many towns. Where the separate-house-with-garden principle is practicable, the wishes of such tenants may be reconciled in greater degree with the legitimate requirements of their neighbors. No statistics on the numbers of urban cats are readily available; but an investigation[6] made in two of the largest towns in Wales—Cardiff and Newport—indicated that house-kept cats probably numbered at a minimum about 10.5 per cent of the human population. In addition, the numbers of stray cats destroyed annually in large towns, by the Royal Society for the Prevention of Cruelty to Animals and similar organizations, may range from 1 to 3 per cent of the human inhabitants: Edinburgh, 1 per cent; Glasgow, 2 per cent; Liverpool and Cardiff, 3 per cent; and Boston, 3 per cent. The minimum number of dogs, as estimated from the licenses issued in each of ten towns in Great Britain, approximates fairly closely in most cases to 5 per cent of the human population.

As a counteractive to the "man-made environment of concrete and steel, stone and brick" may be mentioned the emphasis laid on additional parks and open spaces in the new town-planning schemes. "Parks are, in fact," say Hesse, Allee, and Schmidt in their *Ecological Animal Geography,* "the type of situation richest in bird life in the temperate latitudes." In the satellite garden city of Wythenshawe, for example, with 1000 acres reserved for a permanent agricultural belt and another 1000 for open spaces out of a total of 5500 acres, and with its wide pathways bordered by trees and flowers, bird life will find many more favorable haunts than in the parent city of Manchester.

It need hardly be stressed that the data available for some of the foregoing inquiries were of a provisional and incomplete nature. They provide, however, within their limits, a preliminary picture of the ramifying interrelations of urban man and urban fauna. Such a picture should prove useful in efforts to rectify, from the biological aspect, the lack of effective regulation that lies behind some of the disharmonies of modern urban communities.[7] An exhibit embodying much of the foregoing information has, for some years past, attracted considerable public attention in the Department of Zoology in the National Museum of Wales.

References

1. Matheson, C. *Bull. Entomol. Research,* 32, 165 (1941).
2. ———. *Ann. Rept. 1934, Cardiff Medical Off. Health,* 121 (1935).
3. ———. *J. Royal Sanit. Inst.,* 62, 128 (1942).
4. ———. *J. Animal Ecol.,* 8, 76 (1939).
5. ———. *J. Royal Sanit. Inst.* (in press).
6. ———. *J. Animal Ecol.,* 13, 130 (1944).
7. ———. *J. Royal Inst. British Architects,* 3rd. ser., 50, 279 (1943).



SCIENCE ON THE MARCH

A BRONZE STATUE FROM MAREB, YEMEN

FIG. 1. On February 12, 1952, owing to the collapse of local security, the American Foundation for the Study of Man's archaeological expedition to Mâreb, Yemen, capital city of the Queen of Sheba, ceased excavating the above monumental eighth-century B.C. temple of 'Ilumquh (dedicated to the moon-god, premier deity of ancient Sheba). Under the leadership of Wendell Phillips, the expedition members escaped across the desert to eventual safety in the Wadi Beihan, Western Aden Protectorate, leaving behind in Yemen all expedition equipment, supplies, and recently discovered Sabaean bronze and alabaster sculpture.

THE short but exciting Yemen expedition of the American Foundation for the Study of Man devoted itself at Mâreb to excavating the great Himyaritic temple known as Maḥram Bilqîs (Fig. 1). A large, oval structure, it was built in the eighth century B.C., was named 'Awwâm, and was dedicated to the moon-god 'Ilumquh, as may be learned from an inscription on the outside of the temple. Circumstances permitted complete excavation only of the entrance hall, which consisted of a court surrounded by a peristyle. Among other things uncovered were three bronze statues and the foot of a fourth, all found 35–100 cm above the floor of the forecourt of the temple.

It was the custom of ancient devotees of the moon-god to offer to their deity one or more statues set on a stone base containing a dedicatory inscription. Variant forms were: (1) the inscription cast on a metal (bronze, so far as is known) plaque fastened to a plain stone base; (2) the in-

scription on the front of the statue itself with the base plain; (3) the inscription on a metal (bronze) plaque, occasionally on stone, without statuary.

The largest of the three statues, which was found lying on its left side at the base of a pillar, measures 93 cm. It represents a man who, with head erect, is walking rather stiffly, with a slow and determined step (Fig. 2). Both fists are thrust out before him, with the upper arms nearly vertical and the elbows bent; in his right hand he probably once held a staff or scepter, which is now missing, leaving only a hole through the fist where it had been inserted; in the left he still holds between the second and third fingers his official seal, the design on which now appears only as an oval boss. Statues as large as this are rare, although, judging from footprints on other stone bases, they were occasionally more than life size.

In the statue found at Mâreb the man's skirt, a rectangular piece of cloth, is wrapped snugly around his thighs, with the right end overlapping the left. The open end of the skirt, being pulled taut about the waist, creates an interesting curved line from above the left hip to the center front. The difference in circumference in the top and bottom of the skirt causes the left end to hang slightly lower, thus breaking the otherwise straight line at the bottom. A broad belt hides the top edge of the skirt. It is of even width, without knot or buckle, except for two narrow ends hanging down the back, which are probably intended to represent loose ends or tassels. Stuck into the belt, a little to the right and pointing toward the center, is a sheathed dagger, or *jambîya*. The scabbard is straight, with a slightly bulging round end. The handle of the dagger is thick at hilt and butt, and studded with four large rivet heads. Such *jambîyas* are still carried in South Arabia, but the scabbards have turned-up points.

Over the man's back a lion skin is draped—its forepaws crossed in front across his chest, its hind paws gripping his thighs. No clasps for holding the paws in place are indicated. The lion's head hugs the back of the man's neck, and its tail hangs to the back edge of his skirt. The head of the lion is small and almost unrecognizable, and the paws look like human hands. Apparently the lion skin was extremely thin, for the shape of the man's shoulder blades is distinctly visible through it.

On his head is a thick, knitted cap—indicated by three rows of knobs—encircled by a double band of ribbons, which are tied on the left side just above the ear. On the right side the ribbons project abruptly, leaving a small hole for the insertion of some ornament, such as a feather. Above the ribbons the rows of knobs, converging to a point at the crown of the head, become narrow and indistinct.

The torso is modeled in broad, flat planes. Two small folds of flesh at the base of the neck probably represent the collar bones. The legs are straight, with a sharp ridge running down the front, and a curved incised line, from the ankles to the knees, outlining the muscles of the calves. Another sharp ridge runs up the front of the neck, which is slightly conical, tapering upward to the broad, stiff head. The eyes are large and conventionalized—the irises being indicated by incised circles in the center of the eyeballs. The eyebrow, a sharp ridge above the eye socket, follows the curve of the upper eyelid. The nose is large, arched in profile, and the nostrils are deeply indented. The mouth is a narrow slit, and a lightly incised line runs down each side of it from the nose to the chin. A beard of tight curls, indicated by rows of knobs, extends from the temples to below the chin. The ears are large and simply modeled, with large bulbous lobes.

This is a statue of a man whose name, Ma'adkarib, indicated on the anterior part of the shoulders, is well known in Qatabanian and in Sabaean. The front of the statue is covered with a lightly engraved inscription running from the shoulders to the bottom of the skirt, and continued on the right knee:

'mdhr/bn/lḥ	'Ammdahar, son of Laḥa-
y'ṯt/bn/kr(?)	y'aṯat, of [the family of] Kar(?)a-
t/hqny/'l	t, has dedicated to 'Il-
mq]h/ṣlmn/dhb	umq]uh, this statue in bron-
n/. . . .	ze . . . .

The first name, 'Ammdahar, is specifically Qatabanian, as shown by the name 'Amm, "moon-god," principal divinity of the kingdom of Timna'. An example of this name was found in Mukêrâs. The father's name, Lahay'aṯat, is also found frequently in Qatabanian. At the beginning of line 4 the letters "mq" of the name of 'Ilumquh, moon-god, principal deity of Saba', can be restored. There is no clue to the relationship between 'Ammdahar, the donor, and Ma'adkarib, the person represented by the statue.

A longer inscription appears on the chest of the second statue from this same group:

yṯ''mr/wmyṯ'/ntlynhn/	Yaṭa''amar and Muyaṭa' Ñatlaynahân,
bny/šr'hmw/hqny/'lm	of [the family of] Šara'humu, has dedicated to 'Ilum-
qh/b'l/'wm/mṯlnhn/	quh, lord of 'Awwâm, these two statues

ḏhbn/wbnyhmw/zy	in bronze and their two children Zay-
d'l/wkrb'ṯt/bḏt hwfyhmy/'lmqh	d'il and Karib'aṯat, because 'Ilumquh has accorded them [: the two children]
ḏt/tnb'hw/	that which he [:'Ilumquh] promised him [: Yata'-'amar]
wyhwfynh mw/b'ṯtr/ w'lmqh	and whom he may protect. By 'Aṯtar and 'Ilumquh.

'Awwâm (actually Maḥram Bilqîs) is the temple of 'Ilumquh, as the text indicates, but also of "Bull Lord," another name for the same divinity. The final invocation cites 'Aṯtar, stellar god habitually put first in the invocation, and 'Ilumquh, the principal lunar god of Saba'.

The statues were probably intended as portraits: the first that of Ma'adkarib, whose name is inscribed on the back, the second and third those of the two sons of Yaṯa''amar, who, according to the second inscription were consecrated to the divinity as well as being represented by the two statues. The reasons for the dedications, as stated in the second inscription, were: (1) gratitude for a special favor, and (2) petition for protection in the future.

Stylistically, the first statue is reminiscent of Phoenician sculpture and has its closest parallels in Phoenician and Cypriote statuary from the eighth to the sixth centuries B.C. South Arabia was connected with Phoenicia by trade relations beginning not later than the tenth century B.C.

FRANK P. ALBRIGHT
ALBERT JAMME
American Foundation for the Study of Man
New York

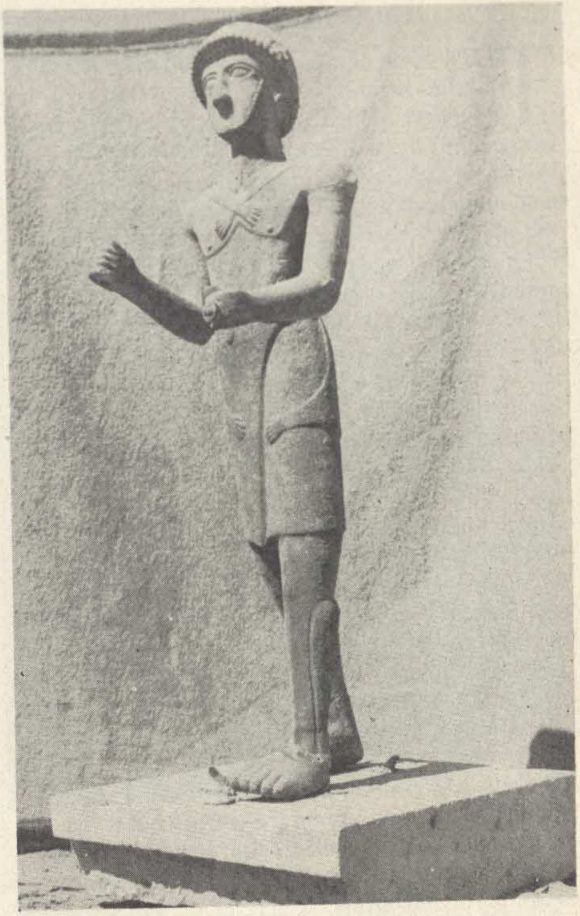


FIG. 2. Front view of bronze statue of Ma'adkarib. In spare moments the explorers restored the broken right foot and left arm with iron rods and cement and mounted the statue on a stone base. Ma'adkarib thus stands majestically surveying the modern Mâreb much as he did the ancient city centuries ago.

GRASS HERBICIDES IN TODAY'S AGRICULTURAL ECONOMY*

THE idea of grass as a weed is, justifiably, quite far removed from the mind of the average person. The mental image evoked by the word "grass" varies with the individual, but in most cases it is a pleasant picture. The city man visualizes fairways, parks, and home lawns, the stockman sees endless miles of Western rangeland, and the agriculturist pictures fields of small grains. Man has been increasingly dependent on grasses for his food ever since the first nomads began to give up their precarious hunting life for the surer returns of tillage. As a result, this close association has fostered most of the world's greatest civilizations. The rice culture of Eastern Asia, the wheat and barley culture of Europe and the Near East, and the maize culture of pre-Hispanic America all owe their development to this unique and generally beneficial plant family.

As in many families of the plant or animal kingdom, however, there are certain undesirable species present in the family Gramineae that interfere with man's productive activities. The presence of such species may cause reduced yields of desirable crops, render cultivation difficult or even impossible, and cause injury to grazing animals on either green or dry feed. The Biblical reference to the useless tares among the wheat (Matt. 13:24–30) would seem to indicate that grassy weeds have been plaguing agriculturalists for many centuries. This particular grass, a species of *Lolium* commonly called Darnell, still infests fields of the Mediterranean lands as it did well over two millennia ago. Today's grass weed problems, both in the United States and abroad, rank in importance with those involving broad-leaved weeds, and are frequently more baffling. Efforts of federal agencies, state universities, agricultural chemical companies, and many individual farm operators have been pooled in attempts to lessen the effects of weed grasses on current production.

Areas of Economic Importance

In sheer acreage, India can probably lay dubious claim to the world's worst grass infestation. It has been reported recently[1] that Kans grass (*Saccharum spontaneum*)† has invaded 10 million acres of needed agricultural land. An aggressive relative of our domestic sugar cane, Kans poses a grave problem in a land that is both seriously overpopulated and underfed. In the sugar-cane fields of our own Gulf states, an acre of land infested with Johnson grass (*Sorghum halapense*) may contain as high as 9 tons of rhizomes, or underground stems.[2] Other serious creeping perennial pests are quack grass (*Agropyron repens*), Bermuda grass (*Cynodon dactylon*), and Reed canary grass (*Phalaris arundinacea*). These species are tough and persistent and present a constantly recurring problem, inasmuch as they propagate readily from rhizomes or stolons. In the Southwestern United States a water-loving perennial called Carrizo cane (*Phragmites communis*) blocks irrigation systems, and its control represents a substantial cost in yearly maintenance of the canals.[3]

The scope of grass control is not limited to perennial grasses alone. These species are confined mostly to humid regions, or to those irrigated arid lands where winters are mild. Annual grasses, on the other hand, are present in all climates where agriculture is possible, and frequently become a more serious problem than perennials. Crab grass (*Digitaria sanguinalis*), a universal lawn pest, reaches its optimum in the Gulf region, where it is generally recognized as the Number One weed in cotton culture.[4] Its success accrues from a prolific seed production, coupled with a dormancy factor which insures a ready supply of seed in the soil for several years ahead. In the sugar-beet belt along the eastern side of the Rocky Mountains the dominant weed is wild oats (*Avena fatua*), and much research has been expended in seeking an economical control.[5] Each cultivation, in turning over a furrow slice of infested soil, creates the aerobic conditions necessary for seed germination. On several million acres of rangeland in the Great Basin states and adjacent areas cheat grass (*Bromus tectorum*) has invaded depleted and worn-out perennial grass and sagebrush mixtures. This prolific and aggressive species ripens early, is undesirable as forage, and constitutes a tremendous fire hazard when ripe. In addition, the sharp awns cause mechanical injury to the mouth parts of grazing animals, thus leading to secondary effects such as actinomycosis, or "lumpy jaw." Other universally distributed weedy annuals include foxtail millet (*Setaria viridis*), barnyard grass (*Echinochloa crus-galli*), witch grass (*Panicum capillare*), and sandbur (*Cenchrus pauciflorus*). A short-lived perennial closely resembling an annual in its habits

* This paper published with the approval of the director, Colorado Agricultural Experiment Station, as Scientific Journal Series No. 406.

† This and subsequent scientific names found herein are those given in Hitchcock and Chase's *Manual of Grasses of the United States*, 2nd ed. Washington, D. C.: GPO (1950).

is foxtail barley (*Hordeum jubatum*), a pasture and hayland pest found ordinarily under conditions favoring a high water table. These small-seeded pests create a yearly problem of control in many crops and add measurably to the cost burden borne by the farm operator. In addition to the direct costs of cultivation, other costs, such as the expense of seed cleaning, are frequently directly related to the presence of weedy grasses on agricultural lands.

Methods of Grass Control

Until very recent years, the chief method of grass control was the traditional one of "hoe and pull." The advent of cultivation machinery materially speeded up this chore, but in-the-row weeding still remained largely a laborious hand procedure requiring cheap and abundant human labor. Fields have often been abandoned as hopeless, and farmers forced to relinquish the investment of a lifetime, because of invasion by quack grass and other noxious grasses. In the late 1920s and early 1930s the chemical industry began to explore the possibilities of herbicidal compounds, and several materials were duly evolved which successfully destroyed vegetation. The borates, chlorates, and arsenicals now widely used as soil sterilants were developed during this period. None of these, however, filled the basic need for a herbicide which would kill grasses, yet leave the soil in suitable condition for cropping. Their usefulness therefore became limited to the eradication of spot infestations or to soil sterilization along fence rows, ditchbanks, and in farmyards or driveways. Other than this, the only practical control approach prior to World War II was frequent cultivation.

Unlike the broad-leaved weed species, grasses have a natural defense mechanism against most foliage-type herbicides such as 2,4-dichlorophenoxyacetic acid (2,4-D) and dinitro-*o*-secondary butyl phenol (DNOSBP). First of all, the little-understood metabolism of grasses differs from that of dicotyledonous plants in that 2,4-D (broad-leaved weed-killer) produces only a fraction of its usual growth-regulating effects. This means that most hormone-type herbicides are not satisfactory control agents for weedy grasses. A second factor involved, largely physical, is that narrow grass leaves are difficult to wet with a spray solution, even when a wetting agent or "sticker" is employed. The third, and certainly a vital factor, is that the growing point, or region of greatest meristematic activity, is at the base of the plant. The sheltering effect of top growth, plus the usual presence of some ground mulch, acts to protect this growing point. For these reasons very few contact sprays, such as DNOSBP, are useful on grasses unless applied when the grass plant is in the very earliest seedling stage.

Carrizo cane (*Phragmites communis*) infesting an irrigation canal near Reno, Nevada. Oil-type herbicides now materially reduce the labor involved in cleaning these grass-clogged systems, but the effects are only temporary. New work with CMU and TCA-2,4-D combinations may eventually provide an effective means of root-kill.[3] Formerly, control could only be accomplished by expensive mechanical means. (U. S. Bureau of Reclamation.)

The first successful forward strides were made when chemists and plant physiologists in England during the early days of World War II began experimenting with a host of chemicals to find something selective toward grasses. Out of these trials a single chemical family showed promise, the *n*-phenyl carbamates.[6] The most active member, *o*-isopropyl *n*-phenyl carbamate (IPC), prevented the emergence of many types of weedy grasses when applied to the soil in light concentrations. With this discovery, interest in grass herbicides expanded rapidly. From England IPC was brought to the United States, where several researchers began attempts to apply it in our own agricultural system.[7, 8] Meanwhile its action was determined to

Sugar-beet field in northern Colorado where foul growth of wild oats (*Avena fatua*) frequently limits production. Left, untreated area; right, IPC applied at 6 pounds per acre and disked in prior to planting. Selective action accounts for the excellent stands of sugar-beet seedlings on the treated area. (Great Western Company.)

be nonsystemic, apparently consisting of mitotic poisoning in the root tips, with a gross effect similar to that produced by colchicine treatment.[9, 10] Nongrassy crops such as legumes, beets, and onions required much higher dosages in order to exhibit the same symptoms.

At about the same time another research group in the Midwestern United States became interested in finding a chemical suitable for controlling local perennial grasses. The most outstanding results were achieved with the alkali and ammonium salts of trichloracetic acid.[11] The most satisfactory member of this group was sodium trichloracetate (NaTCA). Although the physiological action of TCA has never been satisfactorily described, it is considered to be one of the so-called alkaloid reagents, and therefore capable of precipitating protoplasm.[12] In low concentrations, TCA applied to the soil killed germinating seedlings, just as IPC did, but at very high rates functioned more as a powerful, though temporary, soil sterilant. At light concentrations, crops such as beets, tomatoes, and onions were selectively weeded of annual grasses by TCA.[13]

For several years following World War II the prestige of grass herbicides was at a low ebb. This was partly due to the more successful use of 2,4-D and 2,4,5-T on other weed problems, but mostly it was a direct result of the inconsistencies obtained in field trials following the preliminary studies. In some areas workers abandoned the materials, or else concentrated on the nonselective eradication of plant life on nonagricultural locations. Other workers, who felt that certain keys were needed for the successful application of pre-emergence herbicides, kept up their explorations on a small scale in the hope of eventually finding the critical clues. New interest in graminicides was kindled in 1949 by the discovery of maleic hydrazide (MH), a hormone-type compound exhibiting systemic action in established grasses.[14] Applied to foliage, it inhibited growth, flower formation, and the development of viable seed.[15-20] Further studies revealed that MH inhibited respiration, with the inference that dehydrogenase activity was affected.[21] MH is still comparatively new, and its possibilities have not been fully exploited.

New evidence meanwhile began to supply an-

swers regarding some of the failures obtained with IPC. Workers in the Northwest reported that IPC functioned best against members of the subfamily Festucoideae, and that a newly introduced relative, 3-chloro-IPC, appeared superior against species of the subfamily Panicoideae.[22] In addition, it has been consistently demonstrated that soil type, moisture conditions, temperature, and date of application are critical factors which must be considered in using the carbamates.[23–25] Cultural practices are also involved. This year several thousand acres of peas in the Palouse region of the Pacific Northwest were treated with IPC for control of wild oats, with highly profitable yield increases.[4] In the Rocky Mountain area, after four years of research, technologists have perfected the application of IPC for weeding wild oats out of sugar beets.[5] Spraying schedules will be a part of the general field plan for several thousand acres of beets in the 1953 season. In both instances the key to success of the treatment appears to be direct incorporation of the chemical into the soil at the time of ground preparation. In dry climates such a procedure obviates the necessity for rainfall in moving the material to the root zone of the germinating wild oats.

3-Chloro-IPC has been found effective in killing cheat grass without injury to desirable perennials.[26] This has particular significance to the grower of grass seed, to the owner of irrigated pastures, and to the rancher in the cheatgrass areas of Oregon, Idaho, Montana, and elsewhere. When

Prolific seeding of annual cheat grass (*Bromus tectorum*) accounts for its unmatched ability to invade depleted perennial grass range. Here the dry "rough" and current year's crop of seed mask a relict stand of blue grama (*Bouteloua gracilis*), reduced to about 20 per cent coverage by many years of overgrazing.

Mature plants of foxtail barley (*Hordeum jubatum*). Ripe awns of this species cause mechanical injury by working their way into the mouth parts of sheep and cattle.

Test plant of *Hordeum jubatum* sprayed with maleic hydrazide at the rate of 3 pounds per acre. Although the same age as plant shown in preceding photograph, flowering has been markedly inhibited without injury to leaves.

Some of the sheep pictured here suffered injury and subsequent infection from grazing in these ripe stands of foxtail barley. The sharp awns are hygroscopic and twist into the flesh whenever they become attached. C-IPC controls seedlings of this pest, but established plants are not affected.

sprayed on the ground surface during the cool months it does not break down readily, and the residual effect after eight months is known.[15] 3-Chloro-IPC also controls crab grass in cotton, and was eminently successful during the current season in the Mississippi Delta area.[4] The use of IPC and 3-chloro-IPC appears to be restricted to those areas where the planting date occurs in early spring and where weed growth can be expected almost immediately following planting. Hot, humid weather causes the rapid breakdown of these materials, and for this reason they are not suited to certain types of agriculture where late planting is practiced.

Use of Na-TCA has now become widely accepted for both perennial and annual grasses, particularly in the South and Southwest. Since sugar cane exhibits a curious tolerance to TCA, the situation is made to order for controlling Johnson grass. Best results have been achieved where a combination of cultivation and chemical has been employed, reportedly excelling either approach used alone.[27] Apparently the exposure of the rhizomes enhances the effectiveness of TCA. Other uses of TCA have been developed for controlling small-seeded annual grasses in sugar beets and other crops, without serious injury to the crop plants themselves.[13]

Although not strictly graminicides, mention should be made of the usefulness of certain dinitrophenol and hydrocarbon herbicides in controlling seedling grasses. In areas where these materials are plentiful and reasonable in cost, many annual grass species may be killed in the early emergent stage. Established perennials are not seriously affected, as these materials kill only tops. DNOSBP, certain weed oils (both olefinic and aromatic), and hydrocarbon by-products of the petroleum industry compose the bulk of this class of compounds. A new addition to this general group has been recently introduced in the form of shale oil herbicides.[28]

Chemists are also exploring more new compounds every year for possible new and superior grass herbicides. Examples of recently released experimental compounds are 3-*p*-chlorophenyl-1, 1-dimethyl urea (CMU) and disodium 3,6-endoxohexahydrophthalate (Endothal). These materials are showing particular promise and may eventually occupy a definite place in control of grass weeds.

Unsolved Problems in Grass Weed Control

The new respect with which grass herbicides are regarded by no means implies that an end point has been reached in their development, or that untouched problem areas do not exist. On the contrary, all that has been achieved in the past eight or nine years represents a mere scratching of the surface possibilities, both in the United States and abroad. A challenging and far-reaching problem exists, for instance, in the reclamation of the annual grass ranges of the California coast area.[29] For many years plant breeders have been attempting to find perennial species that will become established and crowd out the annuals, but with little practical success. The very definite possibility exists that chemical eradication, closely followed by reseeding with desirable perennial grasses, may one day find a practical application in this large area. Another compelling problem involves the reclaiming of millions of acres of relatively low-value cheatgrass range in the West. The difficulty lies in reducing the cost per acre to a level of economic feasibility, perhaps by means of low-volume airplane application. Certainly the Kans grass problem in India will have to be met squarely, and the difficulties of chemical spraying in a country with only negligible numbers of power applicators will call for the best in research ingenuity.

Each climate has its special grass weed problems, and no one method or material has yet found universal application. Exploration in the field of graminicides has therefore become an urgently necessary adjunct to the whole agricultural research program.

ROGER M. BLOUCH‡

Department of Botany and Plant Pathology
Colorado Agricultural and Mechanical College

Composition of soil affects selectivity of grass herbicides. When TCA was applied at 15 pounds per acre to these flats, all weedy grasses were killed. However, on the sandy soil (upper) and clay soil (lower) the crops of sugar beets, alfalfa, and sweet clover were suppressed. Only on the loam (center) was selectivity apparent. Direct factors appeared to be the higher percentages of organic matter and fertilizer elements present in the loam. Note tolerance of onions.

References

1. TIMMONS, F. L. *Foreign Agr.*, **16**, (5), 99 (1952).
2. STAMPER, E. R., and CHILTON, S. J. P. *Down to Earth*, **6**, (4), 9 (1951).
3. SHAW, J., and BOWSER, C. Private communications (Oct. 1952).
4. WITMAN, E. D. Private communication (Oct. 1952).
5. NELSON, R. T. *Through the Leaves*, **40**, (4), 10 (1952).
6. IVENS, G. W., and BLACKMAN, G. E. *Symposia Soc. Exptl. Biol.*, **3** (1949).
7. ALLARD, R. W., *et al. Botan. Gaz.*, **107**, 589 (1946).
8. MITCHELL, J. W., and MARTH, P. C. *Science*, **106**, 15 (1947).
9. DOXEY, D. *Ann. Botany* (London), **13**, 329 (1949).

† The author's own research in the field has been supported in part through the Colorado Research Foundation by grants-in-aid from the Columbia-Southern Chemical Company, Colorado-Wyoming Academy of Science, and the Herman Frasch Foundation.

10. ENNIS, W. B. *Am. J. Botany*, **35**, 15 (1948).
11. MCCALL, G. L., and ZAHNLEY, J. W. *Kansas State Agr. Expt. Sta. Circ. 255* (1949).
12. HILL, G. A., and KELLEY, L. *Organic Chemistry*. Philadelphia: Blakiston (1943).
13. BARRONS, K. C. *Down to Earth*, **6**, (4), 8 (1951).
14. SCHOENE, D. L., and HOFFMAN, O. L. *Science*, **109**, 588 (1949).
15. BLOUCH, R. M. Unpublished master's thesis, Colorado A & M College (1952).
16. CURRIER, H. B., and CRAFTS, A. S. *Science*, **111**, 152 (1950).
17. CURRIER, H. B., DAY, B. E., and CRAFTS, A. S. *Botan. Gaz.*, **112**, 272 (1951).
18. ENGEL, R. E., and AHLGREN, G. H. *Agronomy J.*, **42**, 461 (1950).
19. GREULACH, V. A. *Texas J. Sci.*, **2**, (2) 219 (1950).
20. MOORE, R. H. *Science*, **112**, 52 (1950).
21. NAYLOR, A. W., and DAVIS, E. A. *Bull. Torrey Botan. Club*, **78**, 73 (1951).
22. FREED, V. H. *Proc. Thirteenth Western Weed Control Conf.* (1952).
23. BLOUCH, R. M., and FULTS, J. L. In press.
24. FREED, V. H. *Weeds*, **1**, (1), 48 (1951).
25. FREED, V. H., and BIERMAN, H. E. *Oregon State Expt. Sta. Bull. 483* (1950).
26. BLOUCH, R. M., FULTS, J. L., and THORNTON, B. J. *Research Progr. Rept., Thirteenth Western Weed Control Conf.*, 22 (1952).
27. WILLARD, C. J., and PETERS, R. A. *Down to Earth*, **7**, (2), 10 (1951).
28. BOHMONT, D. W. *Wyoming Agr. Expt. Sta. Bull. 320* (1952).
29. SAMPSON, A. W., CHASE, A., and HEDRICK, D. W. *California Agr. Expt. Sta. Bull. 724* (1951).

A NEW INSTITUTE FOR A NEW BIOLOGICAL METHOD

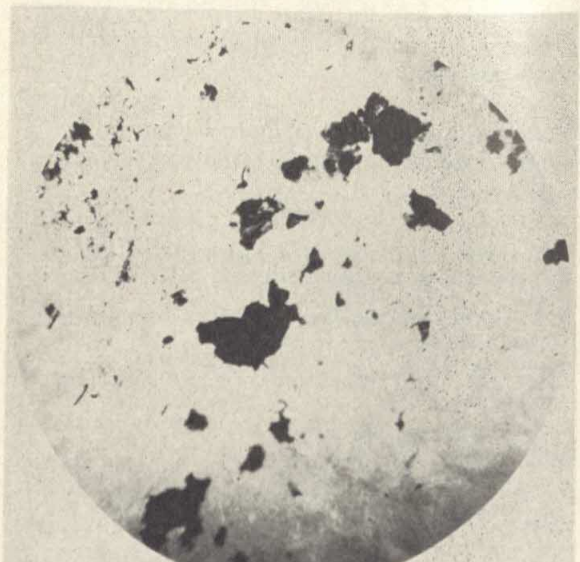
NINE years ago a study was begun on the biological activity of blood outside the body. A technique has been developed for the tissue culture of blood and for observation of the response of its cells to infective and biochemical agents of disease. Significant differences between the blood in health and in disease have been noted. It is observed by vital staining that disease-producing agents may inhibit the respiration of, or destroy, both red and white blood cells.

The new study technique has been named the hemobiological method. Blood is a fluid tissue which normally is accessible to every cell in the body, as a common carrier of food and oxygen and the waste products of metabolism. Thus it logically may be expected to serve as an index to the immune status of the entire body.

Pioneer developmental work has been done by Elizabeth Willcox Kidwell, a microbiologist, who has been advised and assisted by scientists in many fields. Her research was based on studies in animal infection, especially Bang's disease in cattle, and on the work of Wright and other early immunologists on the defense mechanism of the body. During the past century, research on blood has been intensive, revealing much fundamental information concerning its chemical, physical, and immunochemical properties.

The Willcox Research Institute was incorporated on June 2, 1952, to permit study of the new method by scientists in many fields, well represented by the officers and directors of the new organization. The president of the Institute is J. Edward Spike, Jr., secretary of the Research Committee of the American Cancer Society; the vice president and medical director is Stanislaus H. Jaros, chief of the Allergy Medical Division, Grasslands Hospital, and associate medical director of Ayerst, McKenna & Harrison; the associate medical director is Daniel J. Feldman, of the staff of New York University Medical College and Bellevue and Goldwater Memorial hospitals; secretary-treasurer is Mrs. Kidwell. Other directors are Walter F. Willcox, professor emeritus of statistics, Cornell University, and honorary president of the International Statistical Institute; Roy M. Allen, consultant in microscopic sciences; John W. Butterworth, division engineer, Mackay Radio & Telegraph Co.; Clifton Read, publicist; and Wm. Curtis Pierce, member of the firm of Sullivan and Cromwell, attorneys.

The study has been accelerated by the development of a new technique for the preparation of microscopic wet slides by means of Deltaseal, a clear liquid plastic which hardens on drying, is nonabsorbent, noninflammable, and has little effect on blood. It is used to mark the slide with a rectangular chamber of the same dimensions as the



Blood damage in two cases of degenerative disease: Crystals of reduced hemoglobin produced by pancreatin in heparinized blood from a case of diabetes. (×16.5.) Effect of *Brucella* toxin on clot of a patient with rheumatoid arthritis; black clumps produced by the oxidation of the iron-bearing portion of hemoglobin. (×33.)

TABLE 1

PRINCIPAL CHARACTERISTICS OF BLOOD RESPONSE WHICH DISTINGUISH HEALTH FROM DISEASE

Phenomenon	Optimum Resistance	Maximum Effect on Disease
Leucocyte response to bacterial antigen or biochemical	Complete and immediate disappearance	Clumped, degenerated, killed, dissolved
Erythrocyte survival in same slide	Typical form of biconcave disk more or less intact—may remain unchanged for weeks	Degenerated or dissolved to an amorphous mass in a few hours or days
Hemoglobin reduced	Typical light-red color	Violet-red color
Crystals	None or at edge only	Massive, especially at center
Hemoglobin oxidized to methemoglobin	" " " " "	From rust color to deep-brown, part or all of slide
Oxidation of iron atoms in black clumps	None	May be profuse, especially in clot
Clot	Dissolves within a few hours, leaving trace of white film	Persists indefinitely, solid as a piece of tissue with crystals or oxidized clumps; polarizes light
Response to factors associated with hypersensitivity and tissue permeability	Immediate disappearance of leucocytes and lysis of clot	Damage to blood and clot
Response to antigens of *Staphylococcus, Streptococcus, Brucella,* and *E. coli*	Resistance to endotoxins; resistant or superficial crystallization by whole cells	Damage to blood and clot by whole cell and one or more endotoxins, often by all four organisms

cover slip. Slide, cover slip, and instruments are flamed, and the preparation is made under a sterile lamp. Whole, clotted blood is used, with a piece of clot crushed in the center of the chamber, which is then flooded with blood. A bacterial antigen or biochemical agent is added, and the cover slip is sealed in place with plastic. With air and contaminants excluded, the reaction may be observed to completion, possibly weeks later. A series of slides, each containing a separate test substance, may be used to evaluate the individual's resistance to a wide range of disease-producing agents.

If a drop of weak formaldehyde is added to a slide before the antigen is introduced, the blood is inactivated, and no significant damage occurs. The blood/antigen reaction appears to depend on the response of the leucocytes, as is true also when biochemicals are used. Thousands of studies on the blood of hospital donors showed that in bloods of the strongest resistance, every leucocyte disappeared immediately and completely, and the erythrocytes remained practically unharmed. In the blood of diseased individuals certain failures of this mechanism occur, followed by the appearance of various types of blood damage.

The criteria which distinguish health from disease appear to be relative, rather than absolute. All the bacterial and about 70 biochemical agents employed were found capable of producing damage in the blood of diseased animals and humans and the resistant response in the blood of those clinically well. Table 1 illustrates these comparative differences. Data collected from years of study in a variety of clinical states suggest that the disease complex may be associated with impaired blood response in vitro to many such substances. The crystallization of reduced hemoglobin, identified in the slides by spectrophotometer, appears to be associated with superficial inflammation or infection. In blood from patients with degenerative disease, known causative organisms, such as the tubercle bacillus, or biochemicals specific to the affected area produce damage mainly in the clot. All patients with a given syndrome appear to have certain factors of blood damage in common: pancreatin produces such damage in bloods from patients with diabetes or other pancreatic conditions; the digestive enzymes in peptic ulcer; the sex hormones in abortion and other gynecological disturbances. This is true also of substances that Jaros has shown to be associated with hypersensitivity and tissue permeability to which the bloods of the healthiest donors have been found resistant. Four organisms of chronic infection, *Staphylococcus, Streptococcus, Brucella,* and *Escherichia coli,* have been disrupted by ultra sound waves and their endoantigens separated by ultracentrifuge. Cases of chronic infection by each of these organisms have been found associated with blood damage

by one or more of the specific endotoxins.

Another type of blood damage not found in good health, but occurring frequently in advanced degenerative states, is due apparently to the oxidation of hemoglobin to methemoglobin and to clumps of black, roughly crystalline material. The latter has been identified by infrared spectrophotometry as an oxidation product of the iron-bearing portion of hemoglobin.

The relationship of various degrees of damage to the disease process in vivo requires much further study. A scoring method has been devised to show the individual's rating to each substance. Thus statistical data comparing responses in disease and health can now be obtained. The possibilities of the method for diagnostic study in various clinical fields will be investigated by the new institute. A grant has been received from the Victoria Foundation, which aided the developmental work.

Blood samples have been supplied by the Blood Transfusion Association of America, by St. Vincent's Hospital, by other hospitals, and by many physicians.

J. Edward Spike, Jr.

The Willcox Research Institute, Inc.
16 E. Ninth Street, New York 3

Example of damage to clot shown by effect of urea on blood from a case of cancer of the kidney. The structure of the clot is shown in contrast to the dark area of blood. One segment is completely crystallized. The upper portion shows a few crystals of reduced hemoglobin and the polarization of the fibrin of clot, which shows no evidence of crystallization, even under very high magnification. (Polarized light; × 11.5.)

SMO ON THE AIR

Station	Sponsor	Time
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Broadcast Limited (Modern Science)	9:45 P.M.
	(Irregular)	
KBER, Baker, Ore.	Baker-Union Department of Health (Research Report)	....

The Editor of The Scientific Monthly will be glad to cooperate with university or other educational stations interested in securing scientific material suitable for broadcasting.

BOOK REVIEWS

ORIGIN AND EVOLUTION OF CULTIVATED PLANTS

Plants, Man and Life. Edgar Anderson. 245 pp. $4.00. Little, Brown, Boston. 1952.

WRITTEN primarily as a popular book for the intelligent of the general public, *Plants, Man and Life* fulfils that aim in being both entertaining and absorbingly instructive reading for the nonprofessional in botanical fields of learning. In a very real sense, however, the true significance of the book can be appreciated to the fullest extent only by those with a wide scientific background. Books on science are seldom written in a style which creates the impression of personal contact with the writer, but to read *Plants, Man and Life* is like talking with the author. Edgar Anderson is a stimulating conversationalist; this is a stimulating book. Stimulation may, however, be superficial, whereas the theme of this book is thought-provoking. It is broad in scope, tolerant in principle, optimistic in outlook, and specific in pointing to important but neglected areas calling for cooperative research.

The subject is essentially the origin and evolution of cultivated plants. The keynote is that we know precious little about this subject, although new information is coming in at an accelerating rate from related fields, and it is high time that we work to develop ways and means specifically adapted to throw light on this important problem. It is important in an economic sense because an understanding of the history of cultivated plants would lead to more intelligent efforts to improve and utilize them. In a broader sense it is a contributing factor to the understanding of man, for man's cultural development is intimately tied up with his use of plant products. Furthermore, "A bringing together of men in different disciplines for the study of cultivated plants and weeds, an active co-operation of historians, anthropologists, and ethnobotanists would have as its important aim the advancement of understanding in that particular problem. Its greater ultimate effect would be the catalytic transfer of techniques and attitudes from one field to another."

Anderson begins his book by directing attention to man's transported landscapes. The plants that are closely associated with man are those that he has brought, intentionally or otherwise, along with him. The weedy camp followers and cultivated plants are, paradoxically, least known taxonomically, largely because their characteristics have been greatly altered and mixed by association with man. This gap is serious because to most of the scientific world the accurate classification of the plants closely associated with man is of more importance than that of all the other plants of the world put together.

Evidence from the fields of cytology and genetics has done much in recent years to clarify the origins of certain cultivated plants. The unraveling of the story of wheat is an excellent example of how cytogenetic studies have contributed to our understanding of the interplay between polyploidy, weedy relatives, and selection through long periods of time to produce a major crop plant.

There is a brief account of the tragedy of N. I. Vavilov, the great Russian botanist, who drew attention to the fact that the variability of cultivated plants is concentrated in a few relatively small geographic centers. The author suggests that these areas are points at which previously separated floras have come together and hybridized. It is often difficult to detect evidence of hybridization or, for that matter to distinguish ultimately a wild from a cultivated plant. Anderson explains how "pictorialized scatter diagrams" may be used as a tool to help clarify the composition and evolution of a population.

The history of cultivated plants is not only a problem in biology but quite as much one in archaeology, anthropology, nutrition, and other fields among which there is usually little communication. "At the present time, anthropologists and applied biologists are so far apart in their thinking that they seldom realize they have problems in common. Knowledge is all of one piece but universities (by tradition and for budgetary reasons) are divided into departments." Under these conditions a problem that cuts across department lines suffers in proportion to its interdepartmentalization.

Anderson's tribute to the late Professor Oakes Ames, of Harvard, who first aroused his interest in economic botany, is written with charm and provides some of the best popular reading in the book. Ames stressed the antiquity of agriculture and primitive man's uncanny ability to ferret out all known useful plant products.

The importance of the dump heap, a disturbed open habitat created by man, is emphasized throughout the book as a likely place for the origin of most cultivated plants. It is here that weedy plants congregate, thrive, and hybridize, bringing about a great diversity from which man could select the most promising types for his own use.

Plants, Man and Life ends with a chapter on the technique of "the inclusive herbarium," a means of bringing together in concise workable form the large amount of data needed to study adequately populations of cultivated plants. It is a clarion call to taxonomists to get busy and take their rightful place in this endeavor.

Finally, since there is an increasing tendency for scientific studies, including those on cultivated plants, to be relegated to "Our enormous government research labo-

ratories (which) tend to be staffed by able plodders who do not question well-established concepts"—then, "What the world needs if (this or any) science is to forge ahead is some kind of system which will allow the bold young men to try out their wild ideas in spite of the judgement of older and usually wiser men."

HAROLD H. SMITH
Department of Plant Breeding
Cornell University

THE WORLD AROUND US

Journey into Wonder. N. J. Berrill. xiv + 338 pp. $4.00. Dodd, Mead, New York. 1952.

Geography of Living Things. M. S. Anderson. ix + 202 pp. $2.75. Philosophical Library, New York. 1952.

THE common denominator of these two books is geography: man's relations with and knowledge of the planetary surface on which he lives. The Anderson volume is a straightforward, textbookish account of "biogeography," and the Berrill volume is an excursion into history, "the story of man's discovery of the natural world around us"—in the words of the subtitle.

Mr. Berrill is concerned mostly with the tropics, the poles, and the oceans, and he tells again the story of the great voyages that made the life of these regions a part of the subject matter of European knowledge. It is very hard to write freshly about the voyages of Columbus or about Darwin's experiences on the *Beagle*, but Berrill manages to keep the interest even of a hardened reader of natural history books. He does this by using the narratives of the voyages chiefly as a connecting thread on which to string bits of history of science, of his own philosophy, and of modern knowledge about the creatures encountered. Berrill is a man of wide interests and deep knowledge, who writes clearly, simply, and well; any book by such a man is bound to be a refreshing experience in reading, however hackneyed the subject.

The first five chapters are concerned with the voyages of Columbus. These are followed by an account of things seen by the Elizabethans, John Hawkins, Francis Drake, and Richard Hawkins. The middle portion of the book is taken up with the explorations of the oceans, using the travels of William Dampier and James Cook as background narrative. In the last section of the book, the tropics are viewed through the eyes of Alexander von Humboldt and Charles Darwin, and the Antarctic through those of the companions of Scott. The book ends with an excursion into geological history, built around the vicissitudes of the mammal faunas of the Americas.

I found myself most engrossed when Berrill was writing about the sea, where he could bring his own rich knowledge and experience to bear. I occasionally felt an impulse to lift my eyebrows when he got on land and into the tropical forest; but in such places he stuck pretty close to his sources.

The little book by M. S. Anderson belongs to a "Teach Yourself" series and has all the defects that seem to go with such eleemosynary intentions. If you brave the pedestrian and simple-minded prose, the content seems to be a good-enough account of some of the concepts of contemporary human geography, although I was often struck by what seemed to be a lack of awareness of the cultural elements of man's relations with nature. The book covers man as an animal; direct effects of environment on man; man, rocks, and water; man and his food; the soil; soil conservation; and pests and diseases.

I would like to ride a hobby by pointing out the complete absence of documentation in both books. It seems to me that Berrill might have added a section in the back of his book on available editions of the travels he so delightfully annotates. Surely this would in no way bother the general reader at whom he is aiming, and it might even lead such a reader on into further book exploration. Such an appendix, with indications of other sources, would be a great help to Berrill's fellow-naturalists. The absence of a reading guide from a teach-yourself book like that by Anderson seems to me to have no excuse whatever.

MARSTON BATES
Department of Zoology
University of Michigan

THEORY AND PRACTICE IN HIGHWAY CURVES

Highway Curves. Howard Chapin Ives. (4th ed., rev. and including *Highway Surveying, Location, Geometric Design, and Earthwork,* by Philip Kissam.) xvii + 389 pp. Illus. $7.00. Wiley, New York; Chapman & Hall, London. 1952.

THE aim of the fourth edition of this popular book is the same as the first—to present the theory and practice of highway curves as followed in this country. In the more than twenty years since that first edition, however, many advances in highway design have been accomplished. The book therefore has been completely revised and the text rewritten to include selected methods for highway surveying, location, geometric design, and earthwork that, in the words of the preface, "simplify both the field operations and the mathematical solutions."

Part I outlines the principles of usually only one method each of handling simple, compound, reversed, vertical, and spiral curves. The method chosen for each is well discussed, and sufficient examples that outline the procedure to be followed are presented for each type of curve. The even degree and even radius methods of expressing curvature are explained in detail. Numerous formulas and definitions are found throughout the text, with illustrative figures and numerical examples.

The Searles spiral has been selected, and its ease of application to transition areas between tangents and circular curves is explained. All necessary tables for any length of spiral required and for any radius of curve with which it connects are given.

The chapters on earthwork present in a concise man-

ner the procedure for area computation of cross sections and the volume computations from these areas. The determination of haul and the construction and use of a mass diagram are discussed in detail.

Part I also includes a brief résumé of the principles of the preliminary and the location survey and the field procedure for making these surveys. The application of aerial photographs to the making of these surveys is briefly presented. Although the preface states that the principles of highway economics are outlined in the initial chapters, the space given to this important subject is negligible.

A concluding chapter on the computations and surveys necessary for highway interchanges is included, and a rather complete example of the necessary computations is given.

Part II is composed entirely of tables, trigonometric formulas, and an explanation of the tables. These include tables of logarithms, natural trigonometric functions, squares and cubes of numbers, and common conversion tables and useful numbers. Many of these tables have been taken from the previous editions of this book. New tables in this edition make it unnecessary to use logarithms or a calculating machine to compute the values for a simple curve. The necessary tables for curves of even radius, curves of even degree, and Searles spiral are presented in clear, compact form. The explanation of all tables is also clear, and each is well illustrated with examples. Ten pages of definitions and mathematical formulas and identities are included at the end of Part II.

This highway engineering book is designed strictly for engineers. It is handsomely bound and conveniently sized to serve as an excellent reference volume for the selection of fast, precise field and computational methods and the tabular information needed for the office and field handling of highway curves.

HAROLD L. MICHAEL

School of Civil Engineering
Purdue University

ATMOSPHERIC DISCUSSION

Across the Space Frontier. Wernher von Braun *et al.* Cornelius Ryan, Ed. xiv + 147 pp. Illus. $3.95. Viking, New York. 1952.

ONE'S first reaction on seeing a book with this title is almost always "Another of those scientifictive books which mix a little bit of science and a large amount of nonsense with an enormous fantasy." However, when one sees that scientists such as Whipple and Kaplan are among the co-authors, second thoughts begin to enter one's mind, and when one finally settles down to read, it turns out that this volume—an expanded version of a symposium organized by *Collier's*—presents a serious and scientific attempt to give a program for future developments in man's ever-growing want to reach beyond his present confines.

Von Braun, one of the world's foremost experts in rocket research, describes how it might be possible to establish in a satellite orbit 1075 miles above the earth surface a "space station;" that is, a man-made satellite from which flights to the moon or other planets might take place, but which first of all would serve as an observation post both for astronomical purposes and for controlling man's activity on the earth. One can hardly help but feel that Von Braun's statement that the establishment of this "space station" might take place within ten to fifteen years, provided the necessary money (4 billion dollars!) were available, could well be true.

Haber, who until recently was with the U. S. Air Force's Department of Space Medicine, discusses in great detail whether human beings could survive either the journey to the space station or living on (or, better, in) this man-made satellite. This chapter was to me the most fascinating of all the contributions and showed to my mind clearly that even if survival were possible (and the odds seem to be in favor of it), the hazards are great and meteors seem to be an ever-present danger.

Other contributions to this fascinating book are a lucid chapter by Kaplan on our atmosphere, a discussion of the space station by Ley, a discussion of the legal aspects by Schachter, and a discussion of the value of an astronomical observatory outside our atmosphere by Whipple. The only weak spot is the introduction by Ryan, the editor, which is written in true journalistic style without sufficient scientific restriction, and it is just the scientific approach in most of the discussions which makes this book such interesting and fascinating reading.

D. TER HAAR

Department of Natural Philosophy
The University, St. Andrews, Scotland

THE HIGHER LEARNING IN CALIFORNIA

General Education in Action. B. Lamar Johnson. xxvi + 409 pp. $4.00. American Council on Education, Washington, D. C. 1952.

EDUCATION, as a professional discipline, is the target for much harsh and severe criticism at the hands of those who feel it is wallowing around, expending great effort but accomplishing very little. However illogical and poorly thought-out much of the attack is, it does represent real dissatisfaction with manifest weaknesses and excesses with which education can rightly be charged. Perhaps the most important of these criticisms are presented in Robert M. Hutchins' *The Higher Learning in America*, in which he has given expression to a wide range of dissatisfaction with American education. Many of his specific charges are well taken: the confusion as to the purposes of higher education, the function of the high school—a function which has yet to be clearly defined, the cafeteria offerings of universities the curricula of which are built upon popular demand, the fractionization of the college curriculum into isolated subjects. His charge that the

university has spawned a sort of anti-intellectualism is a telling indictment of what goes on behind the ivied walls of many a college and university building.

Dr. Hutchins' indictment against current evils in higher learning is competently thought out, and some of his proposals for remedying the defects are worth the most serious examination and study. In his recommendation that education ground itself in "metaphysics" Hutchins brings his study to a grinding halt. He disavows any desire to delineate the kind of metaphysics he beckons us to follow. Like Erasmus, Hutchins is direct, forceful, and specific in criticism, but is unable to follow through in terms of definable, workable solutions. Perhaps it is enough that a critic should lay bare the weaknesses of our educational theory and practice and leave to others the task of construction and synthesis. Herein lies the value of the volume under review, *General Education in Action,* a book of genuine accomplishment and hope.

If, as Hutchins alleges, the higher learning is marked by confusion, the confusion is perhaps the turmoil that inevitably accompanies experiment and the testing of promising educational concepts and techniques, rather than the bewilderment that signifies loss of direction and purpose. Like our nationwide experiment in democracy, we have been at the business of education a comparatively short time and we have much to learn. Nevertheless, we do have some achievements in which we may well take great pride, the most notable being our vast system of public schools at all levels. To keep its theoretical structure and classroom practice maximally productive, and genuinely conducive to democratic action on the part of millions of students, is an enormously complex task. Mistakes will be many and singular achievements few, albeit notable. *General Education in Action* brings together the findings of the California Study of General Education in the Junior College. Although repetitive and sometimes bogged down by obscure educational terminology, the report is an exciting statement of the role and promise of junior colleges in the California region. As President Conant of Harvard has indicated on numerous occasions, the extension of the junior college movement into one of nationwide proportions may be a workable answer to the problem of higher education for many of our young citizens. The California study lends convincing support to a thesis Dr. Conant has espoused for some time.

FREDERICK E. ELLIS

College of Education
University of Minnesota

BRIEFLY REVIEWED

Mount McKinley and the Alaska Range in Literature: A Descriptive Bibliography. Bradford Washburn. 88 pp. Museum of Science, Boston. 1951.

THIS bibliography is the first attempt to gather together a list of the literature on, and interesting references to, Mount McKinley and the Alaska Range. It is doubtful whether completeness has been achieved in this initial endeavor. The majority of titles listed date from the period following the acquisition of Alaska by the United States. There are nine references from the era of Russian domination, but only three by Russians. It seems probable that others exist, though perhaps they are not available. A large number of articles concerning Mount McKinley and contiguous areas is included. The unique position of Mount McKinley, as the highest mountain in North America, accounts for the predominance of titles concerned with exploration and mountain climbing. The struggle to reach the summit and Dr. Cook's claim to success provide numerous papers. Bradford Washburn's concise descriptions of the references concerned illuminate and clarify the accomplishments in this region. Other portions of the Alaska Range, notably the section traversed by the Richardson Highway, are less thoroughly covered than the McKinley region. For example, the articles concerning the rapid advance of Black Rapids glacier in 1936 are not included.

The bibliography contains 264 entries arranged alphabetically: 200 titles are listed by author; 64 by the name of the newspaper or periodical. The lack of classification is a feature that can be condoned only by the brevity of the bibliography. The separation of the listings into two parts—one devoted to articles by known authors, the second, to accounts in newspapers and periodicals—would in large part segregate primary from secondary sources and thus add to the usefulness of the bibliography.

Aside from the above criticisms, Bradford Washburn is to be congratulated for compiling a surprisingly long list of articles on Mount McKinley and the Alaska Range. His descriptive notes are never monotonous; not only do they depict the contribution each entry added to the knowledge of the area, but they also make the complete perusal of the bibliography an interesting summary of the history of exploration in the Mount McKinley–Alaska Range.

ELIZABETH W. OLMSTED

Lewiston, New York

An Essay on Method. C. Hillis Kaiser. vi + 163 pp. $3.25. Rutgers University Press, New Brunswick, N. J. 1952.

THE purpose of this monograph by a professor of philosophy at Rutgers University is twofold: first, to approach human culture objectively with the aim of classifying and describing its principal "nobler" activities and their differences; and, second, to point out defects in our present system of education and suggest changes which it is hoped would better produce leaders of ability in all fields.

To achieve the former, activities are divided into four exclusive categories: artistic, scientific, philosophic, and religious. In order to discuss their similarities and distinctions, these are further classified with respect to certain qualities. Thus, the author regards artistic and

scientific activities as being playful (done for pleasure) and immediate (concerned with sensed things), whereas philosophic and religious activities are serious and transcendental. On the other hand, philosophy and science both endeavor to draw conclusions, whereas religion and art do not, the former aiming at the achievement of the attitude of conversion, and the latter of aesthetic appreciation. The remainder of the book is largely devoted to the explanation and justification of these distinctions. To illustrate this approach, art, in Professor Kaiser's definition, refers not to the created thing, but the act of creation, and is not self-expression but self-amusement. Science is not a body of facts but of *acts* of discovery. More controversial, perhaps, is his definition of philosophy, for here he requires that its objective be the search for premises underlying the relationship of man and the world which shall be *necessary*, rather than merely possible. In his approach to religion he accepts the necessity of mysticism, and draws a clear distinction between religion and social humanitarianism.

In discussing the role of education in our society, he condemns what is only technical. The extreme progessive school he sees as a system in which the educated are taught by the uneducated, and condemns its use beyond the nursery school as leading to adult infantilism. To quote (p. 160):

"Our formal education, if it is to fulfill this ideal, should not merely tell our students about religion, or religions; it should make them religious. It should not merely portray the range of philosophical positions, historical and theoretical; it should encourage them to *take* a position. It should not merely acquaint them with art products and the history of art; it should make them into artists. It should not merely provide them with scientific information or technological training; it should make them learn to do science."

As a program of education for the "uncommon man," it has merit.

R. C. Buck

Department of Mathematics
University of Wisconsin

Fundamentals of Engineering Electronics. (2nd ed.) William G. Dow. 627 pp. $8.50. Wiley, New York. 1952.

FOR those who are familiar with the first edition (dated 1937) it will suffice to remark that changes in the new edition comprise the adoption of rationalized M-K-S units throughout, some rearrangement and revision of subject matter, the inclusion of devices such as the Cavity magnetron, klystron, traveling wave amplifier, and passing mention to crystal rectifiers and transistors.

Professor Dow's text is indeed a useful contribution to the field of electrical engineering. It is designed to impart an understanding of the physical principles underlying the operation of those nonpassive circuit elements that are the core electronic circuits. In order to accomplish this aim, the following subjects are treated: kinetic theory of gases, gaseous conduction, electromagnetic theory (including electron ballistics), quantum theory, electron theory of metals, and semiconductors. In keeping with the objective of the text, only about 15 per cent of the space is devoted to circuitry and applications. The author probably feels that such matters are given ample emphasis elsewhere in the literature.

It is interesting to compare this text and its title with *Electronics,* by Elmore and Sanders. Although the titles are similar, there is virtually no overlap in subject matter. This in itself is evidence of the remarkable growth of electronics in the past decade.

B. Gossich

Department of Physics
Purdue University

Between Pacific Tides. Edward F. Ricketts and Jack Calvin. Third edition, by Joel W. Hedgpeth. xiii + 502 pp. Illus. $6.00. Stanford University Press, Stanford, Calif. 1952.

AMERICAN students of marine biology are familiar with the earlier editions of this classic presentation of the littoral fauna of our Pacific coast. Joel Hedgpeth's new edition is a great improvement over the earlier publication and will certainly become a favorite on the laboratory tables at marine stations.

Hedgpeth has made two important contributions, one being a splendid chapter on Intertidal Zonation, the other a much improved and expanded appendix. The latter is an annotated systematic index and bibliography to which have been added numerous species, new sections covering intertidal insects and the marine mammals, and countless, carefully selected references. Adding to the beauty and usefulness of the book are a number of new photographs by Woody Williams, and several dozen new drawings, including 24 excellent cuts from Gilbert M. Smith's Monterey algae work. Throughout the body of the reprinted text, specialists have made all necessary changes in nomenclature. Hedgpeth has not only given the book more scientific backbone, but has also made his additions in a pleasing style and, at times, with tangy humor. It is a wonder, for instance, that his remarks near the top of page 475 and on the middle of page 473 escaped the "dollared" eyes of the American Printers' Union.

The publishers are to be commended on two other improvements. First is their courage in making this more compact edition a full inch less in thickness, despite the addition of 138 pages. The thinner, but superior, paper lends itself better to the line drawings. Second, they have reduced the useless 15 pages of "List of Illustrations" to a neat, single page. More houses should follow suit in eliminating these old-fashioned, publisher's "vanity pages," which only annoy the reader and the space-limited author.

R. Tucker Abbott

U. S. National Museum
Washington, D. C.

Roger Bacon and his Search for a Universal Science. Stewart C. Easton. viii + 255 pp. $4.00. Columbia University Press, New York. 1952.

THIS biography is the work of a critical historian who makes his sources and his method explicit. The critical attitude is indispensable in the study of Bacon because of the mass of legend that attributes to him breath-taking anticipations of modern inventions.

Bacon regarded science as having been handed down in an esoteric succession from Seth, a son of Adam, through the Hebrew patriarchs, the Egyptians, and the Chaldeans, to Aristotle; thence, through the Arabian commentators, to Bacon himself.

Another phase of Bacon's thought that will seem strange to us is that he regarded moral purity as a necessity for scientific research. The redemption of man, moreover, opened the cognitive faculties to a degree never enjoyed by the pagans.

Easton has little interest in Bacon's apparent anticipations of modern knowledge. Indeed, from the disciplines he lists as Bacon's concern—among which astrology and alchemy play a large part—one gets the impression that Bacon's modernity has been overrated. The *Doctor mirabilis* did, however, appreciate *scientia experimentalis.* Easton carefully translates this as "science of experience" rather than "experimental science," in order not to make Bacon appear to anticipate his later namesake.

Bacon left some thoughts that remain valuable. His idea that science has its ultimate usefulness in bringing man to Christ is obsolete, but the generality that social salvation is to be expected of the scientific enterprise is worth remembering. His perception of the interrelatedness of the sciences, the organic whole of knowledge, was a vision of which the twentieth century might well be reminded.

RUFUS SUTER

Army Map Service
Washington, D. C.

Science and Hypothesis. Henri Poincaré. xxvii + 244 pp. $1.25, paper-bound; $2.50, cloth-bound. Dover, New York. 1952.

Science and Method. Henri Poincaré. 288 pp. $1.25, paper-bound; 2.50, cloth-bound. Dover, New York. 1952.

ORIGINALLY written in 1903 and 1908, these commentaries on the nature of physical theories have an interest that is now chiefly historical. However, almost a half of the earlier work is devoted to the relationship of pure mathematics and the world, and this portion is still the final word on this question. The second volume contains the often-quoted biographical episode in which Poincaré analyzes the processes of mind which led to some of his own discoveries. Both books come in Dover's inexpensive paper-bound series.

R. C. BUCK

Department of Mathematics
University of Wisconsin

NEW BOOKS

The Elements of Nuclear Reactor Theory. Samuel Glasstone and Milton C. Edlund. vii + 416 pp. Illus. $4.80. Van Nostrand, New York. 1952.

Quantitative Analysis. Harper's Chemistry Series. William Marshall MacNevin and Thomas Richard Sweet. ix + 247 pp. Illus. $3.75. Harper, New York. 1952.

Industrial High Vacuum. J. R. Davy. viii + 243 pp. Illus. $5.50. Pitman, New York. 1951.

Organic Chemistry: The Chemistry of the Compounds of Carbon. (2nd ed.) Lucius Junius Desha. xvi + 595 pp. Illus. $6.50. McGraw-Hill, New York. 1952.

Archeology of Eastern United States. James B. Griffin, Ed. x + 392 pp. Illus. $10.00. University of Chicago Press, Chicago. 1952.

Calculus, A Modern Approach. Karl Menger. xxv + 255 pp. Illus. Mimeo. $4.50 + 35¢ postage. Institute of Technology, The Bookstore, Chicago. 1952.

Histoire Géologique de la Biosphère. H. Termier and G. Termier. 721 pp. Illus. Paper-bound, 8600 Fr. fr; cloth-bound, 9200 Fr. fr. Masson & Cie, Paris. 1952.

The Nile. H. E. Hurst. xv + 326 pp. Illus. $6.00. Macmillan, New York. (Printed in Great Britain). 1952.

Rocks for Chemists. An Introduction to Petrology for Chemists and Students of Chemistry. S. J. Shand. xii + 146 pp. + 32 plates. $4.50. Pitman, New York. (Printed in Great Britain). 1952.

Rockets Beyond the Earth. Martin Caidin. 304 pp. Illus. $4.50. McBride, New York. 1952.

Psychoanalysis as Science. The Hixon Lectures on the Scientific Status of Psychoanalysis. E. Pumpian-Mindlin, Ed. x + 174 pp. $4.25. Stanford University Press, Stanford, Calif. 1952.

Origins of American Scientists. A Study Made under the Direction of a Committee of the Faculty of Wesleyan University, Middletown, Conn. R. H. Knapp and H. B. Goodrich. xiv + 450 pp. $7.50. University of Chicago Press, Chicago. 1952.

Student Deferment in Selective Service. M. H. Trytten. viii + 140 pp. $3.00. University of Minnesota Press, Minneapolis. 1952.

Grafología y Grafotecnia. Alberto Posada Angel. xvi + 444 pp. Illus. Editorial Bedout, Medellin, Colombia. 1952.

The New Dictionary of American History. Michael Martin and Leonard Gelber. vi + 695 pp. $10.00. Philosophical Library, New York. 1952.

Automation. The Advent of the Automatic Factory. John Diebold. ix + 181 pp. $3.00. Van Nostrand, New York. 1952.

Associated Measurements. M. H. Quenouille. x + 242 pp. Illus. $5.80. Academic Press, New York. 1952.

The Methods of Statistics. (4th ed.) L. H. C. Tippett. 381 pp. Illus. $6.00. Wiley, New York. 1952.

The Medieval Science of Weights. Treatises Ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore, and Blasius of Parma. Ernest A. Moody and Marshall Clagett, Eds. x + 438 pp. $5.00. University of Wisconsin Press, Madison. 1952.

Introduction to the Foundations of Mathematics. Raymond L. Wilder. xiv + 305 pp. $5.75. Wiley, New York. 1952.

Physical Chemistry. (3rd ed.) Frank H. MacDougall. xi + 750 pp. $6.00. Macmillan, New York. 1952.

Diffusion in Solids, Liquids, Gases. W. Jost. xi + 558 pp. $12.00. Academic Press, New York. 1952.

LETTERS

DOCUMENTING THE SCHUSS-YUCCA

I AM delighted that at long last you have printed the results of an investigation that will vindicate the late Dr. Grünspann. Although a complete set of the *Handbuch der Yucca* is not available to me, I recall that nowhere in the portions I've seen has Dr. Grünspann clearly indicated why the Schuss-yucca grows so fast. Therefore the following observation, although not quite firsthand, surely should be included in the forthcoming third edition of the *Handbuch*.

My grandfather, in his youth, knew the yucca country very well. He was engaged in the freighting business, holding what would now be called a "cost-plus contract" as a freighting contractor to the government. The distances were so great, he often told me, that he was forced to sublet much of the mileage to other owners of mule-drawn equipment. But a portion he reserved for himself and personally operated his mule trains over that stretch. The reason for this, he explained, lay in the soil. He discovered, quite by accident, that in the yucca country the mule shoes seemed never to wear out; at no time had he ever seen this particular stretch of the long route too wet for good mule footing or so dry as to be dusty. He also insisted that he had never lost an animal on his route. They simply did not die in the Schuss-yucca country, although such losses were very troublesome throughout the West. After several of his mule trains had been operating there he discovered that he had to remove the shoes, since they had become too heavy through accretions of what appeared to be ordinary shoe metal. In self-defense he began to trade mule shoes with contractors; he sold them the "new" heavy shoes worn by his teams, taking in exchange the frayed and worn shoes of his competitors. He was thus able to amass a great fortune on which, of course, no income tax had to be paid. His stories of the Schuss-yucca so closely paralleled the amazing evidence presented by Dr. Albrecht and Professor Grünspann that for many years the family ruled that they were not to be repeated to strangers. Although Grünspann traveled by burro, my grandfather has shown me old records indicating that he personally freighted much of the material for the Grünspann Expedition, pointing out that the most difficult job was keeping the bottles tightly corked!

Grandfather knew nothing of soils and made no attempt to explain his experiences, but a résumé of some recent work, superbly detailed in the *Cornell Vegetable-Crop News*, seemingly holds the key to the strange behavior so carefully documented by Dr. Albrecht and Professor Grünspann. In this paper there is a description of a new material—a soil conditioner so selective as to be called magical—so concentrated that a pound weighs just 16 ounces. This material "ERUNAM" apparently underlies the entire Schuss-yucca range. In fact, from the somewhat meager evidence now available, the whole Schuss-yucca area might be considered a solid deposit of ERUNAM.

I trust every effort will be made to hasten the compilation of the new material so that the third edition of the *Handbuch* will not be needlessly delayed.

AUGUST P. BEILMANN, *Manager*
Missouri Botanical Garden
St. Louis

AN ARTICLE that appeared in the October 1952 edition of THE SCIENTIFIC MONTHLY, entitled "The Schuss-Yucca," is of particular interest to us in Pasadena because the scene of this remarkable plant's activity (the yucca is one of the few vegetables that may be said to engage in activity) is in this city's backyard.

I therefore would like to do a feature story on the Schuss-yucca, but I don't want to wait until the growing season next spring. I'd like to request that you lend this paper prints of the photographs used in your article, and that you be kind enough to give us permission to quote from your article. Credit will be given, of course, on all material used.

You may be interested in hearing of one case of carelessness in regard to the Schuss-yucca that cost a man a promising career. A rising young pugilist, the man liked to hike as an adjunct to the endurance-building roadwork he did. Once he stopped hiking long enough to inspect a yucca at just the wrong time. Suddenly it shot up 16 or 17 feet, dealing him a terrific blow as the top of the stem caught him under the chin. Then and there he gave up the prize ring in favor of a career as a cordwainer's assistant. All he would say of the unfortunate incident was, "Any time a goddam bush can lay me out cold, I know pricefighting [*sic*] ain't for me."

E. E. SLOMAN

The Independent
Pasadena, California

THE note by Dr. Albrecht on the Schuss-yucca has just come to my attention. I cannot locate Taft College* in place or function, and therefore cannot write the author directly. Your correspondent has incorrectly placed the portion of Chilao Flat† which is the principal habitat of the plant in question. I can, however, offer partial corroboration of his observations. Several years ago my interest was aroused by the Grünspann report which he cites and I immediately repaired to

* I assume the function is not electoral and I conjecture the location to be in or near Burlington.

† This is not in the San Gabriel Mountains, but in the San Luciferos, a range that leads in quite a different direction.

Chilao Flat with my trusty Brownie‡ and several rolls of film, with the idea of making not merely time-lapse exposures, but a movie in which there should be no hint of such fakery as has made the name of Disney opprobrious in scientific circles.

A suitable prebloom specimen of the Schuss-yucca was soon located; the camera was set up, and exposures proceeded to a stage midway between Dr. Albrecht's first two figures. Unfortunately the deer§ that guided me was accompanied by a swarm of blue-tailed deer flies.‖ Just at this point one of these insects impinged upon the upthrusting apex of the Schuss-yucca. The violence of the resulting detonation obliterated the Schuss-yucca, the camera, and my own memory of the incident until the latter was just now recalled by Dr. Albrecht's paper. I trust, however, that these fragmentary observations may serve to dispel skepticism such as some of my colleagues here have expressed concerning Dr. Albrecht's investigations.

Dr. Albrecht's mention of unusually potent rattlesnakes in the Chilao region suggests further possibilities for a versatile camera. In certain wooded areas of Michigan, there occurs a mutation of the common garter snake (*Thamnophis sirtalis michiganensis hyperbola-faciens*) vulgarly known as the hoopsnake. This mutation is said to have originated from a primordial garter snake that fell from its original habitat on some aboriginal limb, and landed on its head.¶ Reports of a hoop-mutation of the Chilao sidewinder have been rife among the primitive peoples of Chilao but—to my knowledge—have never been scientifically confirmed. I recommend the problem to Dr. Albrecht's attention, but would suggest that ordinary brands of antivenin may not protect against the hazards of such research.

One detail of Dr. Albrecht's photographs bothers me. Examined with one of the newer phase microscopes equipped with 10 IQ ocular and Schlitz psychosomatic amber objective of n.a. .0025, the face of the lovely model shows greater aging between exposure 1 and exposure 5 than the cited time lapse would seem to account for. However, any doubt I may have first felt in the matter has been allayed by Dr. Albrecht's last, or escape clause, sentence.

E. E. Stanford

Department of Botany
College of the Pacific

‡ As I had in mind purely scientific observations, no fascinating model and no antivenin were included.

§ One of the quadrupedal kind, *Odocoileus hemionus californicus californicus longifrons.*

‖ The velocity of this insect was established some years ago, after some scientific controversy, as approximately 600 miles per hour. (Or it may have been per minute or per second; some chemist borrowed my copy of the paper and I cannot cite it exactly.)

¶ This sounds like pure Lysenkoism but is vouched for by any number—*any* number—of Nordic Michiganders who have never been suspected of subversive leanings.

On pages 250–52 of the October issue is found a glaring example of what can happen in decadent capitalistic countries where uncontrolled plant growth is tolerated. Such an outrageous phenomena is never occurring in our glorious laboratories where we have benefit of thought-control of that greatest of horticulturists, our magnificent leader.

I. Tinkeyiss Offenzieknutz, *Director*
Institute for Socio-Economic
Horticultural Orientation, Splonsk

To confute the skeptics still existing among the readers of The Scientific Monthly, I now release the terrible sixth photograph, which I had withheld out of deference to the American tradition of the happy ending.

The Schuss-yucca, a moment before in full and radiant bloom, suddenly withered, the blossoms turned to seed pods, and the yucca moths (*Pronuba maculata,* var. *Schuss*), which live with the yucca plant in mutualism (a type of sinful relationship between plants and animals), fell like snow, their wings and other parts singed by their rapid flight and other life processes at Mach 2! (See Grünspann, *Handbuch der Yucca,* Vol. 13, pp. 606–909. This section is devoted to the remarkable adaptation of the ordinary yucca moth to the exacting needs of the Schuss-yucca.)

It was only a moment before that the model, ecstatic with surprise and wonder at the beauty of the Schuss-yucca, had whispered sweetly some words of amaze-

ment ("I'll be damned!"). When the terrible transformation to a withered stalk ensued, she wrung her hands in anguish and, muttering incoherently some lines from a dreary poet,* fell in a heap among the dead yucca moths, and had to be revived with two jiggers of antivenin. She has been morose ever since and has taken up spiritualism.

Picayunish photographers and engineers who have incorrectly interpreted the shadows will doubtless be relieved to see them back in place in the accompanying photograph.

GUSTAV ALBRECHT

Department of Chemistry, Taft College

* No more, no more, no more,
Shall bloom the thunder-blasted tree
Nor the stricken eagle soar.
—EDGAR ALLEN POE

I WAS delighted at the hoax perpetrated by the article on the Schuss-yucca in the October issue of your journal; I was so completely taken in that I sent the article to the Royal Horticultural Society, who have now enlightened me.

I am told that after very careful examination no trace has been found of Professor Grünspann or of his book; further, the translation of the publishers' name, Schmutzig-Verlag, tends to confirm that the whole affair is a hoax.

You may also be amused to learn that this article was referred to, in all seriousness, by the Science Reporter of one of our leading national Sunday newspapers.

G. H. BEAZLEY

Nobel House
London, England

HITS AND MISSES

"Psychobiological Periodic Table of the Elements," published June 1952 in THE SCIENTIFIC MONTHLY, ignores all the disciplines of science. Its style is smug, and its content, either trivial or nonsense. This attempt to create a monster, compounded of science and metaphysics, loses itself in a pathetic morass of numerology.

It is possible the article appeared under "Science on the March" because it was thought to be suggestive of a hyperscientific approach to reality, that it hinted at some unifying theory which would include science along with the Atman, Brahma, and Nirvana.

Unfortunately, this is not the case, nor could it possibly be, in view of the author's superficial knowledge of chemistry and ignorance of the bare fundamentals of both physics and mathematics. As it stands, the article is sheer effrontery to honest workers in the field of science.

WM. BRADLEY LEWIS

Idaho Falls, Idaho

I AM taking this opportunity to congratulate you on the excellence of the August issue of your magazine. "Aimlessness in Education" is worthy of particular mention. You people are rendering an excellent service to humanity and the nation. Keep up the good work.

PAUL H. BELDING

Dental Items of Interest
Publishing Company Incorporated
Waucoma, Iowa

SINCE I have been a member of AAAS for some time, and receive THE SCIENTIFIC MONTHLY, I am taking time to comment on your August issue. For many months it was noticed that AAAS was loaded with chemistry, physics, and practice of medicine in its publications, and now it is indeed refreshing to see other sciences appear—especially comments from overseas institutions and professional writers who have something to say, other than having a paper appear in print. This "extrovert" reading in the August issue is imparting knowledge to people who no doubt are getting a new slant on AAAS publications. Since the AAAS is a sounding board for American science, I hope future issues will contain more such material. The articles on statistical administration and engineering progress and the book reviews by Mr. Flesch were about the best in editorial selection I've seen since being with the AAAS.

JOHN A. CASEY

Hempstead, Long Island, New York

I ENJOYED Bestor's "Aimlessness in Education." It was a powerful article and a perfect illustration of the disagreement of reason. Both sides of this controversy are partly right and partly wrong. We need this sort of discussion to bring out the truth. The old classical pedagogues needed a bump to stir them from their fixation on discipline for discipline's sake alone. The modern educators need restraining lest they sugar-coat discipline with immediate utility to such an extent that no lasting qualities remain. Your policy of presenting both sides is commendable.

H. H. SLOSS

Vredenburgh, Alabama

I HAVE read the item "Lost a Cow?" which appeared on page iv of the November 1952 issue of THE SCIENTIFIC MONTHLY, and I would like to call your attention to a statement which would indicate that you received erroneous information concerning this subject.

With regard to the statement that FBI fingerprint experts are cooperating with researchers at the South Dakota Agricultural Experiment Station, I thought you might like to know that this Bureau is not engaged in any research or other project in connection with cattle identification.

For your information, I have seen previous articles relating to this, and the individuals described as FBI fingerprint experts have been determined to be employees of the Office of the Attorney General of South Dakota. They have never been employed by this Bureau.

J. EDGAR HOOVER

Federal Bureau of Investigation

YOU MIGHT AS WELL NOT BELIEVE IN FAIRIES

Fenton B. Turck's article in your September issue, entitled "The Great American Explosion," affected me in much the same way that marijuana does its addicts—gravitation lost its pull and the pearly gates were opened wide. Then as I came to I began to ask myself whether the "explosion" the author describes has yet occurred or is merely in the process of making. He seems to feel that in some esoteric way we have been cashing in on our assets. This gives one a comfortable sensation, like that of being told that instead of reclining on a time fuse terminating in a charge of TNT he is resting on a bed of roses.

Here's hoping Mr. Turck may be right. But the skeptic will still inquire whether we have been cashing in on the assets or merely pledging them. If the latter—and an ever-growing astronomical national debt strongly urges that it is—then The Great American Explosion is still in the offing and is certain to be a very different one from the kind he describes.

Can it be that the savants responsible for screening contributions to The Scientific Monthly really do believe in Santa Claus? If so, the further query arises as to whether I can afford to continue my subscription.

Thomas R. Reed

Atlanta, Georgia

In reading the most interesting article by Mr. Turck in "Science on the March," in the September 1952 issue, the thought occurred that the distribution here in Northern Bavaria of a translation of the article would serve very effectively to counter the constant and insidious Communist propaganda that America is a cultureless land.

I do not know whether your policy would permit the reproduction of a translation of such an article for local distribution but do feel strongly that it would be a useful adjunct to our efforts to establish a working cultural relationship and sympathy between Bavaria and our own country. We would indeed be grateful if you were to find it possible to permit us to undertake such a translated distribution.

May I add that The Scientific Monthly is regularly received by each our seventeen Information Centers (Amerika Haeuser) in Bavaria and wears out quickly because of the demand for it among English-speaking German visitors.

Lowell Bennett

Nuernberg Regional Center
Munich Consular District

I read the editorial in the New York *Times* based on your article "American Explosion" and I was most impressed. That is just the kind of material that is hard to find, but infinitely useful in creating a true understanding in Europe and Latin America about the United States. . . . If reprints are made, please let me know, as I would like to send some to France and South America.

Edward Larocque Tinker

New York

RENDER UNTO CAESAR

The cover photograph used on your November issue was incorrectly attributed to me. Both it and the other shot showing the tank aboard the *Atlantis* were made by Don Fay for Lamont. I should be happy to have made these exposures, but since I did not I feel that Mr. Fay has been unjustly treated. The error seems to have arisen with us.

Richard E. Heningham

Lamont Geological Observatory
Columbia University

ASSOCIATION AFFAIRS

AAAS GENERAL OFFICERS—1953

ON JANUARY 15, the officers elected at the meeting of the AAAS Council in St. Louis on December 27 assumed their responsibilities for 1953. At the same Council meeting a revised constitution and new bylaws were adopted, and they will become effective January 27. On that date the Executive Committee will be transformed into the Board of Directors, which will consist of the three presidents and eight elected members as named below, plus the Administrative Secretary and Treasurer as ex officio members without vote.

The retirement of Kirtley F. Mather makes Fernandus Payne senior member of the board in length of service (elected 1946). Mark H. Ingraham and Paul E. Klopsteg were re-elected for regular four-year terms, and Wallace R. Brode was elected to complete the unexpired term of Warren Weaver, now president-elect. Among the vice presidents, Clarence E. Davies was chosen to serve a second term as chairman of the Section on Engineering. The complete roster of general officers for 1953 follows:

President: E. U. Condon, Corning Glass Works

President-elect: Warren Weaver, Rockefeller Foundation

Retiring President: Detlev W. Bronk, The Johns Hopkins University

Vice Presidents and Chairmen of Sections:

Mathematics (A): Tibor Rado, The Ohio State University

Physics (B): George R. Harrison, MIT

Chemistry (C): Randolph T. Major, Merck & Co., Inc.

Astronomy (D): Bart J. Bok, Harvard College Observatory

Geology and Geography (E): Wilmot H. Bradley, U.S. Geological Survey

Zoological Sciences (F): Paul Weiss, University of Chicago

Botanical Sciences (G): Edgar Anderson, Missouri Botanical Garden

Anthropology (H): James B. Griffin, University of Michigan

Psychology (I): Frank A. Beach, Yale University

Social and Economic Sciences (K): Lowry Nelson, University of Minnesota

History and Philosophy of Science (L): Richard H. Shryock, The Johns Hopkins University

Engineering (M): Clarence E. Davies, ASME

Medical Sciences (N): Cornelius P. Rhoades, Memorial Center for Cancer Research

Agriculture (O): K. S. Quisenberry, U.S. Department of Agriculture (Beltsville)

Industrial Science (P): Francis J. Curtis, Monsanto Chemical Co., St. Louis

Education (Q): Donald D. Durrell, Boston University

Members of the Board of Directors

Detlev W. Bronk (*Chairman*), The Johns Hopkins University

Wallace R. Brode, National Bureau of Standards (1953)

E. U. Condon, Corning Glass Works (1952–54)

John R. Dunning, Columbia University (1952–55)

Walter S. Hunter, Brown University (1951–54)

Mark H. Ingraham, University of Wisconsin (1953–56)

Paul E. Klopsteg, National Science Foundation (1953–56)

Fernandus Payne, National Science Foundation (1950–53)

Paul B. Sears, Yale University (1951–54)

Laurence H. Snyder, University of Oklahoma (1952–55)

Warren Weaver, Rockefeller Foundation (1953–55)

Howard A. Meyerhoff (ex officio), AAAS

William E. Wrather (ex officio), U.S. Geological Survey

PUTTING SCIENCE TOGETHER

THE annual meeting of the Association has the reputation of being the biggest scientific show on earth. In respect to size this is not so, but from several other standpoints the AAAS may as well modestly admit it. No other scientific event brings the accomplishments of scientific research so forcibly to public attention, thanks to thorough and brilliant coverage by the science writers of the country. No other meeting brings so many experts in diverse fields together to explore the fertile areas in which disciplines meet and overlap. In no other convention do so many organizations cooperate in the exploitation of collective experience. The phrase that captured popular fancy in the Association's Arden House statement of policy—"to put science back together again"—is merely a description of what has been going on in a quiet way for several years.

The annual meeting overshadows other AAAS-sponsored meetings, but the others may appropriately be mentioned. It will be evident that the Association gets around, not only in this country, but abroad, for last year it was officially represented at meetings in every continent except Asia.

In May the Southwestern Division met with the Colorado-Wyoming Academy of Science at Boulder, Colorado, and 457 scientists in the Rocky Mountains, Great Plains, and Intermountain states assembled to discuss regional, but by no means provincial, contributions to the advancement of science (SCIENCE, **116**, 135 [1952]). In June the

Pacific Division played host to 17 affiliated and associated organizations at Corvallis, Oregon (SCIENCE, **116**, 407 [1952]), and 1095 scientists, mostly from Western states and Canadian provinces, demonstrated a vigorous interest in, and prosecution of, scientific and technological research. From mid-June to early September, approximately 2000 specialists gathered in a succession of weekly Gordon Research Conferences at Colby Junior College, New London, New Hampshire, and at the New Hampton School, New Hampton, New Hampshire, to consider the new discoveries and new techniques in chemical and related fields. In September, the Association's sections on Engineering (M) and Geology (E) met with the Industrial Minerals Division, American Institute of Mining tnd Metallurgical Engineers, at the Centennial of Engineering in Chicago to consider the raw materials survey of the Chicago region, structural materials, and the recovery of ground water for industrial use. Also in September, at McKinley National Park, the Alaska Division sponsored the Third Alaskan Science Conference, bringing together more than 200 specialists who are systematizing and solving scientific problems indigenous to our northern territory.

The Association also has five active branches that carry on its work within restricted areas. The organization of the Alaska Division is unique in that it consists of three branches—Cook Inlet, Arctic, and Southeastern—each organized around a center or nucleus of scientific activity. Within the continental United States branches have not been regarded as an essential element in AAAS structure because of the work of state academies of science; yet the Lancaster (Pa.) Branch functions in a state with an active academy, bringing science and scientists to a community audience numbering 500–800. Only one other branch is currently active—Springfield (Mass.), which is operating in a section where state academies are scarce.

The Association has been officially represented at scores of other meetings and ceremonies. On some of these occasions the AAAS representative was an active participant—for example, Detlev W. Bronk gave the exchange lecture at the Belfast meeting of the BAAS in September. Enough has been said to indicate that the Association exerts a continent-wide influence in its meeting activities. From all indications this influence is destined to spread and to give American science integration and direction.

THE AAAS AT ST. LOUIS

The **Newcomb Cleveland Prize** (formerly the AAAS $1000 Prize) was awarded to A. M. Gleason for his paper on "Natural Coordinate Systems," given at a meeting of the American Mathematical Society in St. Louis, December 27. Dr. Gleason, 31 years old, is an assistant professor in the Department of Mathematics at Harvard University.

The **AAAS Council** paid tribute to the memory of two scientists who were long active in Association affairs. On December 30 the following minutes were passed unanimously by standing vote:

EDWIN GRANT CONKLIN
1863–1952

A leader in the biological sciences, Edwin Grant Conklin profoundly influenced zoologists and biological thought during his long and inspired career as a teacher at Princeton University. His influence went far beyond the classroom, extending to all American science through his activity in the American Association for the Advancement of Science, of which he was President in 1936, and to all American scholarship in his long continued and devoted work for the American Philosophical Society. His inspiration as a teacher, a leader, and a friend has been bequeathed to the many students who discovered either a career, or a philosophy of life, or both in his classroom.

FOREST RAY MOULTON
1872–1952

Astronomer and pioneer in the problematic fields of celestial mechanics and earth origin, Forest Ray Moulton was a scientist, teacher, and philosopher who, though bound by facts, brought a creative imagination to bear upon scientific thinking and upon the relations between science and society. As Administrative Secretary of the American Association for the Advancement of Science during 1937–49, he brought order and integration to one of the nation's oldest scientific societies and, through the Association, to all of American science.

Arnold M. Rose, of the Department of Sociology, University of Minnesota, was awarded the new AAAS $1000 prize in social science theory, which was established in 1951, but was given for the first time at the AAAS meeting in St. Louis last month. Stuart C. Dodd, director of the Washington Public Opinion Laboratory, University of Washington, received honorable mention. The prize was given to Dr. Rose for his paper "The Theory of Social Organization and Disorganization," on the basis of independent ratings of nearly 60 entries by three judges—George A. Lundberg, University of Washington; Kenneth H. Parsons, University of Wisconsin; and Sidney Ratner, Rutgers University. The competition will be continued in 1953, and announcement of the conditions for this year's prize will be published on or about March 6 in SCIENCE.

The **Theobald Smith Award in Medical Sciences,** consisting of $1000 and a bronze medal, was given to F. J. Dixon, of the Department of Pathology, University of Pittsburgh School of Medicine, for his paper entitled "The Dynamics of Immune Response." The award, which was established in 1936 by Eli Lilly & Company, was given for the eighth time this year. Only scientists under 35 years of age are eligible for the prize.

THE SCIENTIFIC MONTHLY

FEBRUARY 1953

Education for Survival

CORNELIS W. DE KIEWIET

Born in Rotterdam in 1902, the author spent his childhood in Johannesburg. He was educated at the universities of Witwatersrand, London, Paris, and Berlin. After teaching at the University of Witwatersrand and in Salisbury, Southern Rhodesia, he came to the United States in 1929, where he joined the staff of the Department of History of the State University of Iowa. In 1948 Dr. de Kiewiet went to Cornell as professor of modern European history and became, successively, dean of the College of Arts and Sciences, provost, and acting president, until called to serve as president of the University of Rochester in 1951. His article is based on an address given during the AAAS St. Louis meeting last December, at the Conference on Scientific Manpower, jointly sponsored by Section M, the AAAS Cooperative Committee on the Teaching of Science and Mathematics, and the Engineering Manpower Commission.

THE investigation which has been proceeding for a number of years into the nature and distribution of talent and skill in American society has given me a feeling of excitement and satisfaction. We are beginning to lay bare the anatomy of our skilled manpower. It is urgent that these studies be further pursued and refined. It is even more urgent that no opportunity be lost to draw public attention to these findings, and to stimulate a discussion of them. Wherever there are faculties or legislatures or other groups who have a stake in quality or productivity or efficiency, these findings and their meaning should be brought to their attention.

Education is especially indebted to the careful research into our national manpower. We have acquired a most effective reinforcement of the claims of higher education through this dissection of the body of national proficiency and competence, disclosing leanness and occasional fatness, constrictions and disproportions, inadequacy and new opportunity. The nourishment of ability which we call education is being freshly justified and stimulated. This is service of a high order. No faculty or legislature or foundation can properly be indifferent to the meaning of the findings that are being reached.

As in some of our natural resources, so now in our human resources, the time has come for a more precise accounting. We can no longer afford to waste or neglect available resources of human skill. It has suddenly become clear that as a nation we are still too casual, too inexpert, too wasteful, in our attitude toward brains and ability. Because

we have never lost a war, because we are almost continental in our extent, because more than any other people we have turned the raw earth into a flood of food and goods, we are in real danger of living in a world of false proportions, and of acquiring the delusion that we can always be adequate to the tasks thrust upon us. Today we must accept new magnitudes and make new comparisons. In the days of our relative isolation the millions of our population seemed more than sufficient by the side of the great Western European nations with which we compared ourselves. In fact, we considered ourselves so well endowed that, in an ungenerous immigration policy, we applied an embargo against the most precious of all commodities—human beings. For our lack of imagination we are today paying a heavy price.

New magnitudes and comparisons have suddenly been forced upon us. The rise of Russia and the collapse of the British position in the modern world have rent the veil which a sense of remoteness and security had placed between America and the masses of Asia, Eastern Europe, and Africa. Today American life is intimately confronted by the greater magnitudes and multitudes of China, India, Russia, and the Arab world. Even when we apply to these proportions the correction represented by differences in health, longevity, education, and industrial power, it is still evident that the comparisons and proportions of the contemporary world cause deep disquiet. In its world America is a minority group.

A characteristic error has already crept into the debate on manpower. This is the assumption that problems can be solved by legislation and governmental agencies. Federal control of education and federal direction of skilled manpower policies are not very different from one another. There is no substitute for a far-ranging national concern. There is still a great deal of ignorance and indifference even in universities and colleges about the skilled manpower needs of the country. It is false to assume that with an ample supply of students, higher education can therefore undertake to provide an adequate flow of trained people who will be reasonably well distributed in those areas where they are most urgently needed. Neither the social sciences nor the humanities have done nearly as well as the physical and natural sciences in adapting teaching and research to the new context in which American society has its being. In the environment of modern America, we must continually remind ourselves, are the new proportions of Russia, China, and India, the confusion of the buckling imperialisms of Western Europe, an ominous power vacuum in the Indian Ocean and the Middle East, and an unexpectedly early insurgence in Africa and the Arab world. Yet it cannot be claimed that our curricula have sufficiently modified the traditional emphasis upon the life and thought of the North Atlantic rim in order to pay due attention to the new "factors of modern history." Even those centers of study in Southeast Asia, China, or the Middle East which the foundations have providently helped to establish often suffer from a polite neutrality with which faculties isolate them from the main currents of teaching. It is urgent that there be a proper balance between the various skills by which a nation is made strong. It is therefore equally urgent that each of the great disciplines see that it is ready to perform its task.

A major part of the discussion on skilled manpower has been concerned with science and technology. This is understandable, especially if we assume that war is imminent and inevitable. The requirements of war make a balanced and farsighted manpower policy excessively difficult and probably impossible. Beyond military conscription itself, there must be a vast commitment of trained men to the machines that sustain war. To build a modern jet plane, over one hundred times as many engineer hours are required as were necessary twelve years ago. An hour's flight by a jet airplane demands forty hours of ground maintenance. The more complex the machine the greater the tail of skills which, like a comet, it must bear behind it.

American national policy, however, still assumes that war is not inevitable. Wisdom dictates two sets of concessions in order to establish a reasonable balance between the skilled manpower needs of a nation facing the possibility of war and the manpower needs of a nation that cannot abandon its deep hope for peace. There must be trained soldiers, a flow of engineers, assistance for essential industry, the sponsorship of scientific research. But if we are to cope with historic tasks comparable only with those shouldered by Rome or Great Britain at the height of their power we need a supply of knowledge and experience far beyond present levels. Even a world war, if that must be our fate, needs men who know geography, languages, and historical traditions. Although we might explode our way through to a military victory, illiteracy in these things would, as has happened before, turn the greatest victory into defeat.

The simple equation of a skilled manpower policy with the flow of soldiers, scientists, and en-

gineers is both wrong and dangerous. The total policy of a nation is too important and complicated to be guided solely by the recommendations of generals and engineering deans. We could perhaps listen more readily to their statements if the physical force we now possess or can readily develop were really the measure of our power and influence in the modern world. Never in modern times has a nation possessed so much physical power and yet been so baffled in the conduct of its foreign policy. The vast sum of atomic bombs and jet airplanes, of guided missiles and supercarriers and retired generals, cannot release a single American citizen from a Czech prison, or secure a visa for an American scholar to visit Russia. (The outstanding discovery of the past few years was made by a journalist named Oatis. He discovered the powerlessness of the atomic bomb.) Incomparably the most remarkable phenomenon in the modern world is precisely this lack of any equation between nuclear fission and the forces of history. A study of the experience of France or Great Britain in the past twenty-five years makes it plain that the compulsion to mobilize the total power of a nation in war is itself a defeat which an ultimate military victory cannot redress. Great Britain did not win the first world war in 1918. She lost it in 1914. To lose the peace is the great disaster. It is of the utmost importance for us to recognize that the pressure of events in the Arab world, in India, China, and even in Africa, cannot be controlled or guided in any chosen direction by the traditional forms of military action. Nor can the United States of America follow the old logic of imperialism which the Dutch followed when they took the place of the Portuguese in the seventeenth century, and which the British followed in the eighteenth century when they took over colonial control from Spain and France. The forms of action and influence developed by the colonial powers of the eighteenth and nineteenth centuries are at the same time inaccessible and unacceptable.

If our national policy is, rather, the avoidance of war than a fatalistic preparation for war, if it means more to us to be harmoniously associated with the great new movements of the world's peoples, then we must labor to find, by the side of the equation between skill and force, a further well-supported and generous equivalence between skill and the ability of our best minds to think and work constructively in a critical world.

It is not a shortage of engineers or atomic physicists that explains the bankruptcy of our China policy. If we have disastrously erred in the Far East, it is partly because American society had so few experts on China to whom it could turn for knowledge and counsel. Even of these few only a pitiful fraction was competent in knowledge or wise in counsel. At this moment we can only guess at the other disasters which ignorance and inexpertness are preparing for us.

No matter where we stand in this issue it is entirely clear that quality must make good our insufficiency in numbers and that we cannot afford to allow any important part of our potential skills to be undeveloped. The security and welfare of the many depend on the skill and training of the few. Therefore our small pool of talent must receive special consideration. Obviously needed in the first place is a fresh examination of the draft upon skilled manpower. Education itself, has by far the greatest power to elevate the level of talent. Somewhere in the combined eight years of high school and the undergraduate college there is the most serious loss of time, talent, and energy. Almost certainly an entire year could be salvaged in the case of the brightest students. In fact, the equivalent of two semesters out of the four college years are absorbed by ROTC programs. It has been estimated that almost one half of the 1951 eighteen-year-old group who stood in the top quarter in ability did not go to college. If but a third or a quarter or a fifth of the alert minds that somehow fail to finish high school, or fail to go from high school to college, could be salvaged, the relief would be important. From these facts one conclusion is inevitable. Education must work toward a revival of the close connection of two generations ago between high school and college. The great complex modern high school system serves a variety of goals. If there is any statesmanship in education we must move closer to the conviction of fifty years ago that a primary function of high schools is training for college and university. The links maintained by admissions offices and departments of education are today insufficient to provide for the discussion and cooperation that are needed. A greater effort must be made to see the entire eight years of high school and college education as a continuum. The rigidities and exclusions inflicted by high school systems upon college, and perhaps in greater degree by college upon high school, must be replaced by a bridge of cooperation and understanding between both. There is no need to enlarge upon the baffling isolationism of high school systems or the hauteur of college faculties. One of the most rewarding fields for founda-

tion enterprise would be in this wasteland between high school and college, this limbo in which linger the immature talents of so many young people.

We in the colleges are not in a good position to lay blame on the high schools, for we cannot claim that in admissions and counseling, in the curriculum and the use of student time, we show a full appreciation of the critical bearing of higher education upon a national manpower policy.

It is probably true that we lack the wisdom and courage to revise our ungenerous immigration policy. The effect of an intelligently supervised immigration policy would be meaningful. Skill comes at bargain rates on Ellis Island.

It had better be understood we have no great new reserves upon which we can draw beyond this point. The minority groups and other underprivileged or underemployed sections of the community can bring some relief. Those who, like myself, believed and hoped that women still provide a large untapped reservoir will be disappointed to learn that this may not be true. A number of responsible studies suggest that, alongside a natural preoccupation with family life, there seems to be a lower intensity of ambition among women and a lesser willingness to accept the strenuousness of the higher positions of responsibility and effort which set limits upon the share of women in the present emergency. What has been called the secondariness of women is a condition most difficult to analyze and explain. The difficulty in American society of obtaining favorable adjustment in the body of assumptions and pressures which govern the place of women in society is too great to be handled in this discussion. An atomic bomb on Hollywood and Tin Pan Alley would help.

In the final analysis there is no substitute for the qualitative development of our best brains. Our foreign and military policy has no better ally than the educational system. In any assessment of American power, higher education has the same stature as our system of food production, our industrial organization, or our system of defense. Yet education at all times has greater difficulty in presenting its case than any other essential activity. It is easier to assign a portion of our income to digging oil wells than to stimulating a greater flow of our underdeveloped intellectual resources. The deterioration in the past decade in the financial condition of the colleges seriously limits their ability to make the most effective contribution in "upgrading" American manpower. Lack of adequate financial resources means too few scholarships for able but impecunious students. Often it means understaffed faculties. Far too often it results in stifling academic enterprise that is necessary to train specialists in new fields. A number perhaps as great as 150,000 young men and women a year is condemned to a lower level of attainment and skill because of the indigence of higher education.

Those who are the greatest consumers of high-level skills have up to the present been little moved by the national damage caused by the insufficient financing of higher education. With exceptions that are encouraging, the industrial leadership of America has been deficient in recognizing the role of higher education in maintaing the flow of skills on which progress depends. True statesmanship would cause industrial management to encourage universities by whatever means are in their power to support the indispensable tasks of recruitment, training, and placement which they perform.

A special comment is necessary on the gross and damaging error of those men who refuse their material and moral support to the total life of our universities because of disagreements with political and economic views held by individual scholars. Admittedly, faculties have not always been wise or tolerant in understanding the anxiety from which the modern generation suffers. Academic freedom only sometimes leads to wisdom, and never to infallibility. But the errors of such as these are trifling in comparison with those who expose to ridicule the life of scholarship and who inflict an undiscriminating punishment upon all for the sin of a very, very few. The time has come for universities to speak out in clear anger against the continuing sabotage of intellectual enterprise. Without a wide area of free intellectual activity the ideal of a quality manpower cannot be attained. Those who hurl their loose charges of subversiveness against higher education must be told that they are endangering the activities which have made America the center of medical education in the world, which provide the stream of scientists and technologists without whom industry would sag and collapse, and which, above all else, have through the generations clarified and maintained the ideas and practices of a living and successful democracy. To those who accuse the colleges of being centers of subversion let the answer be courteous but emphatic. American colleges are not centers of peculation and corruption because some of their graduates enter politics and plunder the public purse. In this country it is the marvel and the triumph of higher education that

so few of its teachers and graduates have been clearly and basely faithless to their society. To those who plan to "investigate" the colleges it should be explained that there is no activity more blameworthy than to torture the dignity and destroy the confidence of those who think and write and teach. This explanation must be made for several important reasons. The highest function of education is to make human experience contemporary; that is, to make it available for use in the life of a man or a nation. Yet a great body of scholarship remains antiquarian, and many scholars withdraw into a grammarian's funk hole, because this is safer than to deal with the great issues of the age. As a result society is the poorer for want of the wisdom and the understanding that can only come from scholarship. At this point a very ancient piece of human experience must be made contemporary. The anger and the disaffection of the intellectual, once aroused, are a sword against which neither the purse of the rich nor the law of the mighty can ultimately prevail. A great society never declines but the signs are first plain in either the indifference or the hostility of its intellectuals. When frustrated men crucify scholars for not giving correct or pleasing answers to some of the most difficult problems of all history, then our voices must be raised in defense of the learning and patience, the conscience, and the love of man which characterize scholarship at its best.

There are orders of skill and expertness to which no ordinary measures apply. They are beyond the reach of specific training and scholarly method. Wisdom is the power of seeing things as they really are, and of counseling men to choose those actions which increase the total of peace and justice and charity in the world. Genius is the quality of the special spirit, whether in poetry or politics or science, which raises a man above a single locality or nation to influence the people of the world. To wisdom or genius we can assign no price that any purse can pay. Nor can we devise any curriculum guaranteed to produce them. All we know is that they are likely to arise in an atmosphere where thought and learning are held in honor.

A national policy, whatever its form or emphasis, cannot be successful without the understanding and consent of the people. This is particularly true in a prolonged and restless period of crisis. By far the outstanding phenomenon in the recent Presidential election was anxiety—anxiety about war, anxiety about inflation and high taxes, anxiety about corruption and treason, anxiety about Washington's stunted and disorderly silhouette, anxiety of parents about their sons, anxiety of young people about the future. To the extent that anxiety breeds withdrawal, slackens the will or corrodes confidence, the election revealed that Americans are uncomfortable and unsure in their postwar environment. It suggests that we have not yet done well enough in educating the American people in a knowledge of the turbulent world into which they have been thrown, that we have not done well enough in presenting the national interest as superior to the special interests of the diverse components of the nation, that we have not done well enough in convincing them that, in the proportions of today, Americans are a minority group, which can maintain itself most surely by making quality a rule of life. This quality goes beyond skill and training, though these are vital. There is a list of qualities which men in more religious days evoked with familiar approval, but some of which have grown pale in a more materialistic and incoherent age. They are the recognition that the soul and greatness of a nation are the people's trust, and that there can be a sacredness about being a nation if its policies are guided by charity as St. Paul meant it. They are the knowledge that work is not man's punishment, but the source of his power and achievement. They are, once again, the sanctity of human life and the persuasion that the improvement of the conditions of human life is our greatest new frontier.

The entire debate on skill and training is incomplete unless we see that Pestalozzi's trinity of hand, mind, and heart is indispensable to a manpower policy that stresses quality. A brilliant and sufficient cadre of engineers is surely less effective if New York's waterfront is ruled by brigands. A high level of training in economics is less meaningful if labor and industrial leaders cannot learn the lesson that, although the whole is never healthier than its parts, it is always greater. A stockpile of atomic bombs has less power if sections of the population are blocked by prejudice or made stagnant through ignorance. Health and long life multiply the effectiveness of the body. In the same manner a desire to work and work well multiplies the skill of mind and hand. A national atmosphere of consent and congeniality multiplies the willingness to produce and to cooperate.

The greatest skills we need are not in science or engineering, but in human relations. When I say this I am thereby asking that the opportunity be not reduced for history or philosophy or literature

to speculate upon human relations. Skill in the conduct of human relations is being slowly born and cannot be hastened. It cannot be bought with two billion dollars, or achieved by setting up a Los Alamos of the social sciences and the humanities. But if we do not defend the thought that deals with human life and its values, we may have to study the atomic bomb through our tears before we learn what these values are—that a man or a people has the right to stand in a dignified relationship to others, that the death and suffering of human beings in Uganda or China or Peru must cause sorrow in Chicago or Rome or Tokyo, that in today's world consent and congeniality are as greatly superior to coercion and conflict as was the sense of human community of the New Testament over the tribalism of the Old. Unless these are also goals for our skill and training, then for what are we asking our sons and daughters to live and perhaps to die?

The highest possible correlation between talent and national welfare is imperative. This is true whether we think narrowly and desperately of the power and skill needed for war, of the grim competition in which we are engaged for approval in the eyes of the world, or more nobly of man's task of making the future his everlasting frontier.

Lotus-eaters in a hungry world we have never been. The "blight and famine" of today's world, its "clanging fights and flaming towns" are not to us "a tale of little meaning." We recognize that their pestilence is our pestilence, as their peace must be our peace. The powers that attended the creation of the earth hover over us again. Whether they have come to destroy or to create is the answer we seek.

TWO VIEWS FOR A PARALLAX

Searchers-of-the-heavens,
Measuring for stellar distance
From the dome-capped hills,
Sight to Alpha Centauri, and again
Turn to that fiery splendor
When the earth
Has moved halfway around its revolution
Of the sun.

How all impossible
From one view only
To judge the far projection
Of a world, or thought!
From this one garden I might scan
The sky, and cry:
Alpha Centauri is
A little light, eclipsed
By those high plume-tips
Of the deodars!
But with two different views
For this observing,
Two separate eyes for seeing,
We obtain a rounded
Three-dimensional perspective
Of man-centered patterns,
And a heliocentric parallax
For stars.

GEMMA D'AURIA

Hollywood, California

Herodotus on Biology

L. P. COONEN

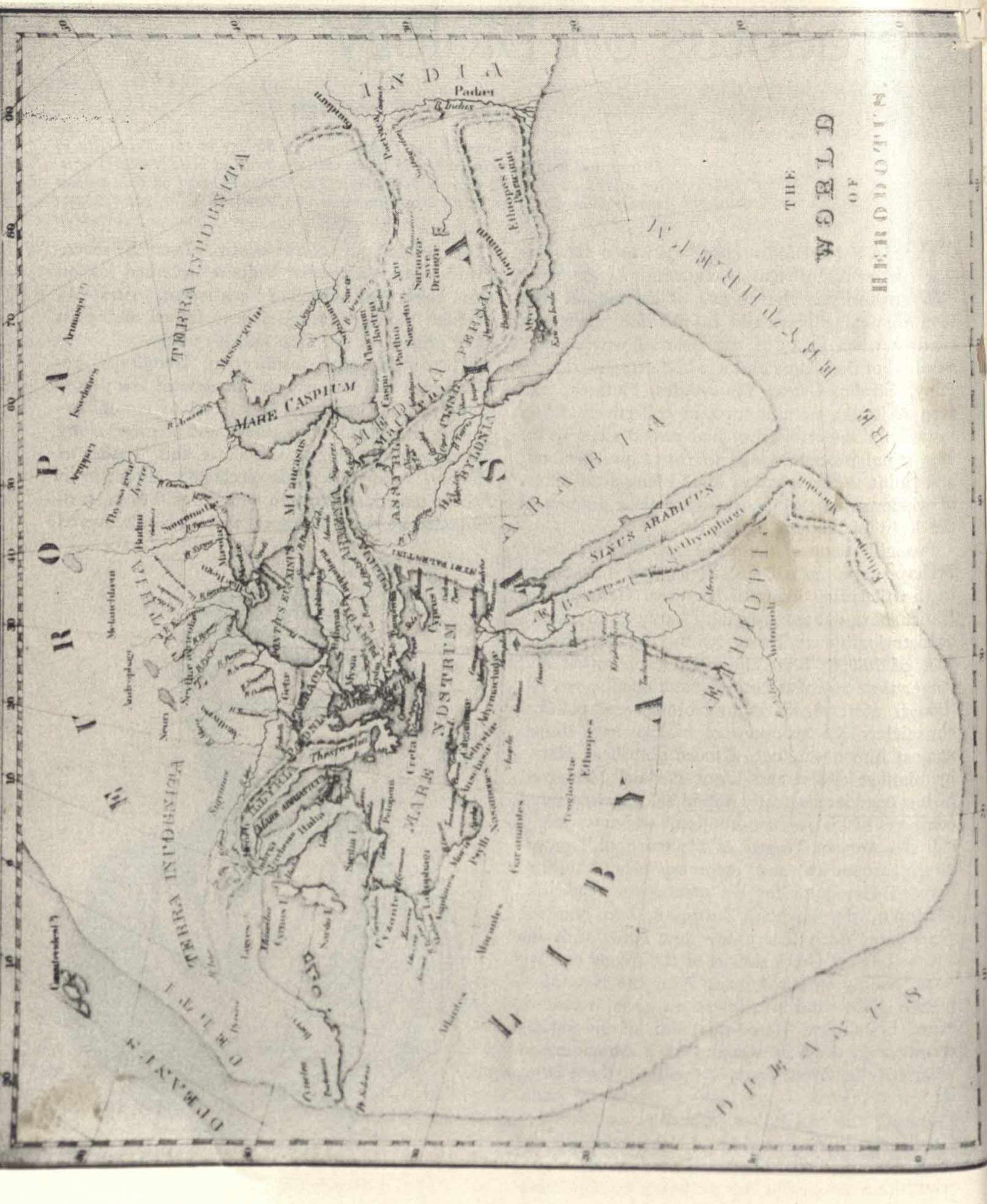
Dr. Coonen, who is head of the Department of Biology at the University of Detroit, has long been interested in the prehistoric roots of biology and is writing a book on biology's obscure beginnings. Two chapters based on this project were published in The Scientific Monthly (**73**, *154* [*1951*]; **74**, *211* [*1952*]).

THAT Herodotus (Fig. 1) was a liar has been said often and vehemently by too many historians. The chorus of defamation rose to a deafening crescendo in the late nineteenth century when many of the classical writers were stripped of their glory and left in defenseless shame. Pliny, Strabo, Galen, Thucydides, Plutarch, and even Aristotle, were in turn, or collectively, taken to task for the errors they had passed along to us. The usual procedure was to ferret out, dust off, and point to the mistakes of the long-dead pillars of wisdom and then to castigate their authors in the light of modern standards.

Probably none of these ancients was malicious. Perhaps none was particularly careless if examined in the flickering lights of his time. Herodotus of Halicarnassus fared especially badly at the hands of historiographers. He enjoys the negative distinction of having been chastened more often and longer than any of the others mentioned above. Although most of his denunciators were political chroniclers, the historians of biology have sinned against him in another, a more diabolical, way—by blanket elision and silent disdain. Moreover, he has been intellectually sniped at by the martyred confreres of his own communion,[1] whom we might call the Ancient Targets of Malediction. Thucydides, compatriot and contemporary historian, derided Herodotus for his spectacular reporting; Manetho, the Egyptian historian, two hundred years later dealt him a few low blows with his stylus; Lucian, Greek satirist of the second century A. D., openly criticized him; Pliny the Naturalist, with a giant wind instrument on his own coat of arms, besmirched Herodotus and all his fellow-countrymen when he wrote: "It is astounding to what lengths Greek credulity will go; there is no lie so shameless as to lack a supporter;" and Plutarch, the biographer, moralist, and brother-Greek who is often considered a distinguished and pre-eminent member of several ancient liars' clubs, paid him a unique but oblique honor by composing "On the Malignity of Herodotus." Even the seventeenth century's acerbic Voltaire[2] splashed ink on this celebrated Grecian gentleman. And so, through the ages, tongues have lashed and pens have pricked at the man and his works.

There was some justification. Herodotus, remember, was a pioneer, and there were few precedents in historiography. The road to factual reporting was filled with potholes, and he raced along oblivious of the bumps. Of course, this "Father of History," who recited his spectacular chronicle in Athens sometime between 446 and 430 B. C., promulgated no great biological theory; he discovered

Fig. 1. Herodotus. (From Rawlinson.)


THE WORLD OF HERODOTUS
EUROPA
ASIA
LIBYA
INDIA
ARABIA
PERSIA
BABYLONIA
MARE CASPIUM
SINUS ARABICUS
MARE NOSTRUM
PONTUS EUXINUS
TERRA INCOGNITA
MARE ERYTHRAEUM
OCEANUS

no important law; he recorded no shining gems of minute observation. But biology did not begin in that fashion. In its peculiar origin he was a force. Herodotus was one of the early collectors of the nondescript, amorphous body of scattered, unclassified items. Theories, detailed descriptions, and laws came with biology's adolescence, not its infancy. And Herodotus, although not a biologist, should be identified with the incipience of the science because he charted some of the feeble creeks that later were to flow into the streams and torrents and broad rivers, eventually into the ocean, of life sciences.

Herodotus was a restless traveler when traveling was a hardship and an adventure. He was an omnivorous reader when writings were scattered and in diverse tongues. His source materials were gathered, many of them on foot, from dozens of countries and over a latitude of thousands of miles (Fig. 2). He was a historian virtually without a bibliography; he was a research writer without a library; he wrote, before archaeology was born, on happenings remote in time. He was unilingual,[3] but he ran headlong into a babel of languages. He was a tireless writer, when writing was a servile task for the patient and the energetic. But, above all, he was a listener. This was both good and bad for him and his works. We can epitomize him in the vernacular: he had a big mouth, but he had ears to match.

Just as we must absolve Homer, Virgil, and Ovid for vagaries in science and telescoping in history, because these men were primarily poets, we must excuse Herodotus; he was a literary man, a moralist, and a reporter, and perhaps only secondarily a historian. In no wise was he a scientist.

Much of what he heard and reported must have caused him some mental indigestion, for he often set it down in writing with an expressed reluctance. On many occasions we find his interjections of embarrassment: ". . . the Libyans say, I do but repeat their words" (IV, 173); ". . . I only repeat what is said by the Libyans" (IV, 187); "If this be true, I know not; I but write what is said" (IV, 195); ". . . my duty is to report all that is said. . . . I am not obliged to believe it all . . ." (VII, 152); ". . . and some people will perhaps believe them, though I for my part do not . . ." (V, 86); and ". . . to me it seems quite unworthy of belief . . ." (VIII, 119).

FIG. 2. The world map as probably visualized by Herodotus—the various countries and tribes are mentioned in his writings. Some of the tribal names suggest certain biological problems: Icthyophagi (fish-eaters); Androphagi (cannibals); Lotophagi (lotus-eaters); Bosporus (cow-crossing or ox ford). Herodotus visited the general areas immediately surrounding the Mediterranean Sea, shown in the approximate center of the map, and he reported legends, true or fanciful, about the other regions. (From Rawlinson.)

From our armchairs, however, it seems that Herodotus misplaced or omitted several apologies. Some are associated with statements since proved true; other declarations, of the wildest variety, were not mitigated either by prefixed partitives or suffixed doubts. Because his curiosity worked like a vacuum cleaner and because he had the interest and ability to describe peoples and customs, his history of the Persian wars is a unique and interesting reservoir of information containing some molecules of biology that might be examined with profit and amusement.

Animals

Herodotus' reportings of animal lore have been of interest to writers in all ages and have often been copied. His story of the crocodile is an example. It has no tongue, said Herodotus; actually, the tongue is small. It eats nothing during the four-month winter, he continued, approaching the truth; the crocodile eats little in winter. He said further that it cannot articulate its lower jaw and that it is blind under water—wrong in both cases. But the great Aristotle apparently borrowed these errors[4] and left his reputation open to many a pot shot because of them. The hippopotamus, too, was a trap for the imaginative writing of Herodotus: he was wrong about the cloven hooves (it has toes), the size (he made it too small), the mane and tail (not like those of the horse). Again, Aristotle seems to have inherited the error of size:[1] no larger than an ass, he said, much later.

On another occasion Herodotus recorded the spectacular and was quite correct. The bulky tails of two kinds of Arabian sheep excited in him an admiration that is reflected in his description of them. One type of tail, he said, is so long and heavy that it is supported and protected by a wooden truck. Similar protective devices are employed today. His digression into the osteology of the camel brought forth some anatomical confusion, for he allocated four thigh bones and four knees to the hind legs. The camel's long metatarsals are partially responsible for this error.

His description of the appearance and habits of Egypt's sacred bird, the ibis, is very accurate. However, the mostly white ibis, not the black one, was sacred.[1] Because the ibis was said to give itself an enema,[5] it was identified with the highly en-

dorsed medical practice of frequent purgations. Hence the bird was considered very gifted, and even god-linked. This protected fowl was a nuisance in the streets of Alexandria[6] and certainly a sartorial hazard. All this special attention probably underwrote the accuracy of its description.

The phoenix, an ancient ornithologist's dream (Fig. 3), was likewise carefully described by Herodotus, but he admitted that he had never seen one. The Syrian philosopher Porphyry[1] says that Herodotus filched this description from his Greek predecessor Hecataeus. This myth-on-wings was supposed to have a life span of 500 years in Egypt; then it returned to Arabia to be voluntarily consumed in fire. Its life cycle began anew when it rose fresh and frisky from its own ashes.

Otters and leeches are described as occurring in the Nile, but according to How and Wells[1] neither animal is found there. Snakes were the subject of various comments and observations. He reported winged snakes in Arabia, and "little snakes, each with a single horn," in Libya. The latter could well have been the smooth, three-inch caterpillars of one or more of the hawk moths. And, as an accurate item of herpetological observation or reporting, he noted in general that some snakes are oviparous, and others ovoviviparous.

Herodotus' story of fish fertilization is less acceptable. During the spawning season females follow the males, he wrote, and swallow the sperm. Fertilization somehow resulted. Aristotle saw the fault here, corrected (in his *Generation of Animals*) his predecessor, quoted his misstatement, mentioned his name;[7] and fixed the record for all time.

That the oxen of Scythia have no horns may be true, but when Herodotus attributed the polled feature to the low temperature he admitted that he leaned upon Homer (*Odyssey*, IV, 85) to support his contention.

On occasions, such as in Book IV (191–192), where he listed the fauna of Libya, our ancient author rattled off long lists of animals. We learn of huge serpents, but not so large as those reported by Pliny, who called the Greeks liars. The Roman reported in his *Natural History* (Vol. VIII, 37) that a certain 120-foot serpent skin was brought to Rome! Herodotus listed other Libyan animals: lions, elephants, bears, asps, horned asses (a kind of antelope?), antelopes, gazelles, buffaloes, asses ("not of the horned sort"), oryxes, foxes, hyenas, porcupines, wild rams, dictyes, jackals, panthers, boryes, "land-crocodiles," two-legged mice (jerboas, with reduced front legs), another mouse called "zegeries," and "prickly mice." The commentaries on Herodotus[1, 2] identify most of these, but not all.

Insects, too, received some attention. This extensive traveler, often enduring primitive transportation and shelter, met many of them in his reluctant passivity. He reported that the mosquitoes were so bad that the Thracians could not inhabit certain areas of their country. And the gnats of Egypt forced the citizens to build high towers to get above them.

The best entomological tale is the much-repeated story of the gold-digging ants of India. Swift's Gulliver could not outdo this whopper, probably first "reported" by Herodotus (Book III, 102, 105). In size, he said, they were ". . . less than dogs, but bigger than foxes." They burrow underground, throwing up sand which "is full of gold." The ants "are so swift . . . there is nothing in the world like them." The tale picked up momentum and inflation as it was mouthed by historians and "witnesses."[1] Nearchus, Alexander's admiral, "had seen" the skins of the "gold-digging ants," which are "like those of panthers;" Strabo embellished the story from several sources; Megasthenes described the plain that the ants inhabited. Prester John's imagination, in the twelfth century, equipped the beasties with seven legs and four wings; the sixteenth-century Flemish traveler de Busbecq related that the Persian ambassador presented an Indian ant, *"magnitudine canis mediocris, animal mordax et saevum,"* to the Sultan of Constantinople. Because this giant hymenopteran was "biting and raging" the scene is realistic. *Somebody* "saw" it, but Herodotus seems to have been the grandfather of that vision.

One tantalizing sentence in Book IV suggests an early application of an endocrinological principle, but not an understanding of it. In describing the customs of a tribe which he called the Budini, Herodotus wrote that the people catch an ". . . animal which has a square face . . . and whose testicles provide a remedy for diseases of the womb."[3]

A few furrows were ploughed in the virgin field of organic evolution. Of course, Herodotus envisioned little of the great panorama of evidence that was to be placed on parade twenty centuries later, but he dwelt upon some of the concepts that were later to be identified with the names of Malthus and Darwin. Even though his examples were weak, he strained to demonstrate and support his theory that the survival of species of defenseless animals is maintained by their accelerated reproduction. Although parthenogenesis and "encapsulation" actually exist in certain rotifers and

Fig. 3. Variations of the phoenix, mythical bird, admittedly not seen by Herodotus, but reported by him. After a life span of 500 years (or more) in Egypt, it reputedly returned to Arabia and voluntarily flew into fire; then from its own ashes it arose to begin another 500-year cycle. The figures middle and left, appearing on ancient monuments, are sometimes believed to represent the phoenix; the one at the right certainly represents it. (From Rawlinson's woodcuts.)

aphids, Herodotus conveniently devised something akin to it in the hare. He stated that there may be four litters in different stages of development at one and the same time in a hare's uterus. Further to fortify his claims, he used an antipodal and equally false situation. The ferocious lioness has, according to this author, only one gestation in a lifetime. He further developed this thesis by stating that but one cub is born, and that the mother's womb is hopelessly clawed and lost during the delivery of it. That, he implied, is nature's way of keeping this predaceous animal numerically in its place. One more example was evoked to demonstrate population control. This time he chose the fanciful "winged serpents of Arabia." The males, he said, are disposed of by the females during mating; the females are likewise destroyed when the young ones eat their way through the maternal body wall at the time of birth.

Plants

Many references to botanical items are interposed, often casually. Rarely do Herodotus' words attempt to elicit mental pictures of the organisms. An exception is his description of Egypt's lotus which, though sketchy, clearly indicates *Nymphaea lotus,* the "white-flowered lotus of Egypt."

Herodotus made only desultory references to plant morphology, and it would seem that his interest in herbs and trees was colored by his concern for commerce and economics. In fact, a standard commentary[1] suggested that Herodotus may have been a traveling merchant. For instance, his stylus lingered over, not the size and symmetry of the castor plant, but the virtues of castor oil. He underscored its uses as an unction and a lamp fuel. Nor were his references to the papyrus plant concerned with its morphology and anatomy. He elaborated upon its importance in the Egyptian economy: for writing materials, ship sails, and even food. Plants and animals as dietary items are often referred to. Fermentation was already an old and commonly utilized process in his native Greece. So he casually referred to wine and beer without comment on their manufacture or effect. Straight-faced Aristotle was not so reserved; he interjected a priceless comment into his own writings[1] to the effect that a "man drunk with wine lay on his face, while beer laid him on his back."

The acacia tree evoked no enthusiasm for its form, size or growth habits, but Herodotus described how three-foot acacia planks were overlapped like bricks and fastened in that arrangement to construct the bulkheads of Egyptian vessels. On another occasion he mentioned asphodel (*Asphodeline lutea* Reichb.) and rushes, both of which supplied the slender stems used by the Nesamonians of Libya for building light, portable houses. On still another, he mentioned the use of madder root for dyeing clothing. Substantiation came recently when mummies were found with garb so dyed.[8]

In at least one instance Herodotus used plant ecology to circumscribe a geographical entity: "Weasels also are found in the *silphium* region" (Fig. 4).* Plants were often mentioned by him only in a cursory way, usually suggesting no interest in the plants themselves—e.g., the Persian sacrificer wore myrtle on his turban; and beans were never sown or eaten by Egyptians.

One allegorical statement he made suggests a knowledge of asexual reproduction, or the lack of it in certain trees. Herodotus quoted one of his

* This plant was probably the modern *Ferula tingitana,* a near relative of the wild carrot. *Silphium's* flowering stalk, according to Theophrastus, was eaten boiled or roasted; its root produced a resinous liquid supposedly of great medicinal value.

FIG. 4. Conventional representations of *Silphium* on coins of ancient Cyrene. This plant is not to be confused with the modern genus of the same name. This ancient and important organism was a member of the parsley family, and not of the Compositae, like the modern *Silphium.* This economic plant was so highly valued in Herodotus' time as a vegetable and as a source of a resinous drug (modern gum ammoniac) that it was figured on coins, as are wheat, fish, and grapes in those countries where such commodities are most important. (From woodcuts by Rawlinson.)

historical characters as saying he ". . . would destroy them like a fir," meaning *completely* or *finally.* Many trees send up adventitious shoots from their new stumps. Not so the fir. When cut down, it has no potential for vegetative reproduction.

Sanitation

Herodotus reported some practices and attitudes that suggest a rudimentary code of sanitation among the earliest civilizations. Whether or not his reports are true reflections of the ancient general practice, at least the ideal was stated or implied. He said about the Persians that "they never defile a river with the secretions of the body, nor even wash their hands in one. . . ." Another time, in reference to the habits of Cyrus, he noted that the great Persian, on a military campaign, carried a water supply with him "ready boiled for use, and stored in flagons of silver. . . ."

Meat inspection seems to have been a religious rite among the Egyptians, as it is today among the Hebrews. Regardless of the final expression of the precautionary measures resorted to, the underlying fact remains that the ancient Egyptians, in a land which is today below average in its practices of sanitation, did inspect the carcasses of slaughtered food animals. Herodotus recorded that the priests first made an external inspection; then they examined the tongue and the mouth. If the animal was free of certain telltale signs of parasitic infection, it was officially approved by branding a horn (if it possessed horns).

According to this Greek writer, the Egyptian priests were paragons of personal purity in their daily habits. They scoured their brazen cups every day and wore freshly laundered linen clothes. What is more, they shaved "their whole body every other day . . ." and bathed "twice every day in cold water, and twice each night." Although circumcision may have become involved in the niceities of religious ritual, Herodotus wrote straightforwardly that the Egyptians practiced it "for the sake of cleanliness."

Medicine

Human curiosity is whetted easily and keenly by anthropocentric ideas. Medicine is the most attractive of those ideas because health and personal preservation are natural and selfish concerns. Herodotus looked long and listened well when he confronted the healing arts, but the very setup of medical practice mitigated against him. It was a closed union. No amount of snooping by an outsider would disclose the carefully guarded secrets of the art. This was especially true in Egypt, where contemporary medicine was a chaotic admixture of astrology and mysticism. The great medical papyri, some of which had been written ten centuries before the time of Herodotus, were moldering in unknown tombs, not to be discovered for another 2300 years. He said, however, of contemporary medicine in the land of the Nile:

> Phisicke is so studyed and practysed with them that every disease hath his severall phisition, who stryveth to excell in healing that one disease, and not to be expert in curinge many: whereof it commeth that every corner is full of Physitions. Some for the eyes, some for the head, many for the teeth, not a fewe for the stomacke and

belly. Finally, such as are of knowledge to deale with secret and privy infirmities.[9]

And he stated in very general terms how he saw medicine practiced in Babylonia. Again, we must note that his vantage point was peripheral and his view contemporary. He leaves the impression that there were no physicians in Babylon. The lame and the diseased were transported to the public square, there to meet the merchants and shoppers who happened by. Recipes were traded, illnesses were compared, fertile imaginations sprung wild ideas, foggy formulas were pumped forth from stagnant memories. This clearinghouse was at once medical school, hospital, and physician in Herodotus' day.

With appropriate skepticism, the Great Reporter recorded a strange medical procedure of the Libyans. Four-year-old children had the veins of their temples or scalps burned "to prevent them from being plagued in their after lives by a flow of rheum from the head." If the drastic treatment brought on convulsions, the practitioners resorted to a standard treatment: they sprinkled "goat's urine upon the child, who . . . is sure to recover."

Without comment or attempted explanation, he reported a case of battle hysteria: "Epizelus . . . was in the thick of the fray, and behaving himself as a brave man should, when suddenly he was stricken with blindness, without blow of sword or dart, and this blindness continued thenceforth during the whole of his after life."

He also reported what seems to have been a double case of psychosomatic effect on childbearing in the royal Spartan household. Wife Number One, in her anxiety to conceive, was barren; Wife Number Two, acquired by special legislation, quickly produced a child. Wife One, released of her sole responsibility to conceive, produced three sons in rapid succession; Wife Two, in her chagrin at losing favor, never conceived again.

Embalming

Of all the accomplishments of the ancient Egyptians none has aroused more wonder and acclaim than their embalming practices. The best proofs of their success are the Egyptian mummies now to be found in all major museums, and evidently many bodies remain to be uncovered. Rawlinson[10] estimated that when embalming ceased in Egypt, about 1200 years ago, 420,000,000 mummies had been prepared. The number has been placed as high as 730,000,000.[11]

Most of the museum pieces were recovered twenty centuries after the death of Herodotus. What he wrote about the details of embalming in Egypt is borne out by careful examination of the mummies—for example, the skulls are empty, and there is a hole bored from the nostrils to the brain cavity, just as he described. Listen to his account of three methods of embalming (there were probably more[1]):

The mode of embalming, according to the most perfect process, is the following: They take first a crooked piece of iron, and with it draw out the brain through the nostrils, thus getting rid of a portion, while the skull is cleared of the rest by rinsing with drugs; next they make a cut along the flank with a sharp Ethiopian stone, and take out the whole contents of the abdomen, which they then cleanse, washing it thoroughly with palm-wine, and again frequently with an infusion of pounded aromatics. After this they fill the cavity with the purest bruised myrrh, with cassia, and every other sort of spicery except frankincense, and sew up the opening. Then the body is placed in natrum for seventy days, and covered entirely over. After the expiration of that space of time, which must not be exceeded, the body is washed, and wrapped round, from head to foot, with bandages of fine linen cloth, smeared over with gum, which is used generally by the Egyptians in place of glue, and in this state it is given back to the relations, who enclose it in a wooden case which they have had made for the purpose, shaped into the figure of a man. Then fastening the case, they place it in a sepulchral chamber, upright against the wall. Such is the most costly way of embalming the dead.

If persons wish to avoid expense, and choose the second process, the following is the method pursued: Syringes are filled with oil made from the cedar-tree, which is then, without any incision or disembowelling, injected into the bowel. The passage is stopped, and the body laid in natrum the prescribed number of days. At the end of the time the cedar-oil is allowed to make its escape; and such is its power that it brings with it the whole stomach and intestines in a liquid state. The natrum meanwhile has dissolved the flesh, and so nothing is left on the dead body but the skin and the bones. It is returned in this condition to the relatives, without any further trouble being bestowed upon it.

The third method of embalming, which is practiced in the case of the poorer classes, is to clear out the intestines with purge, and let the body lie in natrum the seventy days, after which it is at once given to those who come to fetch it away.

He wrote that cats and dogs were also accorded this kind of desiccated immortality and buried in sacred places. Such feline and canine cemeteries have been excavated,[1] again substantiating a point for the Great Reporter.

Without comment, and perhaps without confirmation by posterity, Herodotus noted that the Persians covered the bodies of their dead with wax before burial, and that the Assyrians used honey as an enduring preservative.

Anthropology

If we properly consider Herodotus as a general narrator, and not as an anthropologist, we can

better appreciate some of his musings on the subject of the races of mankind. He claimed that the dark-skinned peoples of India and Ethiopia produce black semen. For this he was rebuked by Aristotle in the *History of Animals* and again in the *Generation of Animals*. The great philosopher chided Herodotus with an analogy: Are not the teeth of the Negro white?

More cautiously, Herodotus referred to the one-eyed Arimaspi of the far northern regions of Europe and Asia, who "purloin gold from griffins." He doubted the existence of this one-eyed race; then, nursing his doubt, he philosophized that the remotest parts of the earth produce "just the things we think most fair and most rare."

He seems to have relished his reporter's role when he stated that in a certain tribe both male and female members were bald "even from birth." Although this is a hereditary and anthropological possibility, such an exotic and conspicuous group would have been noted by other travelers, as it was not. He probably was careless in noting the presence of a religious or ceremonial tonsure, or some other form of acquired "baldness."

His writings teem with references to anthropological criteria. He described a new race of people by noting that their appearance or dress might be similar to one previously discussed, but that their other customs were quite dissimilar. Sometimes his apparent ecstasy unleashed his vagrant imagination, as when he stated that Ethiopians were the tallest, most handsome, and longest-lived men in the world.

He recorded, perhaps for the first time, a description of a certain Paeonian people who were lake dwellers. Their homes were supported by piles driven into the lake bottom. A mountain of evidence gathered in the past two centuries has verified the custom as having been a widespread one in Neolithic and early historical times.

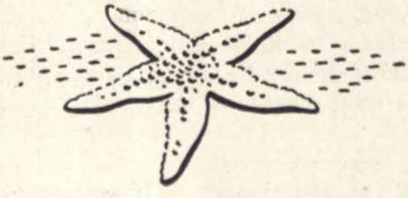
Bizarre osteological finds were reported by Herodotus: a skull without sutures; and upper and lower jaws in another case, each one possessing a single continuous, laterally fused complement of teeth, "made entirely of a single bone." Curiously, Pliny also described the fusion tooth (Bk. VII), and Plutarch noted this condition in young Pyrrhus, a Grecian king. Probably all were inaccurate.

Herodotus had, and still has, some faithful admirers. Aristotle frequently borrowed from him, a tribute further enhanced by the seven times Aristotle mentioned his name. Pliny often quoted him and his data. Sophocles penned a poem to him. And a modern medical historian, Robinson,[4] generously declares "modern scholarship confirms his trustworthiness." Another, the historian of science Dampier,[12] praises him for "his laudable curiosity" and his "valuable description of peoples." How and Wells[1] toss him two well-chosen flowers: he is "never dull" and he is a "great storyteller."

Biologist or no, he unwittingly wrote a neat and appropriate epitome of himself and his work in the first sentence of his famous history: "Herodotus beyng of the citye of Halicarnassus in Greece wrote and compiled an history to the end, that nether tract or time might overwhelme and bury in silence the actes of humayne kynd: nor the worthye . . . might want the due reward of immortal fame."[9]

References

1. How, W. W., and Wells, J. *A Commentary on Herodotus*, 2 vols. London: Frowde (1912).
2. Cooley, W. D. *Larcher's Notes on Herodotus*, 2 vols. London: Whittaker (1844).
3. *Herodotus—The Persian Wars* (trans. by G. Rawlinson). New York: Random House (1942).
4. Robinson, V. *The Story of Medicine*. New York: New Home (1943).
5. Hoffman, R. H. *The Struggle for Health*. New York: Liveright (1929).
6. *The Geography of Strabo* (trans. by H. L. Jones), 8 vols. New York: Putnam (1917).
7. *The Works of Aristotle Translated into English*. Vol. 5, *de Generatione Animalium* (trans. by Arthur Platt). Oxford: Clarendon (1910).
8. Cameron, G. *Ann. Med. History*, **4**, 466 (1932).
9. *The Famous History of Herodotus, Translated into English by B. R. Anno 1584*. New York: Knopf (1924).
10. Rawlinson, G. *History of Ancient Egypt*, 2 vols. New York: Dodd, Mead (1880).
11. *Encyclopedia Americana*. Article on "Embalming," **10**, 273 (1941).
12. Dampier, W. C. *A History of Science*. New York: Macmillan (1943).



Oldest Material: Newest Uses

WILLIAMS HAYNES

In November 1952 THE SCIENTIFIC MONTHLY *published a short abstract by Mr. Haynes from his forthcoming* American Chemical Industry: A History in Six Volumes *(Van Nostrand). His present article is based on Chapter 1 of his new book* Cellulose: The Chemical that Grows, *which is being published by Doubleday & Company on March 5.*

EONS ago, when some lovelorn caveman picked up a stout stick, clouted a pretty cave maiden over the head, and carried her off to his den, civilization started. Man had appropriated a natural object and used it to his own selfish ends. By wielding that club he had become a tool-using animal.

From the very beginning wood has been man's favorite material. His first weapon was a club, his first agricultural implement a crooked branch. Wood was also his first fuel. From planks to toothpicks, even today we fashion wood into more diverse shapes and put it to more different uses than we do any other substance. It is our oldest, our most familiar, our most useful, industrial raw material.

All wood is composed of about half cellulose. Every living thing is made up of minute cells, and the cell walls of all living plants are composed of cellulose. The sturdy oak and stately palm, the grass that covers the good earth, the lichens that clothe the rocks, even the minute algae that flourish in the sea, all are manufacturing cellulose. It is the great primary substance of the whole vegetable kingdom.

Cotton for calico, flax for linen, hemp for rope—our great vegetable fibers—are all composed chiefly of cellulose. Paper is almost pure cellulose. It is made of the fibrous part of wood cleaned of its sap, its resins, its minerals, and its binding compound called lignin, and then felted and pressed into thin sheets between rollers. These are old, everyday uses of cellulose, as commonplace as a handkerchief and this morning's newspaper.

Through chemistry, we have quite recently discovered startling new uses for this, our oldest material. Only a century ago a jolly little bespectacled college professor first combined cellulose with nitric acid and obtained nitrocellulose, the first chemical derivative of the chemical that grows. He never dreamed that he was laying the foundations of four great modern industries. Peering nearsightedly into his porcelain reaction vessel, Dr. Schönbein saw a fluffy white substance. It looked just like the cotton he had used when he began his historic experiment. He soon discovered, however, that this cottonlike substance did not behave like cotton at all. A chemical reaction had taken place. He had a new substance and he found that, for one thing, it exploded.

This cellulose nitrate of Schönbein's had other remarkable chemical properties. He prophesied truly that someday it would replace gunpowder. But he caught no vision of bright-colored plastics or luscious rayons or tough, quick-drying lacquers or transparent wrappings. Yet all these, and other new products too, lay in the future of cellulose once we learned to use it as a chemical raw material.

Today, cellulose treated with chemicals yields not only rayon, plastics, lacquers, and films for wrapping and for photographs and motion pictures, but many other useful products that range from fingernail polish and compounds to relieve stomach ulcers to wallboards and belting to drive machinery. Before we could harvest this rich crop of new products for better living, scores of patient research workers had to learn a lot about cellulose itself. Indeed, they had to probe close to the very innermost chemical secrets of life in order to solve the mysteries of the exceedingly complex cellulose molecule. Then brilliant investigators had to learn bit by bit how to make this stubborn, unreactive material combine with chemicals so as to make the new products to suit our needs. These researches are fascinating chemical detective stories. Their practical consequences to every man, woman, and child in America are staggering.

Motion pictures projected through a cellulose film are as epochal an advance in education and entertainment as was the word printed on paper made of cellulose. Mass production of all sorts of goods, from motorcars to pencils, would pile up in a jam at the end of the assembly line were it not for quick-drying lacquers to protect and decorate

the finished product. By making luxury fabrics available to everyone's pocketbook, rayon has rendered the "silk-stocking class" in America as obsolete as the zinc-lined bathtub. Other chemical offspring of cellulose are, as we shall see, quite as revolutionary characters.

Within the past twenty-five years, thanks to chemical research, man's oldest material has thus become the basis of ultramodern industries. The chemical that grows is now one of our most important, most versatile raw materials, so that today we Americans consume over 85,000,000 tons of cellulose. That is almost as many tons as we use of steel and ten times as much cellulose as we consume of both aluminum and copper.

The purified cellulose we use in industry takes no account of the billions of tons of raw cellulose eaten as fodder by beef cattle, dairy cows, and horses, or the billions more tons that we ourselves consume in carrots and beets, apples and peaches. All grasses, all vegetables, all fruits, contain more or less cellulose.

Fortunate indeed for mankind that this greatest of our raw materials is what the economists call a "replenishable asset." Unlike our great mineral raw materials—coal, petroleum, sulfur, phosphorus, potash, and all the metals—the supply of which is fixed, cellulose is a product of all living plants. It is truly a substance of superabundance in nature. No matter how many new uses we may find—and new uses of cellulose are developing thick and fast—we shall never run short of this raw material. If one source of cellulose becomes seriously depleted (as might conceivably happen) we can use other forms for chemical processing and also for fabricating into wallboard to replace builders' lumber.

Not only is cellulose abundant but, what is most important in these days, it is widely available. Save in the polar regions and the deserts, cellulose is at hand for use by all peoples all over the earth. There is probably no have-not cellulose nation. Cellulose is the great chemurgic crop—that is, a crop grown for industrial use, not for food. The chemical that grows is an ideal raw material out of which to build a global economy of abundance for all mankind.

Nature seems to have arranged this for us most providently and conveniently, for she has set up a neat cellulose cycle. Sheep and cattle digest cellulose, turning it into chops and roasts, which we relish. On the other hand, the cellulose we eat serves only as roughage in our diet, since we cannot digest and assimilate it. A subtle distinction, this, between man and beast, one of tremendous import.

More subtle still is the distinction between cellulose and sugar, two products that are manufactured exclusively by plants. In the chemical elements that make up the complex molecules of cellulose and sugar, the only difference is that of two atoms of hydrogen and one atom of oxygen, H_2O, one molecule, more or less, of water. Although cellulose is quite indigestible in our human stomachs, sugar is easily assimilated and readily turned into fat—too readily, alas, for many of us! This minute distinction of a single molecule of water is another of the thick-veiled chemical mysteries that still surround cellulose.

Reasonably enough, the first processing of cellulose was a simple mechanical fashioning. The Egyptians who made the first paperlike material by pulping papyrus reeds, drying them, and rubbing the surface smooth with a knucklebone wrought no chemical change in the material. From this mechanical pulping and polishing of cellulose, the clever and secretive Chinese took a long forward step when they made true paper by suspending a fibrous mass of cellulosic pulp in water and dexterously depositing it in a thin, even sheet on a wire screen, through which the liquid drained off. They perfected this tricky technique some two centuries before Christ. Not till A.D. 751 at Samarkand was this useful art revealed to the Western world. That fabled city, then the easternmost outpost of the conquering Moslem armies, was attacked by the Chinese, and in following up their repulse, the Arab governor captured several skilled papermakers.

Papermaking soon became quite a business of the Arabs, and it spread throughout the Moslem world. The Moors carried it to Sicily and Spain. Thence it moved on to Italy and France, the Low Countries and Germany, and finally to England. By the fifteenth century paper had replaced vellum and parchment throughout Europe.

Our modern gigantic papermaking machines, from which roll more than 2000 feet of paper a minute, mile after mile, in sheets as much as 24 feet wide, still employ what is essentially a highly mechanized form of the dexterous hand technique of the Chinese. Paper is still a thin tissue composed of any fibrous material whose individual fibers, first separated by mechanical or chemical action, are deposited and felted together on a wire cloth.

The phrase "any fibrous material" opens a door as wide as the breadth of the vegetable kingdom. In different lands papermakers at various times

have used cellulose from many sources, picking always the cheapest and most readily available. The most ancient process of the versatile Chinese was to boil the fibers of the bark of the mulberry tree in a lye made of leached wood ashes. They also employed bamboo pith and, later, after the plant had been imported from India, cotton fibers. Knowing antiquarians can readily identify old manuscripts written on so-called Oriental paper, because it is thick and soft, white, and exceedingly well preserved over the centuries. Paper made by the Arabs is thinner, harsher, and tougher, for it was made of linen.

Whatever the fibrous cellulosic material employed, the problem of the papermaker is always to eliminate the glutinous, resinous, siliceous, and other material found within the cells, always associated with the cellulose of the cell walls of all plants. In Europe papermakers soon discovered that rags furnished the easiest, most economical raw material. The fibers in cloth had already undergone a partial cleansing, and white linen rags were much preferred, for they made the stoutest, whitest paper.

There was then no known method of bleaching dyed rags. Accordingly, even the finest sheets of paper made from hand-picked scraps of fine linen were creamy or grayish. The ordinary grade was distinctly brown, frequently speckled. Nevertheless, quality improved and output increased. So papermakers embarked on the familiar economic sequence: lower price, wider use, greater demand. Their expanding market ran plumb into a barricade: a shortage of rags. Various substitutes were sought. In 1659, when Queen Elizabeth granted a ten-year monopoly in papermaking to her favorite jeweler, Sir John Spilman, it carried exclusive rights to gather not only linen rags but scrolls and scraps of parchment, old fishing nets, cordage, and "any other materials as might be suitable."

After newspapers appeared, the rag shortage became acute. Here was a new use for paper, not to be filed away in official archives or bound in stout calfskin, but a sheet to be scanned and discarded, crumpled under the kindling in the fireplace, or twisted into spills for lighting tallow candles and long clay pipes. Some whimsical economic philosopher may one day enjoy expanding the theme that our first frankly expendable manufactured product was newsprint paper. Be that as it may, the early newspaper editors were not fussy paper buyers. They could not afford to be. They must have paper, lots of paper, cheap paper. So long as it was printable, they did not worry about its appearance or its permanence. Accordingly, papermakers began experimenting with straw and from it they produced the first newsprint. It was pretty poor stuff, off-color and harsh; but it was cheap and in reasonable supply. What distressed the printers was that its hard, rough surface wore down type metal like a file, so that a font of type, previously good for a year's hard service, wore out in three or four months.

About this time both machinery and chemicals appeared suddenly on the papermaking scene. The Industrial Revolution came to paper in 1798 when Louis Robert, clerk in the Essonnes paper mills in France, invented a machine that a London stationer, Henry Fourdrinier, introduced into England. Paper made by machine meant more paper, cheaper paper. The rag shortage became a famine. Discovery of the bleaching action of chlorine saved the papermaking machines. It eked out the pulp supply by bleaching hemp rope and burlap bags. The hemp and jute papers, like those made from straw, were of poor quality. There was still plenty of incentive to find some other cheap, abundant material that would pulp easily and produce a good sheet.

Any cellulosic material can be made into paper. Indeed, a busy French chemist once made a respectable product out of thirty-seven different plants that ranged from artichokes to pine trees. Plainly, it was well worth while for a man to discover a more suitable raw material for the hungry paper mills, so ambitious chemists and adventurous papermakers began hunting. They were granted many patents, but it was a simple hand-weaver of linens in his cottage in Hainichen, Saxony—Gottfried Keller—who in 1840 made the great discovery. Keller had a chum, another cottage craftsman, Heinrich Voelter, a handicraft papermaker who directed his friend to this problem.

Keller first attempted to make paper out of wasps' nests, a tough, water-repellent material that had heretofore (and has since) attracted other investigators. He failed, but one evening while strolling about the village he came upon a group of children intently gathered around a grindstone. One sturdy boy was whirling the crank; the others were taking turns pressing cherry pits against the revolving stone in order to grind in each a tiny hole. It was, in fact, a cooperative enterprise in the manufacture of cherry-pit necklaces. Some smart youngster had discovered that, by half burying the pits firmly in a little depression in a board and then holding them against the whirling stone, the risk of raw and bloody fingers was eliminated.

The revolving stone passed in its lowest circuit through a trough of water upon which floated a thick layer of pulverized wood and cherry stones. Keller scooped up a handful of this woody scum and pressed it to squeeze out the water. It became a pulpy mass which, as he kneaded it between his fingers, reminded him of the rag pulp that Voelter turned into paper. He hurried off to his comrade, and next morning they were at it again, experimenting. Out of those experiments came cheap wood-pulp paper.

The inspired Keller did not become a multimillionaire. He sold his share in the venture for $700 to his friend Voelter, who about 1858 perfected a process for the ground wood pulp. His methods were too easy to copy, so that even he, though he sensed its commercial importance, gathered no suitable reward. Neither realized what he had accomplished. Not only had they broken down the rag-supply barrier, but they had opened up the first great market for cellulose. Furthermore, their ground wood pulp of reconstructed cellulose soon led to chemical treatment of cellulose.

Ever since 1690, when William Rittenhouse, a German papermaker, and William Bradford, an English printer, became partners in the first paper mill in North America, the familiar rag problem had been especially insistent in this country. Rags were always extraordinarily scarce. To make matters worse, the American colonists were avid newspaper readers, a habit that has persisted long after other admirable characteristics of Puritan and Cavalier have vanished.

Inventive Americans tried out all the likely fibrous materials, and Yankee ingenuity came up with some novel suggestions. Patents were granted in 1802 to Burgess Allison and John Coffin for paper from cornhusks, and in 1809 to Samuel Green, the printer of New London, Connecticut, for paper from seaweed. Chlorine bleaching was promptly borrowed from the textile mills by American papermakers. Bleached straw paper began to be made, so that during the early 1850s the price of good rye straw jumped from $6 to $20 per ton. This pleased the Midwest farmers. They had many acres planted to rye to supply the demand of the whiskey distillers, and now they had a pleasant cash bonus in the sale of by-product straw.

Ground wood-pulp paper was made here shortly after 1865. It had defects as obvious as a break in a motion picture film. The cellulose fibers were short, which meant low strength, and the pulp contained the saps, resins, and other substances associated in the wood with cellulose. These were later removed by boiling in soda ash, an idea hit upon by English papermakers, but there was still plenty of room for improvement.

An American made a most notable chemical contribution to papermaking. The innovator was Richard Albert Tilghman, scion of a distinguished family established by Dr. Richard Tilghman, who came with Lord Baltimore's first colonists to the Eastern Shore of Maryland. Of all the soldiers—Colonel Tench Tilghman was General Washington's favorite aide—governors, great lawyers, and many physicians among the Tilghmans, this Richard left a worthy mark. He perfected a process for splitting tallow into fatty acid and glycerin, which revolutionized the soap and candle businesses; he developed the process upon which was founded the famous Baltimore Chrome Works, which made a superior yellow pigment for paints; with his brother he invented the sandblast process of polishing metals and cleaning stone.

We are chiefly indebted to this tall, handsome aristocrat for the sulfite pulp process, which makes a type of paper still in universal use. The principle of his method, though refined in details, is still the basis of the process. Logs, debarked and with the knots removed mechanically, are chipped in a hogging machine to thin slices, which are boiled under pressure with sulfurous acid and lime or magnesia. Tilghman's sulfite process for making fine paper stock, and the English soda process, which treats ground wood pulp with soda ash to produce newsprint, have a younger rival. This sulfate or kraft process produces a tough paper from wood chips, cooked in a mixture of caustic soda and sodium sulfide. This is the industry that since 1935 has blossomed in our Southern states so amazingly that now more than fifty mills are pulping over 4,000,000 cords of wood each year, turning out some 3,000,000 tons of kraft paper and paperboard.

Without the foggiest suspicion of the chemical composition of this curious, complex substance, apt craftsmen of five thousand years ago had found ways to fabricate cellulose. They soaked the stems of flax till the mass fermented, dried them, flayed them with mallets, and picked out the long linen fibers, which they wove into the first true textiles. The cotton plant was found in India, and the fluffy fibers of its seed bolls—almost pure cellulose—were spun and woven into cloth. By hook or crook, men with no knowledge of chemistry had learned many ways of modifying materials through chemicals to make them more adaptable, more

durable, more beautiful. They had discovered how to turn clay into pottery and sand into glass; how to dye cloth and tan leather; how to ferment fruit juices, and how to smelt metals from their ores. Then suddenly, a century and a half ago, just after our own Revolutionary War, a handful of bold, brilliant experimenters laid bare the basic principles of chemical science.

With some knowledge of the elements and some comprehension of how they combine, definite chemical processing methods replaced rule-o'-thumb techniques. Sulfuric acid supplanted vinegar (acetic acid) and sour milk (lactic acid) as acidulating agents in bleaching and dyeing. Sodium carbonate, prepared from common salt and called "soda ash," replaced potash, the crude potassium carbonate leached out of wood ashes, in the manufacture of glass and soap.

So the chemical industry was born. Definite chemicals were put to work on the old job of making natural materials more to man's liking, and while some men were learning the first lessons in chemistry, others began to hitch power to tools, transforming them into machines. These machines created new jobs for chemicals.

When flax was spun and woven by hand, it was all very well to bleach linen by spreading it out on the grass in the sun for several weeks. But when one girl at the automatic spindles could spin 12,000 yards of yarn in the same time that an expert at the spinning wheel could produce a single yard—and with a corresponding increase in fabric turned out on the power looms—then the power-driven textile mills threatened to cover the land with unbleached greige goods. By a lucky historical coincidence, just at this time Claude Berthollet discovered the bleaching action of chlorine. This chemical whitened cloth in minutes instead of the days required by the action of sunlight. Furthermore, it worked as efficiently on a cloudy February afternoon as on a sunny July morning. Chlorine literally rescued the power textile industry.

Thus chemicals achieved a new function. Hitherto they had been employed to change or decorate materials: to transform fat into soap, to tan leather, to dye cloth. Henceforth they were called upon in the Machine Age to save time and to cut costs. In our own day chemicals have been put to an entirely new task, which is rapidly making changes in our way of life as fundamental as those wrought by the Industrial Revolution. Out of chemicals we are now producing our own materials tailor-made to suit our needs.

A century ago, stirred by their new knowledge, the early chemists eagerly began taking apart all sorts of things to learn of what elements they were made. It was a thrilling game, exciting as a lottery. The big prize was the discovery of an element that would blazon a man's name in the annals of science. Less sensational finds had great practical results. Two painstaking French pharmacists, Pelletier and Caventou, specialized in barks, leaves, and roots used in medicine, and they isolated the alkaloids quinine, cinchonine, and strychnine. In this way medicine, like industry, profited by being able to use active chemical principles. No longer was the malaria patient forced to down great nauseous draughts of "Jesuit's powder," the pulverized cinchona bark from which quinine is now extracted and made into neat little pills.

During this orgy of analysis a young German chemist, meticulous Friedrich Wöhler, uncovered an amazing chemical fact, very pertinent to our understanding of cellulose, the chemical that grows in every living plant. Until 1828, everyone firmly believed that a fundamental difference existed between inorganic compounds (mineral substances like gold and granite) and organic compounds, such as blood or beeswax, products of living animals and plants. That year Wöhler published the results of an experiment he had carried out four years earlier. Deliberately he had withheld this information, for he was a cautious, devout man and its logical conclusion was so startling that it appeared sacrilegious. By pouring ammonia water into a solution of potassium cyanate, Wöhler produced ammonium cyanate. In order to recover this colorless crystalline material in dry form, he evaporated the water, but on continued heating, instead of the long crystals of ammonium cyanate, he obtained smaller, needle-shaped crystals that on analysis proved to be urea. Urea, a constituent of urine, is a waste product of all animal life. In a test tube Wöhler had bridged the gulf between organic and inorganic materials.

This astounding discovery revealed new horizons. If Wöhler could produce urea without the aid of a kidney, surely other animal and plant products might be made in the laboratories. So reasoned Marcellin Berthelot, who succeeded in preparing first acetic acid (vinegar), then phenol (carbolic acid) and benzene, two products of coal tar, and finally alcohol. This opposite of analysis—putting together molecules instead of taking them apart—he christened "synthesis." His sensational triumphs inspired other chemists to engage in this seemingly miraculous feat of chemical creation.

Today a multitude of synthetic products play a

common role in the workaday world. They all replace or supplement some natural product. This great array of man-made materials—fibers, coatings, plastic and elastic compounds, abrasives and adhesives, colors, scents and flavors, leatherlike, rubberlike, and spongelike materials, even new alloys which, after all, are only synthetic metals—are all of them products tailor-made to do specific jobs more efficiently, more cheaply, than any of the raw materials provided by nature. This manufacture of materials has become a major function of chemicals. From serving us by modifying materials more to our liking and from saving time and money in manufacturing processes, chemicals have become the actual suppliers of synthetic raw materials.

Interwoven with the story of wood is the universal theme of man's use of his materials. First comes the natural product, firewood and war club; then the fashioned material, shingles and clapboards, bowls and bedsteads; later the processing of wood into paper, potashes, and wood alcohol; finally, the employment of pure cellulose in making fibers, plastics, and lacquers. From simple to complex, our use of cellulose has proceeded step by step from broad employment to highly specialized application.

That, as Kipling said, is another story.

But be it noted here that wood cellulose can now be prepared by either the sulfite or the sulfate process, so pure that it is available for chemical transformation into sheer, transparent films or the delicate filaments of rayon, into the lustrous coating of your television set or the rugged, resilient plastic of your car's steering wheel.

SMO ON THE AIR

Station	Sponsor	Time
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Broadcast Limited (Modern Science)	9:45 P.M.

The Editor of THE SCIENTIFIC MONTHLY will be glad to cooperate with university or other educational stations interested in securing scientific material suitable for broadcasting.

Tick Talk*

CORNELIUS B. PHILIP

Dr. Philip, principal medical entomologist and assistant director of the Rocky Mountain Laboratory of the U. S. Public Health Service, Hamilton, Montana, has had extensive experience with ticks and other insects capable of transmitting diseases to man (see THE SCIENTIFIC MONTHLY, 69, 281 *[1949]). In 1947 he was a member of the Army Scrub Typhus Team in Malaya and in 1951 served as a consultant to the Air Force Arctic Aeromedical Laboratory, Fairbanks, Alaska. He was a colonel in the Sanitary Corps during World War II and is now in the Medical Sanitary Corps Reserve. He was honored with the USA Typhus Commission Medal for his work in the second world war.*

REFERENCES in the writings of the ancient Greeks attest the annoyed attention that ticks received even in those times, and undoubtedly they were even more annoying to our primordial forebears, who had none of the modern repellents to rely upon for protection. Some unique attempts must have been made (perhaps similar to those of today's Central African natives, who anoint their exposed bodies freely with fresh bovine urine collected in the cupped hands as from a faucet) to discourage attachment of arthropod pests. In 850 B. C., Homer wrote, "There lies Argos, the dog, full of vermin [*kynoraistes*]," and this Greek word has arisen to plague us in this era of binomial nomenclature. Homer is believed to have referred to *Ixodes ricinus,* the familiar castor bean tick of Europe.

That ticks attracted increasing attention from that time on is abundantly shown in many historical writings, but until the advent of systematic zoology in the eighteenth century ticks in the Old World were described and complained about under only two categories: the infamous "miana bug," or "teigne," which plagued caravan travelers in Persia, and an ixodid obviously allied to the above mentioned castor bean tick, which was noticed by Homer and his compatriots in Greece and other Mediterranean countries as especially annoying to dogs and other domestic animals. This early literature is full of superstitions and parables, for which there is space to mention only a few.

Pagenstecher in 1861 gave a scholarly review of all the early references that could be assigned to *I. ricinus.* Taking into account the present abundance of the brown dog tick, *Rhipicephalus sanguineus,* in the Mediterranean area, it is probable that at least this species also was concerned. It is not too surprising, in view of the grotesque early methods of treating various maladies, that crushed, engorged ticks fresh off animals should be seriously advised for treatment of open sores and other ailments. Such a concoction was even recommended as the chief base for an ancient love potion, and a notation from Pliny suggests its use as depilatory. Another quotation from Pliny reads, "The blood of the ricinus heals the sacred fire, which power it has in the animal kingdom in common with the earthworms, crickets, heads of the viper and goose grease." The "sacred fire" may be a free translation referring to a modern affliction. Aristotle gives repeated evidence of his familiarity with the source of tick infestation of animals incidental to wandering in the grass. The widely accepted early belief that ticks were disgusting creatures which consisted merely of sacs that became filled with the blood of their victims to the bursting point, and then died because they had no anus, seems to have originated with Pliny. Somewhat later this idea was enlarged upon because the alimentary tract of the tick appeared to terminate as a blind pouch to hold the blood of the host.

Several parables of early times make comparisons with the observed parasitism of the tick burying its head in the blood stream of its victims and remaining there for days without moving during its gluttonous repast. Petronius quotes the following parable: "I thus hope to die, as a dead one I do not feel ashamed; you, however, are so industrious that you won't look behind yourself! You see the little louse on others, but you don't see the 'tick' on yourself." The Scotsman Bobbie Burns was antedated here by several centuries.

Two kinds of vermin were thus recognized in

* Based on the annual invitational public address of the Entomological Society of America in joint session with the American Association of Economic Entomologists, Denver, Colorado, December 19, 1950.

this later literature. It is interesting to note that before the age of taxonomy morphological differences between the sexes, especially when they were fully fed, should result in calling the one "ricinus" and the other "reduvius." Methods of suggested control were naturally primitive, but one of the recommendations of Pliny was the application on infested and suffering animals of an ointment prepared by boiling lichens in sulfur. This has some wisdom akin to contemporary ideas of the utility of sulfur.

The miana bug (probably *Argas persicus*) must have been a real nuisance to travelers in Persia who stopped at caravansaries, for there were repeated reports of fatal attacks on strangers visiting the country. Obviously, local residents were not so affected, since the trouble was often referred to as "stranger's disease." It is probable that a virulent form of relapsing fever now known to occur in the Middle East may also have been involved. One of the drastic cures consisted of wrapping the patient in a freshly removed cow hide and anointing exposed parts with honey as the hide dried and contracted. Presumably this facilitated the squeezing out of the malady. The "stranger's disease" was even mentioned in diplomatic reports to European courts.

Some fascinating reports of encounters with the pigeon tick, *A. reflexus,* appear in early European literature. The custom of building dovecots adjoining upper bedrooms resulted in the invasion of the occupied premises. Physicians who treated persons attacked have given several lurid accounts. In one case the owner disposed of the birds and walled up the pigeon house. Two years later, he reported to his doctor with an erysipeloid swelling spreading from tick bites on the wrist to encompass the entire body. He had shortness of breath, palpitation, and dullness for an hour or so, followed by profuse sweating. The swelling subsided after about 15 hours. In another instance a family moved into a house that had been vacant for four years. The previous occupant had kept pigeons and chickens in the dwelling. The father of the new family developed generalized furunculosis, which was later contracted by his two children. When the family moved out, their trouble stopped. Within two days another family moved in, and the father and a child of the new occupants suffered from the same effects. On thorough disinfestation of the house the difficulties ceased. It seems likely that *Argas,* with contaminated mouth parts, was spreading the affliction.

Of interest here are the recent observations of Anigstein and his colleagues that extracts from adult ticks, especially after feeding, have an antibacterial factor that inhibits the growth of many types of bacteria in vitro. This probably has some significance with relation to the nonadaptability of certain pathogenic bacteria to residence in ticks.

Historical accounts in the writings of Livingston in 1857 during his missionary travels in tropical Africa contain what are probably the first references associating fevers with the bites of ticks endured while sleeping in native huts. The tick concerned, presumably *Ornithodoros moubata,* is one of our most domesticated arthropods.

In Madagascar, natives have reported a tick (*Ornithodoros* sp.?) which can cause death by its bite but to which the local residents have become immune. It is even reported that the ticks are useful to keep in the house to discourage raids by hostile tribes.

Only a relatively few kinds of mites are parasitic on the higher animals in one or more stages, but so far as known all stages of ticks—both the soft-bodied argasids and the "hard-shelled" ixodids—are entirely dependent for their development on the blood of vertebrate hosts. All vertebrates, excepting fishes but including amphibians, have been recorded as tick hosts. This parasitic habit, however, has taken on some remarkable deviations in various species in all parts of the earth, except in the extreme frigid zones. Likewise, the cycles of various kinds of disease agents which have become adapted to transmission by ticks present some fantastic adaptations to this varied tick-host relationship. This is particularly true of the piroplasms, such as cattle fever. This disease, incidentally, has the historical distinction of being the first to be demonstrated as tick-transmitted of a long line of subsequently discovered important arthropod-borne diseases. Because of the extreme variations both in the developmental cycle of the ticks and of the disease agents themselves, methods of control have been very complicated. In some cases only one stage of a given vector is involved; in others all stages may be adequate vectors, but infection is not transferred to subsequent generations. In still others there is adaptation of the infectious agents to transovarial passage as well. Obviously, the more hosts attacked at various stages, the greater the danger of the spread of animal and human maladies.

One of the most interesting recently discovered rickettsial pathogens that is adapted to a wide spectrum of tick hosts is *Coxiella burnetii,* the agent of Q fever. This has now been reported from all continents and in ever-widening localities; nine species of ticks have been found naturally infected.

A jack rabbit found dying near Ringling, Montana. Massive infestation of adult ticks in sparse hair along back caused fatal anemia by withdrawal of blood. (Philip *et al. J. Am. Vet. Med. Assoc.* [1935]).

Tick tissues and tick feces become massively infected, and the rickettsial agent is unusual in that it is filterable and can be extremely diluted in the laboratory. Titers in these materials as high as 10^{-10} have been reported several times. One titration of tick tissue by the writer was 1:500 billion (2×10^{-12}) for tick tissue. This dilution can be dramatically portrayed when it is stated that 1 gram of ground infected tick tissue could be diluted and still maintain an infectious dose for a guinea pig in each drop of liquid, to the equivalent of the amount of water going over Niagara Falls in 5⅓ minutes, assuming that an average of 200,000 cu. ft./sec. is spilled over that cataract. In other words, 8,745,000 gallons would have to spill over to exhaust the diluted gram of infectious tick tissue.

Tularemia is a bacterial disease often transmitted by ticks to man and may cause serious losses in his domestic stock. An epizootic occurred among heavily tick-infested sheep and jack rabbits near Ringling, Montana, in 1934. Forty per cent of 1320 yearlings were affected symptomatically, and 200 died. Infection was recovered from ticks and tissues from both sheep and hares; however, it is known that massive parasitism by ticks can cause death from secondary anemia which is due simply to withdrawal of blood. One moribund hare was found with a hemoglobin reading of only 20 per cent; 152 *D. andersoni* were found on this unfortunate animal, and no infection was demonstrated in either ticks or tissues. Jellison and Kohls have shown that domestic rabbits may be practically exsanguinated with as few as 60–75 adult female ticks feeding over a period of about twelve days.

The argasid ticks feed intermittently and for brief periods, like bedbugs, and produce smaller numbers of eggs than do the ixodids, which generally remain attached to the host for some days. The often restricted host habits of the ixodids make the finding of acceptable animals more hazardous, and therefore their fecundity is considerably greater, even astonishingly so in certain species. Over 18,000 eggs were counted in one batch laid by a single *Amblyomma maculatum*. By comparison, most insects are much less prolific, and in addition are very much shorter-lived. There are some remarkable records of longevity of unfed argasids. One unverified report by Pantazi of eighteen years seems unlikely. But there are a number of records of five or six years or longer with occasional feedings, and Dr. Davis, of the Rocky Mountain Laboratory, kept an *O. turcata* for nine years without a blood meal from the time it molted and for an additional three and one-half years after one feeding.

Pelts, and sick and dead sheep, and a dead jack rabbit collected during a tick-caused tularemia epizootic near Ringling, Montana.

The greatest fecundity of these species seems to be in proportion, fortunately, to the reduced chances of replacement. To ensure the survival of the species we could expect over the years that about one pair would be all that replaced each

parent female of the previous generation. Otherwise, with some thousands of eggs being laid by many ixodid females, we soon would be "swimming" in ticks before many generations had passed in a given locality.

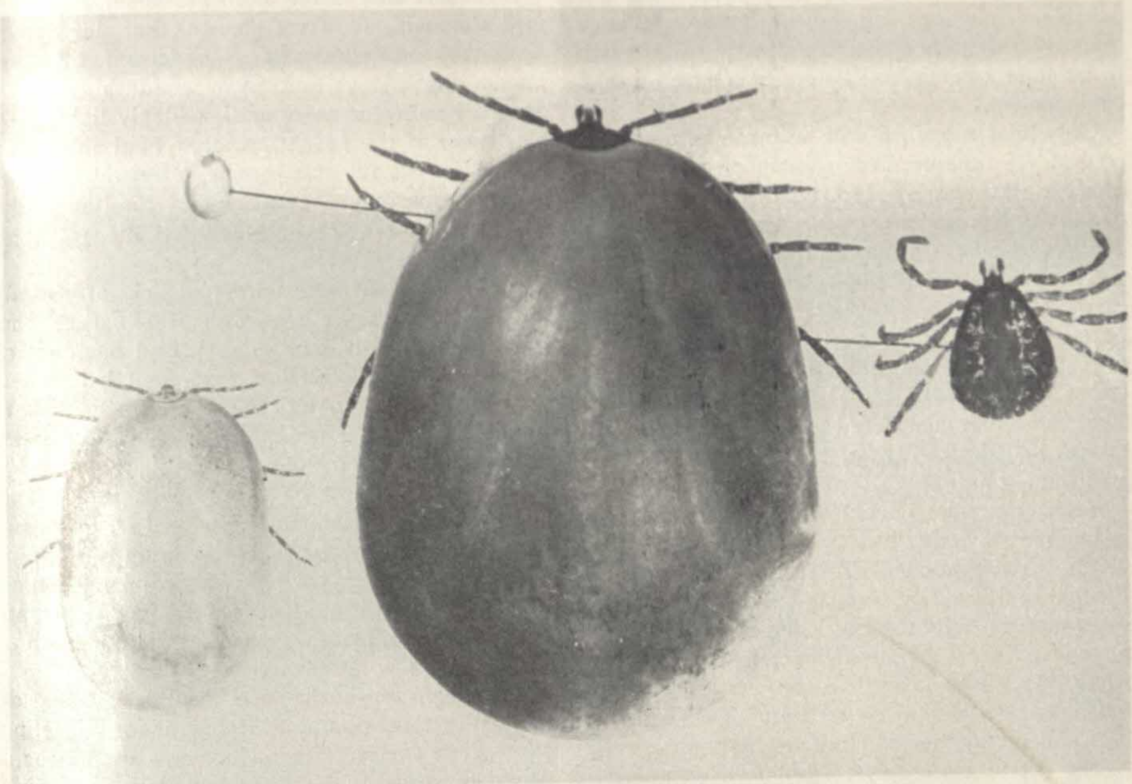
A few species of parasitic mites are known habitually to penetrate the skins of their hosts. Ticks, on the other hand, customarily attach without burrowing in. There are rare records, however, of the complete penetration of the tick beneath the skin of its host. In this country, Portman and Dalke describe the pelt of a red fox which contained some 150 completely imbedded *A. americanum* in all three stages. Strangely, the connective tissue in which the ticks were embedded did not appear to have formed nodules or cysts around the ticks, as was reported by Bell and Chalgren in the skin of a cottontail infested with *I. dentatus*. Seventeen such nodules contained all three stages of the latter when examined. It is possible that the phenomenon reported by Trager of an immune reaction to tick infestation resulted in the engulfment of the attached tick by the edematous swelling of host tissue around the point of attachment. Species of *Amblyomma,* with their long rostellums, are observed especially to cause local irritation in the host, so that the feeding ticks appear to be partially embedded in the swelling skin. The long rostellum of *A. americanum* has even permitted authentic cases of attachment through socks.

In the vast population of arthropods in the world it is perhaps not surprising that some very common and ubiquitous insects pass almost unnoticed by most persons, whereas ticks of one kind or another are a familiar form of animal life to everyone in the temperate and tropical zones of the earth, because of their propensity for parasitizing man and/or his domestic animals. They affect the domestic economy of primitive and civilized peoples alike.

It is not strange, then, to hear all sorts of stories of the best way to get them off once they have become attached. The "drilling" apparatus of the tick consists of a fixed hypostome with immovable teeth (i.e., armed) and movable chelicerae with articulated cutting digits at the tips. As Nuttall has pointed out, the armature of the hypostome varies and seems developed with rows of recurved teeth in some proportion to the habits of parasitism of a given stage, especially in the genus *Ixodes*. Here there may be a bisexual difference where mating often occurs off the host, and only the female may require a blood meal for oviposition. In this case, she may have a well-armed rostellum, whereas that of the male is only feebly so. The larvae of *O. moubata* molt to the nymphal stage before feeding and hence are without teeth in the larval stage. Conversely, although the nymphs of the spinose ear tick have well-armed hypostomes during their several months' existence in the ears of the host, the adults resulting from last-stage nymphs that have left their hosts mate and procreate without another blood meal, and their hypostomes are unarmed.

The hypostomes of some species, particularly many *Amblyommas,* are especially long and heavily

Pelted sheep dead of tick-caused tularemia near Ringling, Montana. Sick and dead sheep are scattered among sagebrush in background.



The largest and smallest known ticks. Engorged females (left) of *Ixodes soricis,* the smallest, from a British Columbia shrew, of *Dermacentor andersoni,* Montana, about as large as a fingernail, and (center) of *Amblyomma varium,* the biggest, from a Venezuelan sloth; (right) male of the last-named.

armed with rows of rigid teeth. When the skin of a guinea pig with *D. andersoni* attached is peeled back, the mouth parts do not usually penetrate through it, but, under similar circumstances, those of attached *A. americanum* or *A. cajennense* will be seen protruding through it; yet the females of each fill up about equally rapidly.

The difference in firmness of attachment is shown when each is forcibly pulled loose. Both may take some skin and flesh, but the mouth parts of *andersoni* have practically never been left behind in our experience with literally millions hand-pulled with forceps from laboratory animals incidental to preparation of "tick-type" spotted fever vaccine; this happens more often with the *Amblyommas,* prevalent in the Southeastern United States and in Latin America.

Among the frequently reported methods of removal is the traditional favorite of unscrewing them, which, of course, is more likely to leave the mouth parts in the flesh than if simple firm traction is applied forward against the direction of attachment. Such reported popular means of removal as the use of vaseline to "drown" them out, use of turpentine or coal oil, running a pin through them, approaching them with a burning cigarette or match simply kill them *in situ,* as tests have shown. Tobacco or "snuss" juice and even Black Leaf 40 had no effect. Tinctures of iodine and merchurochrome seem not to have caused attached ticks noticeable discomfort, although one of each sex of *D. andersoni* changed positions in a capsule on an animal after an iodine application. A feeding female also moved to a new position in a large capsule 15 minutes after turpentine, leaving a strong aroma in the capsule, had been touched to another tick half an inch away.

On the other hand, there is evidence that some animals and people have a natural "aroma" that repels ticks. At least it is well known that certain persons under identical conditions of exposure do not become infested to the same extent as do others. Brennan reported a Missourian a few years ago who actually killed some unattached ticks confined on his skin in ventilated cells for 20 minutes or so.

Considering the fixed armature of the hypostome, and the ticks' resistance to forcible removal,

it is remarkable how easily and quickly they are capable of voluntary detachment. But no substance of our acquaintance will consistently cause them to let go of a living host before they have finished engorgement.

Only brief reference will be made here to tick control. A number of substances have been screened in recent years for acaricidal or repellent properties, by personnel of the Bureau of Entomology at Orlando, Florida, and Kerrville, Texas, of the Rocky Mountain Laboratory, and of the Naval Medical Research Institute. Smith and Gouck found that the repellents stocked by the Army—di-methyl phthalate and "6-2-2"—reduced tick attack if used every three days on clothing, and Cole and Smith considered another, Indalone (although malodorous), as one of the superior available and consistent substances, and confirmed *N*-butylacetanilide, which Brennan had reported to be especially effective, as "outstanding on basis of effectiveness alone." This also killed ticks *in situ* either when they were touched or when they were exposed to fumes, but it did not cause either *D. andersoni* or *A. americanum* to detach, even when they were confined in capsules.

It has been dismaying to learn that strains of flies and mosquitoes can become resistant to our recent potent insecticides. Also unwelcome news is the report of pest ticks in South Africa that are resistant to what were proving to be efficient acaricides. Whitnall and his colleagues have reported populations of *Boophilus decoloratus* which became resistant to standard arsenical dips and subsequently to benzene hexachloride, which was proving so beneficial to the stock industry. Toxaphene—used as a spray, but not as a dip—is now the toxicant recommended for tick control by the Kerrville Laboratory. The toxicity for domestic animals of this material requires careful management in its application, as recently reported by Radeleff and Bushland.

Although we know the rearing requirements of many Acarina, we are only slowly realizing the important parts different ecological factors play. Different species have different moisture requirements. Some chigger mites require constant high humidity, and in New Guinea repellent patch tests had to be conducted by Bushland in jungle tents rather than in the open laboratory less than a quarter of a mile away. Some *Ixodes* species require higher than ordinary humidity for storage and feeding, particularly *I. holocyclus*, the Australian paralysis tick. Some *Ornithodoros* live under extremely dry, desert conditions, and their tough, rugose bodies are well suited to resist desiccation.

Light has only barely been examined as a factor, although we have known that larvae or nymphs of some species like *D. andersoni* will often remain attached on animals in semidarkness, such as in a burrow or cage, until suddenly lighted by emergence of the rodent, exposure of the cage, or removal of the lid of a capsule, when numbers of engorged specimens let go almost simultaneously. This ensures better conditions for the resulting adults.

Some species of both mites and ticks are blind; others have simple eyes with which they undoubtedly react to light. Garnett and Sachtor reported that unilateral light sources of between 5 and 40 foot-candles affected their repellent tests by attracting *A. americanum*. Jenkins found he could train chiggers to respond either negatively or positively to light by conditioning. Smith and Cole showed that immature stages of *D. variabilis* were stimulated by gradually increasing the photoperiod of exposure corresponding to lengthening days in spring. We may need to pay more attention to these factors in experimental rearing and vector studies.

Dragging a muslin flag over tick-infested vegetation has become a standard technique for collecting appropriate stages. This method was used over a period of years in the Bitterroot Valley to study behavior of adult *D. andersoni* in two ways: (1) Their natural occurrence and persistence on a surveyed 40-acre tract, and (2) the tendency of liberated, marked ticks to migrate toward bait or away from a central station.

The studies showed quantitatively that peaks of abundance in the Bitterroot occurred in six successive years during the first two weeks in April regardless of "early, average, or late" spring, and also their almost complete disappearance by mid-June with arrival of hot weather. Ticks remained on the vegetation during rainstorms and could be collected at midnight, but were less alert during high winds. Ticks marked at the beginning of one season were very occasionally retaken toward the end of the second season. In confinement in nature, unfed *D. andersoni* adults have lasted into a third season. Elk, moose, and cattle ranged freely over the area and were undoubtedly responsible for considerable depletion on the survey tract. One tick with two seasons' colors on it was reported with some alarm by a passing rancher, but he was assured that we were not liberating "poison" ticks.

Second, unpublished observations revealed that marked ticks liberated at 5, 10, 25, and 50 feet under varying conditions and directions around a saddle blanket or other animal-scented bait

Tick "dragging" with a flannel flag in 9-Mile District of Montana—notorious as the first area in North America where Q fever was recovered from ticks.

tended to migrate toward the bait or toward a competitive, used game trail in another direction. Usually less than half the total were recoverable by flagging at a given checking operation, but most of these were either within a short distance of the liberation station or at 25 feet or less, moving in the direction of the bait. In two instances at 5 and 25 feet, where the stations were intentionally established on game trails, as many moved along the trails, up to 25 feet, as toward the bait. The slope or levelness of the terrain appeared unimportant, and travel with the prevailing wind did not seem to be slower than against it within 10 feet of the bait, probably because of nocturnal movement on the part of the ticks when the wind died down. Even in cases of counterattraction such as game trails, or at the 50-foot stations where the attractant was weakest, the marked ticks generally scattered in the direction of the bait, and it is probable that the presence of a bedded animal or an overnight camper would be much more attractive. Live animal bait, such as a caged rodent, was not tried. Very few of the 25-foot station ticks actually reached the bait, although they would be scattered in its direction up to 20 feet. Two to 5 ticks out of 50 from the 5-foot stations generally reached the bait in 24–48 hours, although a number would remain at the point of liberation up to two weeks or more, in spite of apparent repeated capture and reliberation in flagging.

Another method of checking migration was by the liberation of batches of 125 marked ticks at central points and flagging the surrounding areas daily. The percentage of ticks recovered at any one flagging was low, but was sufficient to indicate trends on a south slope compared to a level exposure with similar vegetational cover, through a week of prevailing westerly winds. During the first three days of cool weather none was caught beyond 10 feet of the point of liberation. The first warm day, however, accelerated the general movement upwind from both stations to 15–20 feet; 50 and 26 ticks were beyond 5 feet on the upwind side and only 4–9 had moved on the downwind side at this time. This general disproportion in the two directions continued during the next three weeks, although the actual stimulant was not determined, since there were no nearby game trails to interfere, and the direction of the wind per se may or may not have been responsible. On the twenty-

fourth day 3 had moved upwind 35–40 feet, and only 3 in all were taken between 5 and 12 feet from the downwind side of the hill station; at the level station 5 were taken 15–25 feet upwind, and the same number only between 3–9 feet. Thirty-one and 32 were recaptured at this late flagging.

The tendency to concentrate along game trails and roads is well known, although the repeated accusation that ticks drop out of overhanging branches or jump off the vegetation onto passing horseback riders is not credible. It appears that they respond to adjacent hosts by scent, vibration in passing, passing shadows, or combinations of these. William Mann, traveling in Persia in 1915, noticed that ticks with long legs infested sandy, arid, scrub areas, where stock were accustomed to lie in shelter. He called them "cursorial ticks" because they pursued him within a distance of 10 feet even though he changed his direction. The species was identified for him as *Hyalomma savignyi,* an ixodid. The female has a spiderlike appearance, and the species is a known transmitter of disease to animals in the Far East and Africa.

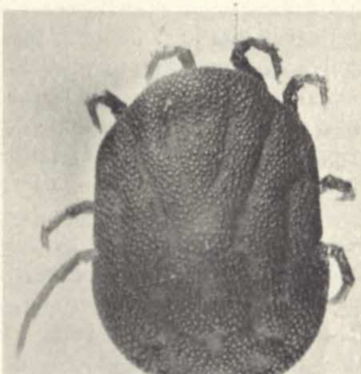
I should like to terminate this "Tick Talk," with a quotation from Holland's *Moth Book,* which I'm sure has stirred many of us in our younger "bug-hunting" days, as it did me:

When the moon shall have faded out from the sky, and the sun shall shine at noonday a dull cherry-red, and the seas shall be frozen over, and the ice-cap shall have crept downward to the equator from either pole, and no keels shall cut the waters, nor wheels turn in mills, when all cities shall have long been dead and crumbled into dust, and all life shall be on the very last verge of extinction on this globe; then, on a bit of lichen, growing on the bald rocks beside the eternal snows of Panama, shall be seated a tiny insect, preening its antennae in the glow of the worn-out sun, representing the sole survival of animal life on this our earth—a melancholy "bug."

He could logically have substituted "mite" for "bug", because a generalized, nonparasitic, lichen-eating oribatid mite could as easily have been that last surviving arthropod.

Brues has found some water mites in thermal springs as hot as 50.8° C and with a pH of over 9.6. Had Holland lived into this decade to write his fanciful epitaph, considering the newer conception of the ultimate engulfment of the earth by an expanding sun, as depicted popularly in *Time* (Nov. 20, 1950), he might have concluded a mite differently, such as

. . . in the futilely protective shadow of a protruding bit of agatized rib of a long-since extinct whale, itself the last remnant of a once domineering type of animals called vertebrates that barely survived the catastrophic developments of the so-called atomic age when the world was young and lovely for living things, . . . in this shimmering shadow in a now bleak and almost lifeless world steaming in the writhing heat of a merciless, expanding ball of fire in the heavens, and beside the ever-boiling, cauldron springs of the North Pole, crouches an anxious, solitary water mite, the sole survivor of animal life upon this agonized, liquidating planet. There will be no chronicles of her life cycle, and the validity of her scientific name shall be a matter of great unimportance.



The Role of Cybernetics in Physiology*

STANTON H. COHN and SYLVA M. COHN

Dr. Cohn has been with the U. S. Naval Radiological Defense Laboratory, San Francisco Naval Shipyard, since 1950, and is at present head of the Clinical Radiobiology Branch of the Biological and Medical Sciences Division. He previously worked for two years on the chemistry of explosives, then spent three years with the U. S. Army in Europe in the chemistry laboratory of a general hospital, and four years as a research radiochemist. Mrs. Cohn had done research on cathode-ray tubes and servomechanisms. She was head mathematician for the Special Operations Division, Operations Analysis Section, of the San Francisco Naval Laboratory when she retired in 1951 to take up the more exacting duties of domestic life.

Those engaged in arts and science, most Gracious Emperor Charles, find many obstacles to the exact study and successful application of them. In the first place, no slight inconvenience from too great separation between branches of study which serves for the perfection of one Art. But much worse is the mischievous distribution among different practitioners of the practical application of the art. This has been carried so far that those who have set before themselves the attainment of an art embrace one part of it to the neglect of the rest, although they are intimately bound up with it and can by no means be separated from it.

—VESALIUS (1542)

We have inherited from our forefathers the keen longing for unified, all-embracing knowledge. The very name given to the highest institutions of learning reminds us, that from antiquity and throughout many centuries the *universal* aspect has been the only one to be given full credit. But the spread, both in width and depth, of the multifarious branches of knowledge during the last hundred odd years has confronted us with a queer dilemma. We feel clearly that we are only now beginning to acquire reliable material for welding together the sum total of all that is known into a whole; but, on the other hand, it has become next to impossible for a single mind fully to command more than a small specialized portion of it.

I can see no other escape from this dilemma (lest our true aim be lost forever) than that some of us should venture to embark on a synthesis of facts and theories, albeit with second-hand and incomplete knowledge of some of them—and at the risk of making fools of ourselves.

—SCHRÖDINGER (1945)

IN THIS age of specialization where one man is the master not of a whole field of thought but, rather, of a very small portion of that field, the need for correlation of the various special sciences into an integrated, coherent picture is becoming daily more urgent.

In 1542, Vesalius[1] wrote the words quoted above, deploring the state of affairs in which the scientific worker was separated from a complete knowledge of the field of his endeavor. In the four hundred years that have elapsed since the time of Vesalius, the situation has grown much more acute for the individual, and today the plea for correlation of the various branches of science is reiterated by such leading scientists as Schrödinger, whose statement is also quoted here.[2]

* The opinions or assertions contained herein are the private ones of the authors and are not to be construed as official or as reflecting the view of the Navy Department or the Naval service at large.

We may not again see another Leonardo da Vinci, "perhaps the most versatile genius of whom we have any record,"[3] who was proficient as a painter, mathematician, philosopher, scientist, musician, and engineer. Since his time few men have taken the whole of knowledge for their province. One of these was Gottfried Leibnitz, who has been characterized as "the Aristotle of the Seventeenth Century"[3] and who was perhaps "the last human being to undertake the mastery of all existing knowledge."[3] Leibnitz was noted as a "brilliant mathematician, physicist, geologist, philologist, historian, and editor" and "an important personage in public affairs."[3] In the nineteenth century another scholar, Hermann von Helmholtz, made notable contributions to a number of fields—medicine, neurophysiology, physics, mechanics, aerodynamics, and mathematics.

The appearance of so few general scholars among the many learned men of the past few

centuries is closely related to the growing tendency for knowledge and learning to be treated along increasingly specialized channels, with the universal aspects receiving correspondingly less attention. With the growth of scientific knowledge, it may be only by the unification of the language and basic concepts of various fields of learning that man can once more attempt to embrace a large area in the field of knowledge. As Dingle points out in his provocative and stimulating book *Through Science to Philosophy,*[4] elementary experiences have been rationalized into schemes known as physiology, psychology, biology, mechanics, thermodynamics, etc., each covering only a portion of the whole field of phenomena, and each expressed in a particular set of terms and a particular set of laws. All the fields of scientific endeavor have similarities, and advances in the techniques and organization of one field may very well be applicable to another. When, however, one tries to go outside his field into a neighboring one, the difficulties of learning the new language and the new laws are almost insurmountable obstacles to comprehension. Much of the work to be done, therefore, in the way of scientific endeavor is that of correlating these separate fields into one homogeneous rationalization. As Dingle points out, it is in the borderline area between the various fields that the greatest advances are to be made. Our century is witnessing a wide-scale invasion of this territory, but the tradition goes back at least to the seventeenth century.

Application of the Physical Sciences to Physiology

Some of the earliest attempts to apply the concepts of physics to biology were made by Santorio and Borelli, two exponents of the iatrophysics school of the seventeenth century. The tradition was carried forward by Descartes who, conceiving the body as a machine with the heart acting as a pumping plant, worked out his hypotheses along physicophysiological lines. Similar progress can be traced in the iatrochemical school of the seventeenth century, beginning with Von Helmont and Du Bois. In the eighteenth century, Lavoisier made further advances in delineating the chemical cycle of life. At this time, also, Galvani discovered animal electricity, or "galvanism," and Volta did further work on the application of the electricity of physics to living organisms. This correlation of the physical and physiological sciences has been continued and enlarged upon up to the present day.

Some of the pioneers in this work of correlation at the present time are Norbert Wiener and the group of men associated with him. It is just this challenge of the correlation of two specialized fields of knowledge that Professor Wiener wishes to meet. He says of his purpose:

> For many years Dr. Rosenblueth and I had shared the conviction that the most fruitful areas for the growth of the sciences were those which had been neglected as a no-man's land between the various established fields. . . . It is these boundary regions of science which offer the richest opportunities to the qualified investigator.[5]

Wiener and his associates are tying together several fields of knowledge: the physical sciences mathematics, statistics, and electrical engineering, and the biological science neurophysiology. The focal point of their activities—the common denominator for these various fields—is the interest in machines which compute and control, and the systems in the human mechanism which perform similar functions.

The name given to this new, unified field of endeavor is "cybernetics," taken from the Greek word for "steersman."[5] The word "governor," a Latin corruption of the same basic Greek word, is closely related in meaning and can be said, in the new terminology, to describe a "cybernetic device."

The story of the development of cybernetics extends back only ten years. The first important date in cybernetic history is 1942, the year in which Rosenblueth, Wiener, and Bigelow presented a new approach to the study of voluntary human activity in a paper presented to a meeting held under the auspices of the Josiah Macy, Jr. Foundation.[6] This paper was published in 1943. In the winter of 1943 Wiener and von Neumann, of the Institute for Advanced Study, convened a meeting of various persons who had become interested in the new ideas being promulgated, in which engineers, physiologists, and mathematicians participated.[5]

By 1946 there was greatly accelerated progress of the new work. In the spring, the neuropsychiatrist Warren McCulloch arranged several meetings, again sponsored by the Macy Foundation, in which psychologists, sociologists, anthropologists, neuroanatomists, and neurophysiologists took part. Cybernetic ideas were thus widely disseminated, and it was decided to meet semiannually for an exchange of ideas among the various groups represented. The results of these further meetings have been presented in a series of papers on cybernetics.[7-9]

In 1946, also, an experiment of prime importance—one of the first trials of the new theory—was performed by Wiener and Dr. Rosenblueth at the Instituto Nacional de Cardiología, in Mexico City.[5] In 1947 a second work of great cybernetic

importance, that of "designing an apparatus to enable the blind to read the printed page by ear" was performed by Dr. McCulloch and Mr. Pitts. On the basis of this work, these two men developed a new theory of the physiology of the visual cortex, drawing on anatomical, mathematical, and engineering principles, which was presented at a 1947 meeting.[5] It was at this time that the term "cybernetics" was devised to cover the new field of activity. In the fall of that year Wiener began work on his book, which was published in 1948.

The development of cybernetics, however, has its roots in the last century of work in mathematics, physics, engineering, and physiology, and in order to understand the work of Wiener and his associates, it is necessary to understand the background from which he drew his inspiration.

Physiology in the Age of Power

The nineteenth century was the "Age of Power." It saw the development of the machine, and concomitant with it there arose a mechanistic philosophy of life and a mechanistic interpretation of the life processes. If there is one law that marks this era definitively, it is the principle of the conservation of energy. This principle, which is expressed in the first law of thermodynamics, has been characterized as the greatest generalization in natural science. But it is not the final word.

It is true that many phenomena can be described by application of the energy principle alone, as is borne out by the remarkable success of Rashevsky in applying this principle to processes of biology and physiology.[10] Rashevsky expresses his profound belief in the energy principle as the basis on which to build biology: "All human activities may be considered as biological manifestations and the principle of maximum energy flow may be applied to all of them. . . ."[10]

Nevertheless, it is now recognized that the energy principle, if applied exclusively, leaves no room for the explanation of the role of information and intelligence in any rationalization of the ordered life processes. Indeed, it does not provide an adequate basis for the description of life, either, as is well brought out by the fact that physicists and biologists are split on the question of whether some new principle is needed to explain life.

Brillouin, in a searching article entitled "Life, Thermodynamics, and Cybernetics,"[11] classifies those who hold opinions on this subject into three groups: those who believe that life can be explained in terms of the present concepts of physics and chemistry; those who feel that "something more is needed before we can understand life;" and those who believe that life cannot be understood without reference to a "life principle," and that the behavior of living organisms is completely different from that of inert matter. That there can be such a diversity of opinions would lead one to believe that the application of the energy principle which has dominated scientific thought up to the present decade has not met the requirements for a basic principle that will unify the physical and biological sciences.

Science has advanced beyond the mechanistic stage, however. Just as the nineteenth century was the Age of Power, the twentieth century is the Age of Communication and Control. It is not enough to make a powerful machine, having the ability to do many times the work of man. There must be an intelligent application of this energy—it must be controlled. Just so, the human body has intelligence which utilizes information in directing its activities. The study of these in both mechanics and physiology is enlarging our field of knowledge at the present time. As it happened, much greater progress was made in the fields of electronics and mechanics in the systemized study and presentation of control and communication. But since there are parallel functions in the field of physiology, it remains to apply the principles formulated to the latter field. This, then, is the work that Norbert Wiener has undertaken. He summarizes this by saying:

> In the nineteenth century . . . all the fundamental notions are those associated with energy, and the chief of these is that of potential. The engineering of the body is a branch of power engineering. Even today, this is the predominating point of view of the more classically minded, conservative physiologists. . . .
>
> Today, we are coming to realize that the body is very far from a conservative system. . . . In short, the newer study . . . is a branch of communication engineering. . . .[5]

The discussion of cybernetic principles can be grouped around two very important concepts: the servomechanism, and negative entropy.

As mentioned above, the wonderful machines that have been developed in the past hundred years or so need intelligence for their operation. What is the nature of the machine brain that permits our factories to operate like automata? The key to this story lies in an elucidation of the servomechanism. Literally translated, servomechanism means "slave mechanism." It is a system whereby a signal, or bit of information, with a tiny amount of power, controls and directs large amounts of power capable of doing much useful work—thus the machine becomes the slave of the intelligence.

The servomechanism is not a particular, single

machine. Rather, it is a closed loop system, a *scheme* for setting up various types of equipment. Many of the systems of the body appear to function in such a manner that the elements involved can be said to be arranged as the elements of a mechanical servomechanism, and the mathematical techniques that have been worked out for the mechanical "servo" are now being reworked to apply to biological and physiological phenomena.[12]

The science of automatic control and the theory of the servomechanism—a development of the past fifteen years—have led Wiener into new techniques and theories on the understanding of human activities. The central concept of which Wiener has made use is that of negative feedback, as Eisenhart points out in an excellent review of cybernetics.[13] The feeding back of a signal from the controlled member is the heart of the whole servomechanism loop and makes its behavior different from the responses of other systems.

The performance of a servomechanism may be characterized as follows. Information in the form of messages or stimuli is fed to a system by pickup devices or receptors. These messages are sent through communications means—circuits or nerves—to a central computing system, or brain and reflex center. This center then compares the incoming signal with a desired value, and operates on the difference, or error, to produce a signal that is then fed to the power equipment, or effector organs. The latter then move in such a way as to alter the conditions producing the incoming information, and bring it closer to the desired value. Especially to be noted are the closed loop nature, or circularity of the system, and the fact that the system embraces the object upon which it operates. As a matter of fact, in mathematical analyses of mechanical systems, it is not possible to distinguish between the controlling and controlled portions of the systems.

This analysis has been well established for mechanical automatic control systems, but it is new and highly revealing when applied to the human organism.

Wiener provides an excellent illustrative example. Consider the action of picking up a pencil. This action is initiated by the desire to pick up the pencil. Note, as Wiener points out, that we do not will ourselves to move in a particular sequence a certain group of muscles. The receptors feeding information into the control loop are the eyes, which give information on the position of the object. This information is sent to the computing center, the brain, via the nerves. The brain then computes the amount by which we have not yet picked up the pencil—that is, the error of the system. We then have the difference in positions of our hand and the pencil. At the same time, this scheme also requires that information be sent into the central computing system regarding the position of the arm and the hand with relation to the body. Part of this information is visual, but not all. Some is conveyed by means of the proprioceptive sense.†

The brain acts upon the information received, and produces a proportional or more complicated type of response. This signal is then sent through the nerves of the efferent nervous system to the muscles, which then perform the activity that will bring about the accomplishment of the desired event.

We would expect that if the servo theory were indeed to hold true, it should be possible to verify by experiment the mathematical prediction of the performance of the system. A highly interesting experiment of this type was carried out in 1946. It is known that certain adjustments of servo systems will cause these systems to go into oscillation. The frequency of the oscillation is a function of the values of the system parameters. Wiener and Rosenblueth performed the following experiment. They studied one of the muscles of a cat—the quadriceps extensor femoris—by cutting the attachment of the muscle, fixing it to a lever under known tension, and recording its contractions when the muscle was loaded to the point where a tap gave rise to automatic jerking movements, usually spoken of as clonus. They thereupon compared the frequency of oscillation in relation to the frequency as predicted from the "constants" of the system. Although the system is not linear, and thus analysis becomes rather complicated, it was possible to make a mathematical estimate of the period of vibration, which was found to be 13.9 cycles per second. Observed oscillations varied between 7 and 30 cycles, but generally remained within the range of 12–17 cycles per second, which shows remarkably good agreement for such a problem.

Although the feedback mechanism is the central theme of cybernetics, another factor is of great importance also, and that is communication. There can be no control unless some message is fed into the system as a command signal, and there will be no feedback unless a signal is sent over communica-

† Proprioceptive sense: The tendons, joints, and skeletal muscles contain receptors which respond to the stimuli of pressure and stretch. These receptors supply information via afferent fibers to the central nervous system regarding the position and movement of the part of the body in which they are located.

tion circuits. Thus Wiener, in developing cybernetic theory, is forced to include the science of communication. In making his analysis, Wiener found that the amount of information in a system, which is a measure of its degree of organization, can be measured by a quantity which turns out to be the negative of the physical quantity called entropy. What is entropy, and what is its physiological importance?

It has been found in thermodynamic experiments that there are processes that are irreversible, not in that any energy is destroyed in them (as, indeed, this is not possible), but only in that the quality of the energy (as measured by its ability to do useful work) is lowered. This process is called "thermodynamic degradation," and it is measured by the quantity entropy, which is formulated in precise mathematical terms. Moreover, it has been found that irreversible processes result in an increase in entropy, whereas reversible processes are marked by constant entropy. Inasmuch as all processes fall into one or the other of these two classes, the second law of thermodynamics has been formulated: "The entropy of the universe is increasing."

A body in thermodynamic equilibrium—an undifferentiated body, or one in a high state of disorder—has a high entropy. A body in a complex (i.e., a well-organized, or differentiated) state, has a low amount of disorder, or a low entropy. The amount of "order" possessed by a body can be expressed by the reciprocal of the quantity that expresses the disorder, or entropy, according to Schrödinger.[2] This quantity, the reciprocal of entropy, is called "negative entropy," or "negentropy."[14]

Wiener suggests that perhaps man may be considered a "metastable Maxwell demon," which decreases entropy. Schrödinger has offered the hypothesis that man feeds on negative entropy, "to compensate the entropy increase he produces by living and thus to maintain himself on a stationary and fairly low entropy level."[2] Brillouin sees brilliantly the connection between Wiener's concept of information or degree of organization as negative entropy, and Schrödinger's concept of the complex organism as a body feeding on negative entropy—i.e., organization and order—and from these suggests that a generalized description of all the ordered processes, among which is life, can be made.

Heretofore, the first and second laws of thermodynamics have been applied only to isolated, or "closed," systems. No way has yet been devised to measure entropy in "open" systems, the class of systems that includes all living organisms. Thus, although progress has been made in defining entropy in open systems qualitatively, it remains to find a way to make a quantitative measurement of entropy in these systems. This problem is of the greatest importance, because, as Brillouin points out, "it does not make sense to speak of a quantity for which there is no operational scheme that could be used for its measurement."[11]

Several investigators have already attacked the problem of making the generalized concept of entropy applicable to the new situations in which it is being considered. Brillouin discusses some of the problems arising from the application of the concept of negentropy to various types of information.[14] Ostow has done further work on this problem, in taking up the application of the entropy concept to psychic function.[15] Raymond has put forth the suggestion that the entropy of a system be defined as "the sum of the positive thermodynamic entropy which the constituents of the system would have at thermodynamic equilibrium and a negative term proportional to the information necessary to build the actual system from its equilibrium state," with the definition applied to open systems by closing them momentarily.[16]

We seem to stand on the brink of understanding; perhaps here is the clue that will determine whether life is to be explained in terms of known physical principles, or whether a new element must be added—the enigma that puzzles so many of our leading scientists today.

References

1. Vesalius, A. Trans. from *De Humani Corporis Fabrica.*
2. Schrödinger, E. *What Is Life?* New York: Macmillan (1945).
3. Barnes, H. E. *An Intellectual and Cultural History of the Western World.* New York: Random House (1937).
4. Dingle, H. *Through Science to Philosophy.* Oxford: Clarendon Press (1937).
5. Wiener, N. *Cybernetics, or Control and Communication in the Animal and the Machine.* New York: Wiley (1948).
6. Rosenblueth, A., Wiener, N., and Bigelow, H. *Philosophy of Science,* **10,** 18 (1943).
7. Foerster, H. von, Ed. *Cybernetics: Circular, Causal and Feedback Mechanisms in Biological and Social Systems.* Transactions of the 6th Conference, March 24–25, 1949. New York: Josiah Macy, Jr. Foundation.
8. *Ibid.* Transactions of the 7th Conference, March 23–24, 1950. New York: Josiah Macy, Jr. Foundation.
9. *Ibid.* Transactions of the 8th Conference, March 15–16, 1951. New York: Josiah Macy, Jr. Foundation.
10. Rashevsky, N. *Mathematical Biophysics.* Chicago: Univ. of Chicago Press (1948).
11. Brillouin, L. *Am. Scientist,* **37,** 544 (1949).
12. Kreezer, G. L. *Science,* **110,** 681 (1949).
13. Eisenhart, C. *Ibid.,* **109,** 397 (1949).
14. Brillouin, L. *Am. Scientist,* **38,** 594 (1950).
15. Ostow, M. *Ibid.,* **39,** 140 (1951).
16. Raymond, R. C. *Ibid.,* **38,** 273 (1950).

The Engineer and the Fundamental Sciences

C. G. SUITS

The author is a physicist, whose specialties include nonlinear circuits, vacuum tube methods of harmonic analysis, and the study of high-pressure arc temperatures by sound waves. He has been with General Electric Company ever since he took his D.Sc. at the Technische Hochschule, Zurich, in 1929. From 1940 until 1945 he was assistant to the director of research; then he succeeded William D. Coolidge in that position. His article is based on an address given at Yale University during the celebration of the centenary of the School of Engineering last October.

IN DEVELOPING my thesis, I will have much to say about technology, by which I mean that complex of science, engineering, and experience that is the foundation of industrial know-how, and which depends heavily upon engineers and scientists for its present strength and future development. Here I wish to examine the relationships between science and engineering and, particularly, to point out how highly developed and interdependent these relationships have become in the research and advanced engineering phases of a highly technical industry. To gain perspective, let us look briefly at engineering and science in the early days of American industry.

Primitive Industry and Early Science

It is clear that, at the start of the industrial revolution, around 1770, and during the industrial development that extended into the 1850s, empiricism rather than science was the rule. During this period, neither science nor engineering was recognized as a component of primitive industry. The foundations of modern industrial technology were, however, being laid in the scientific work of this same period. In the electrical industry, modern technology is built upon the early work of Coulomb (1736–1806), Ampere (1775–1836), and Faraday (1791–1867), followed by Maxwell (1831–79), Hertz (1857–94), Henry (1797–1878), and many others. Their researches, largely accomplished in a university environment, provided the observational facts and interpretation that became the basis for electrical engineering practice. The mechanical industries were similarly generated in fundamental researches of Newton (1642–1727), Carnot (1796–1832), and Joule (1818–89), among others. In chemistry, I suppose the names of Lavoisier (1743–94), Priestley (1733–1804), Scheele (1742–86), and Dalton (1766–1844) must be reckoned among the pioneers whose work was later to support the engineering foundations of the chemical industries. The few names I have mentioned by no means complete the list of pioneers of science upon whose work, translated into engineering terms and techniques, industry built a large structure.

The development of machines, especially Watt's steam engine, initiated the factory system and the Industrial Revolution, but the primitive industry that was thereby born drew but little of its strength from science and engineering. Watt's steam engine, Cartwright's power loom, Hargreave's spinning jenny, the cotton gin of Eli Whitney, and many similar machines, employed in their development a very small portion of science and a large content of mechanical inventiveness. The later phenomenal growth of modern industry could not have taken place, however, without the foundations in early natural science which developed independently of, and concurrently with, primitive industry.

The Engineer in American Industry

The real beginnings of engineering are of considerable antiquity, but the trained engineer appeared on the American industrial scene around 1850, at about the time many of our engineering schools, including the Yale School of Engineering, were founded in this country. The period between 1850 and 1900 was, among other things, the age of inventors. In addition to the trained engineer, who was making his way into industry, the early inventor played an important role in the transla-

tion of scientific facts into embodiments of practical utility. Industrial technology during this period was not yet so involved, nor basic scientific knowledge so complex, as to require intense specialization. Hence, a single talented individual could comprehend a broad area of industrial skill and related technical knowledge and could excel in conspicuous individual inventiveness. In the industry with which I am associated, it was the inventive genius of Bell, Stanley, Elihu Thomson, Edison, and others who translated the science of Faraday, Henry, Maxwell, and Hertz into products capable of manufacture and sale.

The increasing complexity of both science and its application led to the gradual development of engineering and of the engineer as the specialist who reduces the laws of natural science to the more or less exact practice of analysis, design, and construction. Of course, engineering does not concern itself solely with the application of science. Although this is a large and increasing content of engineering practice, the application of experience—and especially the extrapolation of experience—are important functions of engineering. The traditional inventor, as an individual with great natural aptitudes and a smattering of science, has almost disappeared as a logical consequence of the increasing technological complexity. Natural aptitudes of inventiveness have not, of course, disappeared. They have simply ceased to be individually conspicuous because, in modern technology, individual effort merges into a background comprising a great manifold of highly specialized technical skills.

The Birth of Modern Science

Consider the year 1890 and some of the significant events associated with it. It is well known that the scientists of the time felt that most of the fundamental laws of matter—represented by the knowledge of electricity, magnetism, mechanics, and thermodynamics—had been observed and interpreted, and that refinement of detail rather than new discovery would comprise the future of science. The electrical industry had its beginnings at about this time, originating in inventions based on the work of early electrical scientists. With minor exceptions, however, no scientists were employed by industry—a remarkable fact, in view of the many applications of science that were being made at that time. This situation can be understood, however, in terms of the traditions of primitive industry and the philosophy of early science. Early scientists felt that the adapting of the laws of nature to the practical uses of man involved a stigma, and that science was pure only if it was sterile. The men of early industry, on the other hand, knew that their experts had been trained almost entirely by working experience; hence, the concept of academic training for industry was contrary to all practice. It cannot be said that these early ideas have been thoroughly dissipated even now. I hope that my readers do not share these feelings concerning the debasement of science, for I shall have occasion to describe brazen instances of the application of new scientific discoveries to practical use; and I may as well confess that such cases give me much inner satisfaction. Furthermore, such wickedness seems to be on the increase!

The well-defined boundaries which science seemed to have in 1890 soon disappeared. Roentgen discovered his famous X-rays in 1895, and Becquerel discovered the radiation emitted by uranium in 1896. The electron was discovered by many investigators, but was first identified and interpreted by J. J. Thomson in 1897. In 1900 the Curies discovered radium, and Max Planck formulated the beginnings of the quantum theory. In 1903 Rutherford and Soddy correctly described and interpreted natural radioactivity. This span of eight years was in that year and against this background that the world had seen, and the era of modern science had its unmistakable origins in this flood of important new discoveries. The most significant result of this exciting period was the disappearance of the previously well-defined boundaries of science; it is a remarkable fact that, more than fifty years later, in spite of tremendous research effort in laboratories throughout the world, the boundaries to new scientific discovery cannot even now be seen. Before 1900 industry had not yet embraced science, but it was about to do so, for it was in that year and against this background that the first scientific research laboratory was established in industry. Scientists thus began to play a direct role in industrial organizations and to develop their functional relationships to engineering.

By 1900 the engineer in industry and his industrial functions were well established. To be sure, engineering was by no means as specialized then. In General Electric, for example, only four specialized skills in engineering could be distinguished: civil, mechanical, mining, and electrical engineering. The engineer in industry was likely to be engaged in a variety of technical activities involving the design, construction, testing, and operation of industrial equipment and processes. He applied what science was available to him, but large factors of experience, judgment, and courage entered into his designs. This is, of course, no less true today.

Around 1900 the engineer and the scientist could be clearly distinguished as separate professional specialists. In the first place, the engineers were to be found largely in industry, and the scientists were almost entirely in the universities. Scientists in industry were somewhat apologetic concerning their defection from professional tradition; and industrial engineers, on the other hand, were equally apologetic over any dealings with scientists, who were known to be a thoroughly impractical and visionary lot.

This distinction between the scientist and the engineer was, moreover, aided by dress and demeanor. The scientist of the times was likely to look like a scientist in the classical sense, whereas the engineer frequently looked the part and affected a manner befitting a thoroughly practical man. When I first came to industry, one could still find in our Schenectady plant a number of picturesque individuals who were reverently pointed out as "real" engineers. They were pioneers who had grown up with the electrical industry and who embodied the early traditions of the profession to a conspicuous degree. They wore their hats at work, indoors and out, occasionally spat upon the floor, and were somewhat given to profanity. They were distrustful of theories, especially those expounded by young men just out of college. New developments in science reached them by an exceptionally circuitous route, if at all, and they were almost entirely spared from direct contact with scientists in the normal course of their engineering activities. That professional complexity was to come later! They may have remembered, in contemplative moments, the period some forty or more years before when they were young engineers entering industry. *They* were *then* the impractical technicians with their theories and their mathematics, who subsequently had to establish their worth with the practical men in the infant, but rapidly growing, electrical industry.

In the early days, the technologists were the practical men, aided and abetted by inventors, and guided in analysis and design by engineers who first translated the electrical phenomena of Henry, Faraday, and Maxwell into particular embodiments of sufficient utility to permit the beginnings of a manufacturing enterprise. Engineers soon became the keepers of industrial technology, and they have for many years carried forward its development. Scientists appeared on the industrial scene fifty years after the engineers, and their work has contributed to expansion and diversification, particularly in the electrical, chemical, and communications industries, and has led to the great present-day growth of industrial laboratories.

Although the process has been going on for some fifty years, the extent to which science has now been integrated into the industrial complex has not been generally appreciated. For example, the early and successful employment of scientists and engineers in the electrical, chemical, and communications industries has not by any means established a pattern throughout industry. There are manufacturing businesses in our country, with an annual sales volume of hundreds of millions of dollars, which employ no scientists and few engineers. The extent to which science and engineering have been applied to industrial technology thus varies widely with industries. In some industries the techniques of employing engineers and scientists have hardly been explored, and in others scientists and engineers work together on complex technologies as vital to the business as its manufacturing and sales functions.

Technological Complexity and Professional Specialization

The development and increasing complexity of industrial technology have been paced by the increasing technical specialization of its practitioners. In engineering alone there is now a great variety of technical specialization. Although the engineering curriculum at many schools is extensive, I suspect that there are still some engineering specialties not included in training programs. The electrical industry originally employed only four professional categories of engineering, whereas the number is now very much greater. In the G-E General Engineering Laboratory, one can distinguish sixty-two separate fields of engineering, and the company-wide total undoubtedly would be much greater. A further index of specialization is provided by the number of national technical societies that are now in existence. In the field of engineering, for example, there are at least 135 national engineering societies in this country.

The same specialization has taken place among scientists and their national professional societies. The terms "physicist," "chemist," and "metallurgist" are now useful only as broad generalizations; they no longer denote definite fields of scientific specialization. It is not possible, at present, to be a specialist in physics. One might hope to encompass the specialized knowledge associated with a narrow field of physics, if this field is not expanding too rapidly; for example, classical acoustics, sound, heat transfer, and thermodynamics may present this possibility. But in the rapidly growing fields of modern physics this is impossible. Some

years ago when the atomic nucleus was a novelty, one might have specialized in nuclear physics, but today the field is too broad and there are many specialists within this area, including reactor, radiation, and cosmic ray physicists. A similar, but less extensive, degree of specialization is probably developing currently in solid-state physics, where spectacular progress is being made in the art and science of the semiconducting elements.

It is a fact that modern developments in science and engineering are becoming more and more complex. As a first approximation to the truth, one is tempted to conclude that all the simple discoveries have already been made, but this, of course, could hardly be true. Each new tool of science opens up a new vista that could not be seen with the older, and generally simpler, tools. The electron microscope has unlocked a world that light microscopy could not probe. With microwaves, details of nature are seen that escape kilometer waves. Particles with energies of several hundred million volts resolve nuclear phenomena that exhibit no detail at lower energy. The storehouse of nature, once a simple structure, is now revealed as a vast edifice of ever-lengthening corridors adjoining more and more intricate inner chambers. The translation of these complexities of nature into the service of mankind is an important industrial function which employs all the subdivisions of professional specialization lying between exploratory fundamental research and production engineering.

The path from a complex new discovery to practical utility is frequently long and distressingly expensive. Bringing nylon to market is vastly more difficult than bringing cotton or wool to market. Crawford H. Greenewalt, of Du Pont, has stated that ten years and $27,000,000 were required to accomplish this particular modern miracle. The accumulated costs, as an index of scientific and engineering effort, that have gone into the development of television (which I hesitate to call a modern miracle) have probably not been computed, but the total certainly would be staggering. One need hardly mention the cost and complexity of bringing some of the military developments, especially those in the atomic field, to practical utility. The two-billion-dollar cost of the first atomic bomb is familiar to everyone. The continuing cost of bringing out improved yearly models accounts for an appreciable fraction of the acute sensation of pain annually experienced by our citizenry on March 15. Hundreds of millions of dollars have already been spent in developing atomic fission as a source of power, but certainly a billion dollars will be spent before economical atomic power plants are built. Fortunately, the military applications of uneconomical nuclear reactors are important, and their development will help to pay the bill. I mention these examples—and many more could be cited—of the great cost and complexity of important modern developments of science and engineering. There are undoubtedly modern developments of science and technology that are simple in concept and in translation to practical use, but they are not numerous, and they will be more rare in the future.

The Scientist and the Engineer in Modern Technology

Let us examine some of the consequences to science and engineering that result from this technological complexity. For example, Does the overwhelming weight of planned programs of research mean that exploratory fundamental research has disappeared? I think not. The fact that vastly greater funds flow into the area of applied science does not impress me as evidence to the contrary. I think it is true that fundamental science has never before been so well supported. Although nuclear research is an exception, the fundamental research stage of the technological process is generally not an expensive one, at least not in comparison to the applied research and engineering development that may follow. Hence, in this area, a small amount of money goes a long way. In spite of the great cost of fundamental nuclear research, vast sums have been made available for this work by the Atomic Energy Commission. The enlightened policies of the Office of Naval Research and the recently established National Science Foundation are designed to encourage fundamental science, and they actually do provide generous financial support. Industry is supporting fundamental research to an ever-increasing degree, partly in its own laboratories and partly under university auspices. If truly worthwhile research proposals cannot obtain support under present circumstances, the sponsors must be remarkably inept in presenting their case.

How about the individual scientist in this age of cyclotrons, synchrotrons, supersonic wind tunnels, and atomic proving grounds: Is it forever his fate to submerge his individuality into a background of programmed research projects? To the extent that he works with expensive research tools, this is likely to be the case. If a scientist spends ten million dollars building a particle accelerator, even discounting the probable source of the money, he must not expect to find its research use to be a

contemplative solitary endeavor. The mad, three-shift confusion that surrounds some of these machines creates quite a different impression. Fortunately, not all of modern science is paced by million-dollar machines. The study of the solid state of matter is a notable example of a field of research of the very greatest importance, which has not yet developed the expensive tools and inconvenient trappings of atom-smashing. Here, outstanding progress is being made by individual researchers and small scientific investigation teams.

In modern technology, at what point does science stop and engineering begin? In the early days of industry, when the two were quite separate, this question was easily answered. At present, both scientists and engineers man the numerous stations along the stream of technical progress. At the end nearest to design and manufacture, it is mostly staffed by engineers; at the other end, where the laboratories are located, the scientists predominate; in between, the professional groups intermingle, depend upon each other's technical contribution, and may be difficult to distinguish professionally.

What is the impact of modern fundamental science upon engineering training and practice? Time was—and it was an earlier time—when the engineer could assimilate, during formal engineering training, a codified set of the laws of natural science which provided the tools for a career in engineering practice. This traditional idea of stored technical knowledge sufficing for a professional career is fast becoming obsolete, not only in engineering but in many other fields. To be sure, the old rules are still true and still fundamental, but science has been adding to the rule book, so to speak, and the new editions, with appendices, come out so frequently that it is difficult to follow the game. Consequently, engineering is becoming more complex and more specialized, as new subject matter is added at an ever-increasing pace. The engineer who stays in practice finds it more difficult than ever before to remain current in his technical knowledge, particularly if he is isolated from research laboratories and development laboratories. The technical societies play an ever more important role in bringing to their membership the latest information concerning new develpments. It seems to me, however, that a more direct attack on this important problem clearly is necessary. We must recognize the need of replenishing and renewing knowledge as a consequence of rapid technological progress. Perhaps college refresher courses in the latest engineering developments would make it possible for the practicing engineer to augment his fund of specialized knowledge. In any event, it is clear that occasional technical reading will no longer suffice for this purpose.

The scientists who are causing all this trouble are, in turn, in a similar plight. A good scientist, of course, keeps up with the scientific literature, at least in his field of specialization. That could formerly be accomplished by occasionally reading the technical journals in question. But the journals have multiplied in specialization and have become much thicker. Even the abstracts journals in some fields, especially chemistry, have become so thick that there is a need for an abstracts journal to abstract the abstracts journals. If a research scientist were really conscientious about his reading, his research project would start and end in the library, for it would take all his time to read what others are doing in the field in which he intends to work.

Is the engineer subject to more regimentation as a result of the increasing complexity of engineering practice? I think the contrary is likely to be the case. It is undoubtedly true that the efficient pursuit of large research-and-development projects—for example, the development of a nuclear reactor—requires a high degree of coordination of the many technical skills that must contribute to the final result. However, the extensive variety of technical skills employed by modern industry provides, as a corollary, a broad selection from which an individual may choose his field of specialization. Thus, a great range of individual interests may be accommodated within the confines of large coordinated project activity.

What is the saturation point for scientific and engineering manpower in industry, and when will it be reached? Consider the following figures. U. S. industry in the year 1900 employed, on the average, one engineer for every 250 employees. In General Electric at present there is one engineer for every 20 employees. The number of chemists in U. S. industry has doubled in the past fifteen years, and the number of physicists has doubled in the past eight years. Another interesting index is provided by the tabulation of the number of companies which recruit at the colleges. In 1916 only 12 companies did so. By 1929 this had risen to 750 companies; by 1942 the number was 1500; and during 1952, 5200 companies recruited at the colleges. I see no indication that this clear trend in the employment of trained people for the development of industrial technology will not continue long into the future. *Technology is based on science, and science seems to be boundless.* Con-

tinuing research will undoubtedly lead to new developments in science that will lead to new applications to engineering. This technological process appears to be an inexhaustible natural resource.

Some New Technologies

The relationships I have been discussing between fundamental science and engineering will perhaps be clearer if I cite some examples of important and rapidly developing technologies. Many cases come to mind, but a particularly good example is provided by recent research in the field of solid-state physics, where spectacular progress has been made in understanding some of the detailed electrical and mechanical behavior of matter, and where equally spectacular and important progress is being made in the application of the knowledge to practice. The semiconducting elements, of which silicon and germanium are important examples, have provided the best foil for the attack on the electrical properties of solids. Here theory preceded practice (the converse is generally true), and it was only with the discovery of means of drastically purifying the semiconducting materials that theoretically predicted properties were realized. Chemically pure germanium may be hopelessly contaminated, since one part of boron, as a typical impurity, in 100 million parts of germanium will double its electrical conductivity. Skillful experimenters finally discovered means of reducing the impurities in germanium to one part per billion.

Figure 1 shows a laboratory setup for producing pure germanium, and Figure 2 shows an ingot of high-purity germanium which has been made in this furnace. Fortunately, this sample is sufficiently pure to satisfy even the theoretical physicists, who never really expected that anyone would be able to make germanium as pure as they had specified. After we went to all this expensive trouble, the theoreticians said that they did not really want pure germanium after all, but that they wanted to put in their own impurities; and they were particularly partial to indium and antimony. If you put an occasional antimony atom in the germanium crystal lattice, where it fits nicely, the valence electron is surplus and it becomes a conducting electron. Or you may put an occasional indium atom in the germanium lattice. In this case, the *lack of an electron* leaves a sort of "hole" which moves like a positive charge, much like a bubble (which is an absence of something) moves through a liquid.

To make a long story short, if you put some antimony-germanium in contact with some indium-germanium the result is a rectifying junction. This in itself is not remarkable because there are other rectifying junctions—for example, copper-copper oxide. But the germanium junction *is* remarkable because: (*a*) people can understand why it rectifies; and (*b*) it is an almost perfect rectifier, passing a very high current in one direction and almost none in the reverse direction. This has been a wonderful thing for the theoretical physicists, because it worked out just as they had said it would; but it has been a wonderful thing in many other respects, too, for the phenomenon of semiconductors is adding a new dimension to industrial technology. For example, rectifiers made on this principle are very efficient for use in high-frequency electronics circuits, where they are known as germanium diodes. One such rectifier is shown in Figure 3. Or, they may be made for real industrial power purposes, where their high efficiency makes possible rectifiers of very small size capable of carrying hundreds of amperes of current; one is shown in this picture. The Bell Telephone Lab-

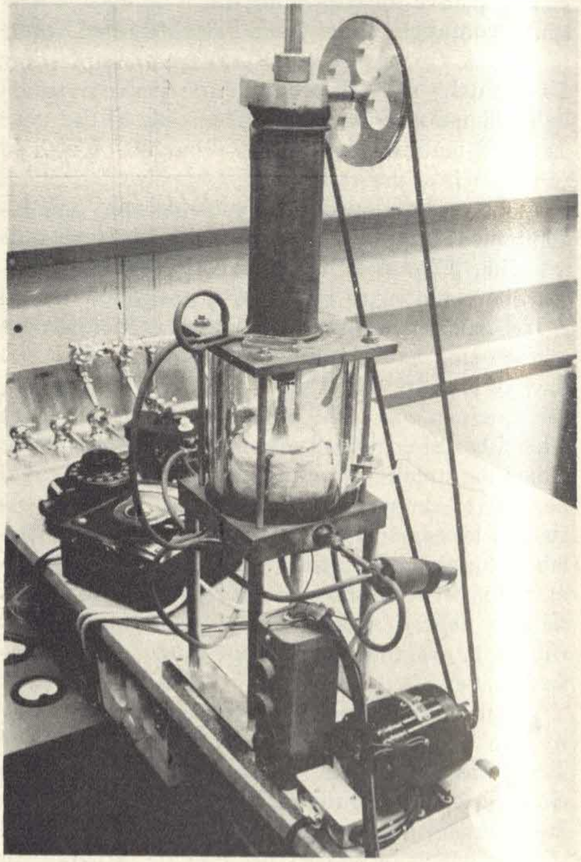


FIG. 1. Laboratory apparatus for producing high-purity germanium.

oratories discovered, a few years ago, how to put the equivalent of the control electrode of a vacuum tube into a semiconductor, thus making power amplification possible in a device called a transistor. Although these transistors were originally thought of as low-power devices suitable only for electronics circuits, we have made a transistor recently for high-power applications. Robert Hall, who accomplished this result, is shown with one of these units in Figure 4. In the span of a few years, there have come from this hitherto neglected field of semiconductors a flood of new technical capabilities which have already developed great importance, and which will in the future exert a profound influence upon electrical technology. The particular capabilities I have mentioned, embodied in practical devices for controlling electric power, are certain to have important implications in industry. But it would be a mistake not to recognize that the real significance in this development resides in the fundamental understanding that has been achieved in the basically important field of the solid state of matter.

In the hands of metallurgists, the study of the solid state is yielding new knowledge that is destined to have a correspondingly important bearing on mechanical technology. The strength of materials, especially of metals and alloys, is a primary limitation on the capabilities of the vast galaxy of machines that pace modern civilization. The speed of a motor, the power of an engine, the efficiency of a turbine, the endurance of an airplane, and the vital properties of a host of mechanical things all depend on, and are limited by, the properties of the metallic materials of construction. Without exception, these metals and alloys are crystalline solids, and the problem of improving and extending their properties reduces, in its most basic form, to the problem of understanding the mechanics of crystals. Until recently, attempts in this direction have been frustrating. Figure 5 shows the crystalline character of two ingots of aluminum. There is no doubt that a crystal is a regular three-dimensional array of atoms, and, from a knowledge of the atoms and their arrangement, it should be possible to deduce simple mechanical properties. The density of a crystal is, of course, correctly calculated from the known atomic weight of the constituent atoms and their arrangement, but that is about all that the theory of crystals does for the mechanical properties of solids. If one attempts to calculate the tensile strength of a crystal from this simple picture of atoms in regular array, the calculated result differs widely from the

FIG. 2. Robert Hall, specialist in semiconductors, and Saul Dushman, formerly associate director of the G-E Research Laboratory, examining an ingot of purified germanium.

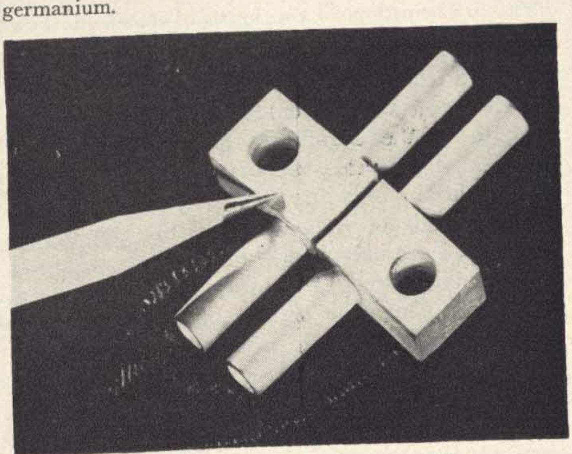


FIG. 3. Air-cooled germanium rectifier.

FIG. 4. Robert Hall and two germanium rectifiers designed and built by him.

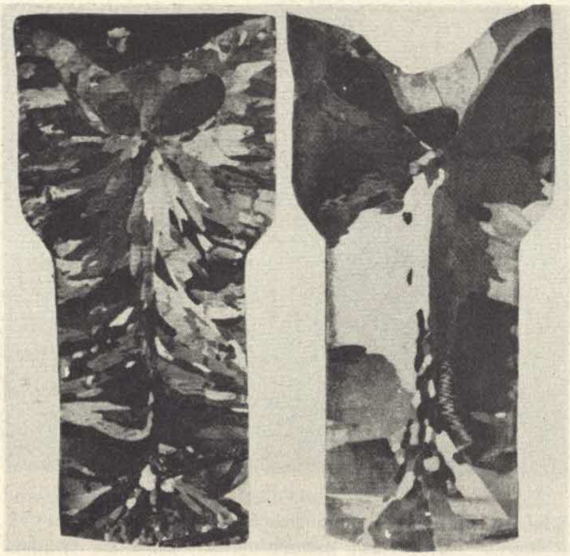


FIG. 5. Comparison of two ingots of aluminum, showing difference in crystal structure.

measured tensile strength. This is shown in Figure 6, where on the extreme right the theoretical strength of the aluminum crystal is shown as 600,000 psi. You may see that pure aluminum in practice attains only approximately one hundredth of this calculated value, whereas the best aluminum alloys are only about one tenth as strong as the calculated strength of a perfect crystal of pure aluminum. There are similar difficulties in interpreting other practical properties of crystals in terms of perfect crystal structure. Measured rates of diffusion, rates of solution, rates of crystallization, and rates of phase change, for example, all very important in metallurgy, defy interpretation on the classical basis of crystalline regularity. The key to these problems of great theoretical and practical importance lies in the defects of crystals. Everyone has known for a long time that crystals are generally not perfect, but it now appears questionable whether any example of a perfect crystal has ever been seen, at any time, by anyone! Hence it is not the geometric perfection of crystals, but their invariable imperfections, which determine the practical properties of crystalline solids such as metals and alloys. Concentrated study of the detailed nature of these defects is now revealing for the first time the fundamental nature of the properties of the materials upon which our mechanical civilization rests. A beautiful example of the role of the crystal defect is shown by the growth of crystals from a saturated solution. A perfect crystal would grow very slowly indeed, because the perfect plane crystal faces are atomically smooth, and hence resist the attachment of an atom from the solution. Practical crystals are, however, full of defects and structure discontinuities, in the neighborhood of which almost all the phenomena of crystal growth occur. These growth phenomena have been observed under the microscope, and have even been photographed in motion pictures. Such motion pictures illustrate in a graphic way the crystal growth associated with a stepped, or screw, dislocation. This particular dislocation appears to be a very important one in many practical cases. The geometry of the stepped-wedge dislocation is illustrated schematically in Figure 7. Consider a perfect cubic crystal in the shape of a perfect cube. Make a cut into one face parallel to the cube edge and halfway through the crystal. Then displace the layers of the crystal planes on one side of the cut, relative to the layers on the other side (Fig. 7). As compared to the atomically smooth faces of the crystal, the wedge dislocation is a highly preferred area for growth phenomena to occur. Once this was realized, scientists all over the world discovered spiral growth patterns arising from wedge dislocations in a great variety of crystalline materials. Crystals of silicon carbide that had been in the Mineralogy Department of the British Museum for many years were found recently to exhibit beautiful spiral growth patterns. The stepped dislocations that lead to this phenomenon presumably arise in nature from a variety of causes—impurities, strains resulting from temperature and concentration gradients, and the like. A dislocation of only one lattice parameter could not be seen in the microscope; presumably a single-layer stepped dislocation leads to the

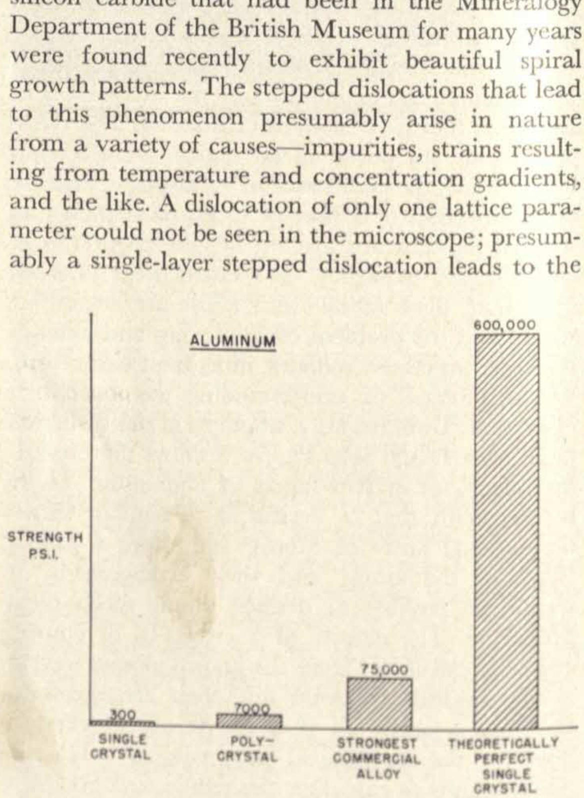


FIG. 6. Comparison of tensile strengths of crystalline materials compared with theoretical value.

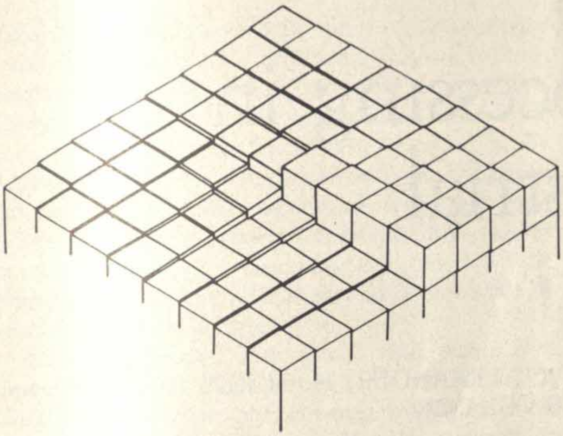


FIG. 7. Schematic illustration of stepped-wedge, or "screw," dislocation.

multiple layer steps that *are* readily visible. Some particularly beautiful growth patterns are seen on the surface of crystals of silicon carbide, as illustrated in the frontispiece. Different colors are seen through the microscope. These are presumed to be due to a surface oxidation film.

The importance of crystal dislocations is of course not confined to the growth of a crystal from solution. Far greater importance attaches to the generalization which logically proceeds from this particular observation. *The practical properties of crystalline solids reside in their lattice defects, not in their crystalline regularity.* This important fundamental observation is a vital key to future progress in the mechanical properties of solids. Both science and engineering will share in the capability, the responsibility, and the satisfaction of translating such knowledge into terms of human welfare.

ATOMIC SCIENTIST

He looked at us with a half-smile,
And listened calmly as we fussed.
He was the gentlest there, the while
He took the questions that we thrust.

"Now that you've given us the bomb,"
A student said, "what move is next?"
The easy smile curled slowly from
His lips . . . His words were not perplexed:

"Why, nothing. No, nothing at all.
All we can do is sit and wait."
Our disappointment was a wall—
But what did we anticipate?

"Live with your work," he said. "Just live,
And let the future slowly come.
We work and dream, we take and give,
And listen to the news at home."

We left him with his patient air
As one aware of what he said.
We went outdoors to mutely stare
At all the future overhead.

DANIEL SMYTHE

Schenectady, New York

Information Processing in Social and Industrial Research*

JAMES L. McPHERSON, SAMUEL N. ALEXANDER, H. BURKE HORTON, and EZRA GLASER

James L. McPherson served as tabulation expert for the 1940 Census of Population and Housing. Since 1943 he has been machine development officer of the Bureau of the Census. Samuel N. Alexander is chief of the Electronic Computer Section of the National Bureau of Standards. Dr. Alexander built the Standards Eastern Automatic Computer, and it is operated at NBS under his supervision. H. Burke Horton was a statistician with the Bureau of Transport Economics and Statistics of the Interstate Commerce Commission during 1945–50. Since October 1950 he has been chief of the U. S. Air Force Interindustry Research Office. Ezra Glaser served in several federal government agencies as an economic statistician and since 1945 has been with the Office of Statistical Standards of the Bureau of the Budget.

I. Electronic Computing Equipment as a New Industrial Tool

LARGE-SCALE electronic machines are capable of processing data for many different applications at very high speeds. These devices are the most recent effort of man to provide a mechanism able to handle arithmetic and logical operations much faster than the human brain, the accountant's pencil, or the desk adding machine. Automatic data computers are not new; what is new and potentially revolutionary is the technology that makes reliable, high-speed, large-capacity data processors operationally effective.

Eight or more general-purpose machines are in operation today, and others will be operating in the near future. The basic components of SEAC (Standards Eastern Automatic Computer) and other large-scale digital computers are the memory, the control unit, the arithmetic unit, and facilities for communicating with the outside world—the input and output units. The most powerful performance characteristics of these devices are their ability to modify the detailed instructions which control the solution of a problem and their selective sequencing ability.

* Based on papers presented at the session of the National Academy of Economics and Political Science at the 119th Annual Meeting of the AAAS, St. Louis, Missouri, December 26–31, 1952.

Table 1 illustrates how such devices are controlled. The 15 digits at memory position 101 are understood to say, "Add the number in memory position 200 to that in position 300, put the sum in 400, and look in 102 for your next instruction." At position 102 is an instruction through which a transfer of control is accomplished. It says, in effect, "If the contents of memory position 101 are the same as the contents of 160, go to 103 for the next instruction, but if the contents of 101 and 160 are not the same go to 106 for the next instruction." When control goes to 106, Operand A is the contents of 101 to which the contents of 180 are to be added. The sum is then placed in 101, which will now read, "Add 201, 301, 401, 102." The machine will proceed through the cycle, 101 to 102 to 106 to 101 to 102 to 106 . . . , etc., until 101 finally reads, "Add 299, 399, 499, 102," which is the same as the constant in memory position 160. At that point control will go from 102 to 103, where it will find instructions for other parts of the problem.

Table 1 illustrates how information stored in the memory can act as an instruction when it is transmitted to the control unit and can be treated as a number when transmitted to the arithmetic unit. This ability, combined with the ability to choose which sequence of instructions to execute, which was typified by the *Equ* instruction, makes possible routines of instructions to govern long and involved

data-handling processes by a series of simple steps.

Since electronic data-processing devices execute instructions at rates of 1000 or more per second, it is obvious that they will be important new tools for our economy. But the significant question is, By whom and how soon will they be introduced? Based on practical working experience with such machines, it may be expected that extensive use of electronic digital data-processing equipment will be made by commerce and industry in a time scale of years rather than decades.

A clerk equipped with pencil and paper is a "general-purpose data-processing" facility. The clerk is much more versatile and intelligent than the SEAC, but the SEAC is infinitely faster than the clerk at doing the relatively few things the SEAC can do. The SEAC can carry out arithmetic processes, it can compare two numbers, and it can choose between next instructions wherever the logical rules can be reduced to simple yes-or-no propositions.

One important characteristic the clerk and the SEAC have in common: until instructed in what to do neither is useful in any specific data-processing situation. To use an electronic digital data-processing device, it is necessary to know what operations the device is capable of performing. When directing the activity of clerks one must have an awareness of the clerks' abilities. Communication with them in a language they understand is essential. A much more fundamental requirement, however, is an understanding of what is to be accomplished in the specific sense. There are comparatively few people today who understand both the real end objectives of many data-processing activities and the mechanics employed to achieve those ends. It is precisely that kind of knowledge which is essential to economically successful use of these new devices. The electronic scientists and engineers have provided the devices. Now those with the jobs these devices can handle must contribute their detailed knowledge of specific problems in order to make use of the new tools.

It is, perhaps, somewhat unfortunate that in the popular press and elsewhere these information processors have been called giant brains. This term tends to make them appear forbidding and difficult to master. They are complicated, ingeniously designed tools intended to free the brains of men by taking over certain routine and repetitive operations. And, like any other tool, they can be useful when properly managed, as aids to achieve prestated objectives. It is remarkable to be able to communicate with an inanimate machine in much the same way that one communicates with a clerk; it is not particularly difficult once one knows the language the information processor understands. The skill with which the device solves a problem is a direct reflection of the intelligence that has been transferred into its operation by the human beings who plan its use. That this can be done seems achievement enough, and it is not necessary to claim the attributes of a brain for such devices.

Distinctions between the "whats" and the "hows" of data-processing activities can be illustrated by census undertakings. The "what" in any given investigation can be described by displaying the schedule used to collect information and the material published presenting that information in organized tabular form. The "how" for such an in-

TABLE 1

Illustrative Sequence of Computer Instructions

	Memory Location	Operation (3 digits)	Operand A (3 digits)	Operand B (3 digits)	Result (3 digits)	Next Instruction (3 digits)
	101	Add	200	300	400	102
	102	Equ*	101	160	103	106
Instructions	.					
	.					
	106	Add	101	180	101	101
	.					
	.					
	160	Add	299	399	499	102
Constants	.					
	180	000	001	001	001	000

* Equ = "test for equality." If Operand A is equal to Operand B, get next instruction from memory position shown under "Result;" if not, get next instruction from memory position shown under "Next instruction."

vestigation involves lengthy description. All the instructions to enumerators, respondents, editing clerks, coding clerks, and the other types of employees used in conducting a census investigation are part of the "how" of doing the task in question. The "how" is the complex part of many undertakings. Frequently, the beginning and the end objectives are easy to describe. But interested persons responsible for controlling inventories have discussed the potential use of electronic data-processing equipment. These persons know both the "what" and the "how" of the inventory control problem. They realize that the new equipment might change the "how" very materially, but that it would not alter the end objective, which has been and still is: "Have the right thing at the right place at the right time."

Unfortunately, some data-processing activities have become so compartmentalized that experts in intermediate processes often are really not familiar with either the originating transaction or the ultimate objectives. The employee in charge of the files in any large activity is likely to be an individual with specialized knowledge of how to classify and pigeonhole a variety of documents so they may be found at some unpredictable future time, but with no knowledge of how the papers handled originate or how the product to which they relate functions.

Such an employee might find it quite difficult to accept a system that did not require any removal of documents from the files. Yet such a system is perfectly feasible in the utilization of some of the techniques of the electrical communications field, which provide the means for transmitting and storing information. And electronic computer developments provide means for processing information. Together, a combination is made that has a great future potential. Consider a new system of filing that would employ magnetic recording on automatic playback devices. Telegraphic communication could be established to enable the users to "dial the file" for information, in much the same way that the telephone is now used to find out the time or the weather. Some of the advantages of such automatic files might be: (1) less space required for filing documents; (2) automatic reference to the file; (3) reference to the file without removing the original data; (4) records kept in bombproof underground vaults could be used and kept current; (5) magnetic records could be used which would be more durable than paper or microfilm and more resistant to both water and fire damage; and (6) files arranged so that they would not yield information until a key code word had been supplied, or all the confidential information stored in a scrambled form and an information processor used to decode the information for people authorized to interrogate the files.

The foregoing concerning inventory control and automatic files indicates two areas of potential applicability of these devices. They are by no means the only present possibilities. Any activity that involves a significant amount of clerical work is one on which information processors may have an important impact. Broad knowledge, wide vision, and bold imagination will be needed to harness the new devices to the tremendous loads of paper shuffled every day. There is in sight a clerical revolution that could match the Industrial Revolution. The Industrial Revolution consisted in large part of finding new, standardized, and automatized ways of handling material things. The revolution of today and of tomorrow encompasses the automatization of methods to handle information, and this in turn should generate still better or faster ways to handle things—whether in the automatic factory, or the automatic file, or the control of plane traffic in and out of a busy airport. The tools are ready. Now the persons who have the jobs to be done must contribute their part.

II. Some Uses of Interindustry Statistical Techniques

Interindustry analytical procedures show considerable promise as a means of attacking many types of economic problems. In the Department of Defense improved methods for testing the feasibility of its programs have grown steadily—during World War II and subsequent years. The old-style adamant insistence on military "requirements," which must be met at all costs, has given way at all echelons to a healthy respect for the limitations imposed by the nation's industrial and natural resources. This increased emphasis upon consideration of the nation's capabilities has, in large measure, evolved from the too-frequent adjustment and hasty alteration of plans that occurred during the period 1942–45.

To a great extent past difficulties have been due to inadequate means of measuring and taking account of the goods and services needed *indirectly* in the course of carrying out a war plan. The greatest single advantage of the interindustry technique is that it provides a systematic framework for taking account of all requirements, direct and indirect. A large part of previous mobilization difficulties resulted from attempts to carry out an initially impractical program. In effect, the nation's productive apparatus was used as a gigantic analog

computer to test, ex post facto, the feasibility of a whole series of programs. The answers took the form of insuperable production difficulties, time delays, and constantly changing delivery schedules, rather than "red lights on a control panel." Such a "computation" by trial and error could easily cost the nation more time than it can spare in the event of a new emergency. The present objective is to remove at least a part of this trial-and-error process from the nation's industrial establishments and to place it instead in the computation laboratories.

The broad outline of a procedure for representing the industries of the nation by means of systems of equations has been completed. Modern electronic computers, specifically the SEAC and the UNIVAC,† have demonstrated that they can handle the simplified mathematical systems that can be based upon data now available. These structures are very coarse approximations to the nation's intricate productive systems. Their validity depends upon the persistence of past production techniques and the ability to foresee and incorporate the more important departures from past interindustry relationships.

Computational structures must be sufficiently reliable so that grossly unworkable plans can be detected and modified prior to initiation. The production processes will then run more smoothly, greater reliance can be placed upon decentralized operational control by private management, and the governmental control apparatus can be streamlined and held down to the very minimum necessary under mobilization conditions. The procedures now being perfected are advance forecasting tools —not operational control devices.

The mathematical structures, or "models," now being used, are the result of a marriage of linear programming techniques, as applied to internal Air Force activities,[1] and the "input-output," or interindustry, techniques of economic analysis originally developed by Wassily Leontief, of Harvard University,[2] and developed further by staff members of the Bureau of Labor Statistics, U. S. Department of Labor.[3] The principal ingredient in the mathematical model is a set of functions which express in quantitative terms the relationship between the output of each industry and its requirements for the products of other industries on current account. The analytical procedures handle in separate and distinct stages current account purchases, needed to support production without increase in capacity, and capital account purchases required to increase capacity.

The Division of Interindustry Economics, Bureau of Labor Statistics, U. S. Department of Labor, was given the task of assembling the first postwar interindustry transactions table. This table was based upon the 1947 Census of Manufactures, the 1948 Census of Business, and a variety of supplementary sources. The basic study was carried out in 450-industry detail, with supporting product analyses for many industries. The Standard Industrial Classification, by which each establishment is assigned to an industrial class according to its principal product, was adopted as the basic classification. The heterogeneity of the output of many industrial sectors and the lack of acceptable physical measures of output in such instances led to the adoption of dollar sales (or purchases) as the principal unit of output (or consumption).

For computation purposes the 450-industry table was condensed to 190 industrial sectors. Table 2, an extract from this condensed table, indicates, for example, that the Motor Vehicle industry purchased \$36.4 million of Electric Power in 1947. The Electric Power industry in turn purchased \$7.5 million of Motor Vehicle products in 1947; this was in addition to capital outlays for complete new vehicles.

The limited amount of information now available for any one cell of this table precludes the use of elaborate functions relating consumption to output. If it is assumed that inputs, or purchases for current account, are proportional to output, a set of coefficients can be obtained (Table 3). This table shows, for example, that if sales and purchases (measured at 1947 price levels) vary proportionately, \$76,986 of Electric Power is required to produce \$1,000,000 of Primary Aluminum.‡ The network of industrial interrelationships is so complex that the output required of each industry for any given final demand can be determined only by summing an infinite set of values. It can be shown, however, that the sum is finite.

If A denotes the matrix of coefficients from which

† UNIVersal Automatic Computer, produced by the Eckert-Mauchly Division of the Remington Rand Corporation. UNIVAC No. 2 was accepted by the U. S. Air Force on June 25, 1952, and operates in the Pentagon 24 hours per day, 7 days per week. The Mathematical Computation Branch of the Planning Research Division is responsible for its operation.

‡ Linear nonproportional relationships can be handled with ease by present computing equipment. Such linear relationships can frequently be used over a wide range of values to give a satisfactory approximation to a more complicated function. Insufficient data are available in most cases, however, to serve as a basis for altering the assumed proportional relationships.

TABLE 2

EXTRACT FROM TABLE 1,* INTERINDUSTRY FLOW OF GOODS AND SERVICES BY INDUSTRY OF ORIGIN AND DESTINATION†

Continental United States, 1947

(Millions of Dollars)

Sector No.	Industrial Sector	4	16	49	58	88	127	145	148	167	169	Final Demand‡	Gross Domestic Output
4	Food Grains and Feed Crops	816.7	0	25.3	0	0	0	0	0	0	0	61.8	11,004.2
16	Coal Mining	0	14.6	17.2	0.2	0.3	0.4	14.9	0.9	384.6	408.1	444.0	3,036.4
49	Industrial Organic Chemicals	4.6	0	264.2	2.7	0	0	9.0	0	1.3	0.1	210.6	1,672.3
58	Fertilizers	245.7	0	1.0	60.6	0	0	0	0	§	0	18.4	522.8
88	Primary Aluminum	0	0	0	0	0	0	0	0	0	0	– 16.3‖	284.6
127	Ball and Roller Bearings	0	0	0	0	0	16.8	104.8	5.9		1.5	44.4	387.0
145	Motor Vehicles	58.3	15.2	0	0	0	0	3,294.0	22.2	7.5	0.2	7,385.4	12,519.7
148	Aircraft	0	0	0	0	0	0	16.7	143.0	0	0	1,363.8	1,604.9
167	Electric Light and Power	5.5	55.6	18.7	3.3	21.9	3.6	36.4	10.2	395.5	28.3	294.7	4,436.5
169	Railroads	117.1	6.0	40.9	46.7	3.9	3.7	237.2	11.6	127.6	411.5	4,689.2	9,959.0

* Source: U. S. Dept. Labor, Bur. Labor Statistics, Div. Interindustry Economics (Oct. 1952).

† Each row shows distribution of output of producing industry named at left. Each column shows input distribution for purchasing industry named at top.

‡ Final Demand is the consolidation of Households, Inventory Change, Gross Private Capital Formation, Government, Foreign Trade, Construction, Stockpile, Waste Products, Small Arms, and Small Arms Ammunition.

§ Less than $50 thousand.

‖ Very little Primary Aluminum enters into Final Demand. Most of the output is used as an intermediate product. A decrease in inventory in 1947 and some imports (negative Final Demand) account for the minus sign.

Table 3 is extracted, the "inverse complement of A" [or, symbolically, $(I\text{-}A)^{-1}$] is a matrix of factors which makes possible the direct calculation of such sums. Table 4 is an extract from the 190-order inverse complement of A.§ Granting the basic assumptions, Table 4 points out that orders of consumers for $1,000,000 of Motor Vehicles generates the following outputs by other industries: Railroads, $52,376; Electric Light and Power, $15,867; and Coal Mining, $15,515, as indicated in Column 145. The row for each industry contains a set of factors that measure the given industry's stake in the consumption of products of all other industries For example, the Ball and Roller Bearing industry has a stake of $14,100 in an order for $1,000,000 of Motor Vehicles. The production of $1.0 million of motor vehicles for final consumers regenerates *indirect* effects of $1.7 million as measured in terms of plant sales for all industries. Thus, a total of $2.7 million of transactions is required to fill an order for $1.0 million of motor vehicles. This type of table has implications for market analysis, although it was not developed for this purpose.

§ The inversion of this 190-order matrix was one of the largest integrated mathematical operations ever successfully completed. It involved about 7,000,000 accurate multiplications of 9-digit numbers. This task was done in less than 50 hours. Air Force and Eckert-Mauchly personnel worked jointly to complete the operation, using the UNIVAC No. 1 of the Bureau of the Census in Philadelphia. Several such large-scale inversions have since been performed on the Air Force UNIVAC in the Pentagon.

Timing is of the essence in mobilization forecasting. Table 5 is extracted from a 190-order table of average timing factors prepared from data furnished by the Office of Business Economics, U. S. Department of Commerce. Each factor measures in calendar quarters the average time interval by which the output of goods and services from the industry at the left must precede the output of the industries listed across the top. These factors were estimated primarily from inventory data (reciprocal of turnover rates), with certain additional adjustments. Row 127, Column 145, Table 5, indicates that ball and roller bearings should be delivered to plants of the Motor Vehicle industry an average of 0.9 quarter prior to delivery of a completed automobile or truck. For computation purposes integral lead times are desirable. The associated input coefficient is split, therefore, 9:1. The first portion is assigned an input lead time of 1.0 quarter, and the second portion receives a lead time of 0.0 quarter, thereby preserving the average lead time of 0.9 quarter.

The use of integral lead times changes a time-phased model to the same form as a very large static model. If there are 190 industries and 16 time periods (4 years, by quarters) plus a 17th

TABLE 3

EXTRACT FROM TABLE 2,* DIRECT PURCHASES PER MILLION DOLLARS OF OUTPUT†

Continental United States, 1947

Sector No.	Industrial Sector	4	16	49	58	88	127	145	148	167	169
4	Food Grains and Feed Crops	74,216	0	15,143	0	0	0	0	0	0	0
16	Coal Mining	0	4,821	10,268	430	994	977	1,190	581	86,684	40,978
49	Industrial Organic Chemicals	421	0	158,725	5,116	0	0	715	0	284	5
58	Fertilizers	22,329	0	608	116,187	0	0	0	0	6	0
88	Primary Aluminum	0	0	0	0	0	0	0	0	0	0
127	Ball and Roller Bearings	0	0	0	0	0	54,896	8,371	3,707	0	150
145	Motor Vehicles	5,300	4,997	0	0	0	0	268,605	13,848	1,682	24
148	Aircraft	0	0	0	0	0	0	1,331	91,473	0	0
167	Electric Light and Power	499	18,319	11,163	6,298	76,986	9,284	2,906	6,329	89,140	2,845
169	Railroads	10,638	1,961	24,449	89,348	13,728	9,473	18,946	7,199	28,758	41,321

* Source: U. S. Dept. Labor, Bur. Labor Statistics, Div. Interindustry Economics (Oct. 1952).
† Each entry shows direct purchases from industry named at left by industry at top per million dollars of output of the latter.

period for "level-off,"‖ then there are 17×190, or 3230 variables, to be computed. This can be handled in the same manner as a 3230×3230 static problem. By taking advantage of certain special characteristics of the system, however, it is possible to reduce this unmanageable method to seventeen 190×190 systems solved in sequence.¶

The results of the first large computation, called the "Emergency Model," are being appraised. This model is based upon 1947 coefficients brought up to date as of about 1951 in important areas. It was found necessary to supplement the listed industries by special analyses of important military end items not in production in 1947. The design of sampling procedures to determine the industrial impact of the host of civilian-type items procured by the three services is a complex task. Before this structure and the computed military requirements could be used, estimates of nonmilitary final demand were found to be needed to round out the complete picture of requirements. These estimates were prepared by governmental agencies outside the Department of Defense.

Analytical tools of the type described above have been developed as part of the Air Force research program in interindustry economics. This program was undertaken at the request of the Department of Defense. Army and Navy personnel are assisting in translating military requirements into the precise form necessary for these mathematical structures. In addition to the projects mentioned above, techniques are being developed for the purpose of translating production schedules into manpower requirements, industry by industry. Other projects, which are well under way, will provide a means for quickly determining the effect of adding new capacity in industries found to be deficient in the course of a computation. The Bureau of the Budget has accepted the task of monitoring the research projects carried on by other governmental agencies and by universities.

If, as a result of this research, any major differences between military requirements and industrial capabilities are brought to light, the program will have been worth while. Corrective action may be taken on both sides of such discrepancies. If urgent military necessities do exceed present or prospective capabilities, it is highly important that the facts be known years in advance, while there is time to take corrective action. If the indicated corrective action itself exceeds the means, then there is a need to review the military necessities. Early review will guard against the haphazard, unbalanced readjustments that will surely occur if an impracticable program is attempted and cutbacks are made in haste. It appears that a substantial portion of these major difficulties can be brought to light by present interindustry analytical procedures. It is highly desirable that this process of review and adjustment take place in the computation laboratory in advance of an emergency, rather than in the nation's factories when it is too late for expansion of indus-

‖ This permits the assumption of continuing production at constant levels for all industries in the postmodel years.

¶ The procedure, suggested by George Suzuki, of the Air Force Interindustry Research Office, takes advantage of the fact that inputs precede outputs. Consequently, if the last time period is solved first, indirect requirements which must be produced in earlier time periods are fixed. The computation proceeds backwards in time, yielding a set of solutions for each time period. This problem was handled by an iterative procedure on the SEAC.

trial capacities or for changes in a balanced program.

III. Formal Programming and Information Processing in Social Science, Accounting, and Management

Questions concerning formal programming and information processing do not fall within any traditional field of study. Rather, they occur near the boundaries or in the overlapping regions of statistics, other and younger branches of applied mathematics, social sciences (especially economics), technology, and management. Certain logical and practical advantages are derived from the circumstance that the analytical framework of interindustry studies constitutes a closed system of information in the sense that some well-defined domain is covered without duplication. This kind of analysis need not be restricted exclusively to economics. It has possible applications elsewhere, in areas where some of the advantages of the approach might carry over to new problems.

In searching for applications of especially constructed routines of information processing in whatever branch of applied science, their course drifts toward the same areas as the two general lines of inquiry in the above separate discussions. Collectively, a useful unit is formed. Without the very high-speed equipment, with its related coding and processing techniques, the formal economic analysis could have realized only a small part of its potential usefulness. Similarly, the equipment is a lost and confused servant without a carefully worked-out set of definitions, classifications, and systems of information processing.

It is useful to follow through the motions of how a general-purpose computer might be used in some specific business unit—for example, a billing office of a department store. For convenience, the operations or activities may be divided into three classes. Into the first go all routine activities. These are operations that require no judgment and that occur frequently. The second class of activities is composed of operations that require judgment. Either there are too many variables influencing the decisions, or their relation to the decision is complicated, or the relevant information is subtle by nature or difficult to find. The third and last class contains atypical items that do not require any particular judgment in the usual sense of needing the attention of a person of superior mental ability and experience. A clerk, for example, can do what is required. This case arises so infrequently, however, that it does not pay to establish a routine for mechanical or electronic handling. The high-speed computer can do the first task—a routine operation. It cannot do the second—that of judgment. It does not pay to have it do the third—the simple but atypical work. An operation classed as atypical might occur more frequently than had been guessed; thus it might be moved into the first class and a new routine established. Some judgment acts in the second class might be reduced to a formal program and placed in the first with a fairly complicated routine.

A case of applying a computer to a complete administrative system, perhaps a department store, can be considered. The interest will not be mainly in the computer, but rather in a rational approach to the processing of information and the programming of activities.

Consider first the domain of financial data—the information in the general accounts. Identify every point where a decision must be made. With each decision point is a set of alternate courses of action—one particular procedure for each decision that can be taken. For routine decisions the procedures will be completely described; for the most complex decisions the procedure might not be determined in advance. The decision points will then be classified into the three classes described above: routine, judgment, atypical. This will provide a tentative split of the operations of the administrative unit into two groups—those performed by machines and those performed by human beings.

The domain of information can be expanded to cover, for example, an inventory-control system. A new list of decision points is catalogued and classified into the three classes. Now, however, there are interactions between the domain added and the original one. New uses for the old data appear, sometimes in combination with new kinds of information. As the domains of information and decision making are extended to, perhaps, production scheduling and control, market analysis, product research, and development, several new factors will appear: (1) new uses for information will evolve; (2) more complex decisions will be listed—those requiring a greater volume of data, or more different kinds of information, or more complicated relationships between the information and the decision; and (3) broader scope of decisions will come under scrutiny—i.e., decisions affecting more parts of the organization or large ranges of products, or longer intervals of time—to the extent that the general type of decision made will be less like that of a file clerk and more like that of a supervisor, or even approaching the function of some minor executive.

In principle, the analysis of information uses

TABLE 4

EXTRACT FROM TABLE 3,* (TRANSPOSED) DIRECT AND INDIRECT REQUIREMENTS PER MILLION DOLLARS OF FINAL DEMAND†

Continental United States, 1947
(Unit: $1000)

Sector No.	Industrial Sector	4	16	49	58	88	127	145	148	167	169
4	Food Grains and Feed Crops	1,086.5	.5	27.3	5.0	1.2	.5	2.7	.8	.3	1.6
16	Coal Mining	7.2	1,009.0	36.2	39.2	25.2	11.8	15.5	7.5	99.0	45.4
49	Industrial Organic Chemicals	3.5	1.9	1,212.3	32.2	22.9	1.8	12.4	3.6	1.4	1.5
58	Fertilizers	28.0	.1	3.5	1,135.7	4.0	.1	.4	.2	‡	.1
88	Primary Aluminum	.1	.6	.7	.7	1,026.1	1.7	4.7	11.4	.1	.7
127	Ball and Roller Bearings	.5	.6	.3	.3	2.2	1,058.9	14.1	5.6	.2	.4
145	Motor Vehicles	13.7	9.0	3.1	3.3	4.2	1.4	1,376.2	23.9	4.3	2.3
148	Aircraft	.1	.1	.1	.1	.1	.1	2.3	1,100.9	‡	.2
167	Electric Light and Power	11.7	22.7	23.1	20.7	100.5	16.8	15.9	14.7	1,101.6	6.3
169	Railroads	21.3	7.7	54.3	136.0	55.0	25.9	52.4	21.1	36.0	1,047.3

* Source: U. S. Dept. Labor, Bur. Labor Statistics, Div. Interindustry Economics (Oct. 1952).
† Each entry shows, per million dollars of deliveries to final demand by industry named at top, total dollar purchases directly and indirectly required from industry named at left.
‡ Less than $50.

and decision making within an administrative unit will eventually produce a catalogue of all possible uses of each piece of information. The initial question, then, of how to use a "feeble-minded" but fast mechanical servant has led to a different question: How can a study of informational domains and informational flows be systematized in relation to decision making and the construction of efficient processing of "paper work"?

Why does a formal study or a formal "science" arise in a case such as this? What is behind the evolution of a new branch of applied mathematics or applied formal logic? When the analysis of a problem demonstrates the need for many distinctions, a classification system is constructed in which there are many classes. When there are too many classes, and their properties are too complex to remember and apply to a problem, and the relationships among them assume a great variety of logical patterns, then it becomes necessary to define formal rules and create formal structures for assembling, analyzing, and using information. This is one of the origins for a new kind of applied science: the theory of information.

At the one extreme decisions are made by "hunch" or an undefined "experience." A maximum of efficiency for any one brain is eventually reached, with only limited opportunity to pass along the knowledge to others and limited opportunity for division of labor in the judgment matters. At the other extreme—and this is of course an extreme position—there is almost no use for "hunch" or personal judgment. Each decision point is defined, and an array of all useful decisions is con-

TABLE 5*

INPUT LEAD TIMES FOR SELECTED INDUSTRIES†
(Unit: 1 quarter)

Sector No.	Industry	4	16	49	58	88	127	145	148	167	169
4	Food Grains and Feed Crops	4.0		0.7							
16	Coal Mining		0.7	0.7	0.7	0.5	1.5	0.9	1.3	1.0	0.5
49	Industrial Organic Chemicals	4.0		0.7	0.7			0.9	1.3	1.0	2.1
58	Fertilizers	4.0		0.7	0.7					1.0	
88	Primary Aluminum								1.3		
127	Ball and Roller Bearings						1.5	0.9	1.3		2.1
145	Motor Vehicles	4.0	0.7					0.9	1.3	1.0	2.1
148	Aircraft and Parts							0.9	1.3		
167	Electric Light and Power	1.9	0.3	0.2	0.2	0.1	0.6	0.3	0.6	0.1	0.1
169	Railroads	3.8	0.6	0.6	0.6	0.4	1.4	0.9	1.3	0.9	1.1

* Source: U. S. Dept. Commerce, Off. Business Economics, Business Structure Div.
† Each entry shows the average time by which the delivery of goods and services of the industry listed at the left must precede the delivery of completed products of the industry listed at the top.

structed. Functional relationships between the information and the decisions are discovered or postulated. The result is a set of logical processes by which many kinds of data can be brought to bear on a problem as though there were some one brain equal to the task of mastering the entire range of data, relationship, and decision.

The application of the principles of formal programming and formal information processing to social science, accounting, and management, then, encounters two difficulties: (1) many data and usually many different kinds of data are needed, and such must be in a particular form with respect to dating, accuracy, classification, units (including price level if the units are in money values), and, perhaps, other matters; and (2) the uses of the data are complex—a decision might profitably take into account more variables than one can bring to mind, and functional relationships in the mathematical sense might be too complicated to grasp without especially devised formal analytical equipment. Frequently, *both* the number of variables and the complexity of relationships dictate the need for a formal framework.

Part I has developed the accounting application of information processing equipment—often called computers. It has attempted to dispense with mental "ditch digging" in keeping accounts, mailing bills, totaling individual records, and filing information and referring to it later when needed for some business operation. The objective is to obtain accurate work at high speed and low cost. This involves simple decision making. As the number of variables increases and the relationships become complex, the direction is away from clerical operations toward management operations, away from the routine processing of information toward the complex.

Part II has discussed a specific application in the social sciences. By earlier standards, this economic analysis required a formidable volume of basic data from many sources. Even if the economic model is restricted to linear relationships, the system of relationships is complex in that every industry is simultaneously affected by the level of production in nearly every other industry. No individual can examine raw tables, study the relationships, and derive approximate results. But after the formal processing has proceeded far enough, then a trained person can derive very useful classes of answers. In this case, however, there is a dependency upon the formal analysis which has already been completed. It does not seem possible to keep account of either the data or the relationships without a formal programming system.

It now appears to be evident that the search for an electronic file clerk is neither unrelated to—nor inconsistent with—an attempt to make major judgments more systematically and with the fullest use of available information and discoverable relationships. In many situations these problems may prove to be complementary in the sense that neither undertaking could be done well by itself.

References

1. Wood, M. K., and Dantzig, G. B. *Econometrica,* **17,** 193 (1949); Dantzig, G. B. *Ibid.,* 200.
2. Leontief, W. *The Structure of the American Economy, 1919–1929.* Cambridge: Harvard Univ. Press (1941). Now available in revised form, *The Structure of the American Economy, 1919–1939.* New York: Oxford Univ. Press (1951).
3. Evans, W. D., and Hoffenberg, M. *Rev. Economics and Statistics,* **34,** 97 (1952).

SCIENCE ON THE MARCH

BIOTIC CHANGE IN NEW ZEALAND

MANY areas recently colonized and developed by Europeans have undergone such rapid and often revolutionary changes in their native vegetation and animal life, and in some of their soil and physiographic conditions, that thorough study should yield valuable information for ecologists, other biologists, and conservationists. Several regions in the temperate zones of the Southern Hemisphere, especially parts of Australia, South Africa, and New Zealand, have passed through some of the most rapid changes in modern times. Of these, New Zealand probably offers the most significant information, because its islands have formed a microcosm to which the changes have been confined. Also, the original conditions were in many ways unique; likewise, many of the changes have been accurately recorded and recently evaluated by geographers and biologists. After a year's visit to New Zealand and a review of the records of its settlement and the induced changes in natural conditions, I was impressed with the numerous ways in which American scientists might benefit from a study of this record, which is not too well known or appreciated in this country.

Four recent publications bear particularly on the subject of induced biotic changes in New Zealand: two by the geographers Cumberland[1] and Clark,[2] and two by the biologists Wodzicki[3] and Murphy.[4] Clark's account of changes in the South Island is a comprehensive study in historical geography, which traces the many facets of change and development, such as the human population, deforestation, crop development, grazing and pasturage, and the effects of the introduction of particular plants and animals. Cumberland shows how slip erosion on steep hills has greatly increased because of overgrazing, changes in the grasses and other vegetation, and frequent fires. Wodzicki discusses the often disastrous effects of the introduction of mammals and other animals, and some plants, which in many cases have become destructive pests. Murphy, in a short but strongly written article, also points out how the introduction of animals has often been a mistake, and he lays much blame for this upon the acclimatization societies, which in their zeal brought in animals and plants without any concern as to how they would fit into the total ecological pattern of the islands.

Clark's historical geography is much the most complete and best documented of these publications. He lists in his bibliography over 500 titles, which cover nearly every phase of change, and in the text he thoroughly considers, among other factors, cattle and sheep grazing, raising of wheat and potatoes, deforestation, and some of the notable cases of the effects of introduced plants and animals that have "gone wild" and overproduced in their new habitat.

The islands—especially the South Island—have such a variety of climates and physiographic conditions that many distinctly different areas occur in close proximity. There are great mountain ranges, such as the Southern Alps, and wide plains, such as the Canterbury Plain. The rainfall varies from about 200 inches a year to as little as 15 inches, within a distance of some fifty miles. Large forests once existed that varied from subtropical types to cold subantarctic types within short distances. Along the mountain ranges and their foothills a particular type of tussock grassland had developed that before settlement had not been grazed or often burned. There were no mammals among the native animals, which consisted mostly of birds. They existed in moderate numbers and in balanced populations, among which there were few predators. Only a few Maori had settled, and these Polynesian people had done little to disturb the intricate balance of nature.

These islands were invaded over a period of several decades, mainly from 1840 to 1900, by Europeans and a few Orientals, who brought with them some 53 different mammals, 125 birds, and numerous plants for the forests and hedges and pastures and crops. The human immigrants did the usual things that upset the balances in nature as they converted former forests to fields and fields to cities. There was intense activity in clearing forests for sheep pasturage, and fires spread far beyond their intended bounds. Following the fires, native plants, notably the manuka, or tea tree, spread and occupied the ground with scrub growth, which, although inferior economically, has served to stabilize the soil and initiate the ultimate return of the forest. Soils overgrazed by merino and

Tussock grasslands and Southern Alps mountains, with scattered, small forests of the southern beech trees, and scrub vegetation in the valley. All three types of vegetation have been greatly altered by man and introduced animals within the past twelve decades.

mixed-breed sheep soon became gullied and eroded on the steep slopes that are so common on both islands. All manner of consequent changes took place, and from the record much can be learned of the particular results of certain practices. But to many ecologists and other biologists the less direct man-induced changes are the most interesting.

The sheep, and to a lesser extent the cattle, were the first grazing animals ever present on the native grasslands, which were in effect as near the climax type as the soil and climate could support. These animals, by their differential choice of forage, their trampling, and other activities, changed the nature of these grasslands even in areas where they were not numerous enough to overgraze. Thus there is a record here of what happens to undisturbed grasslands suddenly exposed to the effects of grazing. New Zealanders have studied these effects, and their records can tell us how certain changes were produced. Such information should be valuable to a better understanding of the past history of the North American grasslands.

The sheep have done their damage to the grasslands, but currently animals are being introduced into the forests and scrub that are more slowly but just as surely changing them. These are mainly deer, goats, and wild pigs introduced from Europe, America, and Asia, and the opossum introduced from Australia. Because of its arboreal habits, the opossum is eating the forests from the top down, and the browsing and grazing hoofed animals are eating the forests from the bottom up. Numerous areas lack a natural growth of seedlings and young trees because of the activity of these animals, and in some areas where the opossum is abundant even the larger trees are being injured or killed in large numbers.

The introduced rabbit has been the worst pest of all in some districts, such as central Otago, and the costly attempts to control it illustrate another mistake made by the acclimatization societies. Certain Musteliadae, especially weasels, were brought in as predators to reduce the population of rodents, and Wodzicki noted evidence of their feeding on birds. One such bird is the very rare Notornis, now living only in a high valley of Fiordland; here the introduced predators must be trapped to near extinction if the small Notornis population is to be preserved.

These are but a few examples of the upset eco-

logical balances among the animals. The plants furnish similar examples. The European gorse, and to some extent the brooms, originally introduced as hedge plants, have spread onto the pastures and over other large areas, taking over valuable pasturage. The brambles of the genus *Rubus* have similarly spread and become an important pest. Indeed it is said that on the South Island west coast there is one blackberry bush, and it is a hundred miles long.

Nostalgia for the homeland, which for the majority of settlers was England or Scotland, prompted the introduction of the gorse, red deer, and certain birds. American introductions were similarly made, and several, such as the Monterey pine, have become valuable acquisitions. But the main difficulty with the zeal of the acclimatization societies, or any other groups making such introductions, has been their lack of ecological foresight. This group of islands, therefore, points up the thesis of many ecologists and conservations: that a knowledge of the multiple physical, climatic, and biotic relations is the proper basis for introductions, planning of change, and management. These multiple relationships are seldom easy to ascertain, but some estimate of them may be gained from studies of those changes of the past related to man's indiscriminate tampering with nature. New Zealand offers an excellent field for such studies.

JOHN H. DAVIS, JR.

Department of Botany
University of Florida

References

1. CUMBERLAND, K. B. *Soil Erosion in New Zealand: A Geographic Reconnaissance.* Wellington, N. Z.: Soil Conservation and Rivers Control Council (1944).
2. CLARK, A. H. *The Invasion of New Zealand by People, Plants and Animals; The South Island.* New Brunswick, N. J.: Rutgers Univ. Press (1949).
3. WODZICKI, K. A. *Introduced Mammals of New Zealand*, Bull. No. 98. Wellington, N. Z.: Dept. Scientific and Industrial Research (1950).
4. MURPHY, R. C. *New Zealand Geographer*, 8, 1 (1952).

New Books Received

Carbohydrate Metabolism. (Rev. ed.) Samuel Soskin and Rachmiel Levine. x + 346 pp. Illus. $8.50. University of Chicago Press, Chicago. 1952.

An Introduction to the Chemistry of the Hydrides. Dallas T. Hurd. x + 231 pp. $5.50. Wiley, New York; Chapman & Hall, London. 1952.

Storage Tubes and their Basic Principles. M. Knoll and B. Kazan. xiii + 143 pp. $3.00. Wiley, New York; Chapman & Hall, London. 1952.

Maleic Anhydride Derivatives. Reactions of the Double Bond. Lawrence H. Flett and William Howlett Gardner. x + 269 pp. Illus. $6.50. Wiley, New York, 1952.

Wood Chemistry, Vol. 1. (2nd ed.) Louis E. Wise and Edwin C. Jahn, Eds. vii + 688 + xxxii pp. Illus. $15.00. Reinhold, New York. 1952.

Great Barrier Reef Expedition, 1928–29. Scientific Reports, Vol. BI, No. 3., *Madreporaria, Hydrocorallinae, Heliopora and Tubipora.* Cyril Crossland. 257 pp. + 56 plates. £3 10*s*. British Museum (Natural History), London. 1952.

Ink and Paper in the Printing Process. Andries Voet. xii + 213 pp. Illus. $5.90. Interscience, New York. 1952.

Personality in the Making. The Fact-Finding Report of the Midcentury White House Conference on Children and Youth. Helen Leland Witmer and Ruth Kotinsky, Eds. xviii + 454 pp. $4.50. Harper, New York. 1952.

Psychiatry and the Law. Manfred S. Guttmacher and Henry Weihofen. viii + 476 pp. $7.50. Norton, New York. 1952.

Great Adventures in Medicine. Samuel Rapport and Helen Wright, Eds. xxi + 874 pp. $5.00. Dial Press, New York. 1952.

Pasture Production and Management. R. H. Lush. x + 193 pp. Illus. $3.75. Blakiston, New York. 1952.

Limbo. Bernard Wolfe. 438 pp. $3.50. Random House, New York. 1952.

Rheumatic Fever. A Symposium held at the University of Minnesota on Nov. 29, 30, and Dec. 1., 1951, under the sponsorship of the Minnesota Heart Association. Lewis Thomas, Ed. 349 pp. $10.00. University of Minnesota Press, Minneapolis. 1952.

A Physiological Approach to the Lower Animals. J. A. Ramsay. ix + 148 pp. Illus. $2.75. University Press, Cambridge, England. 1952.

Soviet Civilization. Corliss Lamont. xviii + 433 pp. $5.00. Philosophical Library, New York. 1952.

Elementary Analysis. Kenneth O. May. xviii + 633 pp. $5.00. Wiley, New York; Chapman & Hall, London. 1952.

Human Milk. Yield, Proximate Principles and Inorganic Constituents. Technical Communication No. 18 of the Commonwealth Bureau of Animal Nutrition, Rowett Research Institute, Bucksburn, Aberdeenshire, Scotland. S. D. Morrison. viii + 91 pp. Illus. 10*s*. 6*d*. or $1.50. Commonwealth Agricultural Bureaux, Farnham Royal, Slough, Bucks. 1952.

Biological Chemistry. An Introduction to Biochemistry. Alexander Gero. xvi + 340 pp. (No price given.) Blakiston, New York. 1952.

The Indian Tribes of North America. Smithsonian Institution Bureau of American Ethnology Bull. 145. John R. Swanton. vi + 726 pp. + 5 maps. $3.50. GPO, Washington, D. C. 1952.

Introduction to Theoretical Physics. (3rd ed.) Leigh Page. xi + 701 pp. $8.50. Van Nostrand, New York. 1952.

Life and Letters of R. A. F. Penrose, Jr. Helen R. Fairbanks and Charles P. Berkey. x + 765 pp. Illus. Geological Society of America, New York. 1952.

Manual of Photogrammetry. (2nd ed.) xiv + 876 pp. Illus. $9.00, members; $12.50, nonmembers. American Society of Photogrammetry, Washington, D. C. 1952.

BOOK REVIEWS

THEORETICAL PHYSICS

The Theory of Relativity. C. Møller. xii + 388 pp. Illus. $7.00. Oxford University Press, New York, 1952.

ACCORDING to Professor Møller, this book is the result of twenty years of teaching relativity at the University of Copenhagen. It is distinguished by careful attention to the didactic problems presented by the theory and by special emphasis on its "physical"—i.e., intuitive—aspects. At the same time, the presentation is unusually complete and is suitable as a basic text for a year's course which starts at a fairly elementary level but which will, by the year's end, have taken in the major aspects of both the special and the general theory. In writing this book, Møller has had the advice of Niels Bohr, and I believe the book is written in the manner in which Bohr likes to have the theory presented. Acknowledgments have also been made to Professors Mott and Sneddon, themselves the authors of famous textbooks.

After its triumphant entry into the arena of physics in the first two decades of the century, the theory of relativity has had a rather curious history. The special theory of relativity has become part of the daily bread-and-butter of the physicist, inasmuch as even the cheapest little electrostatic generator will accelerate electrons, so that their behavior must be described relativistically. Even nuclear particles, with their much greater masses, will exhibit some "relativistic effects" at energies of about 20 mev (ordinary cyclotrons) and will behave completely relativistically at the energies of cosmic radiation, or even at those that are achieved in the Brookhaven cosmotron. At these energies, the effective mass of the particles consists for the better part of the mass carried by their energy of motion, and their rest mass, which alone is known to ordinary mechanics, becomes less and less important. In all other respects, the behavior of these fast particles bears little resemblance to that of slow-moving bodies. When they are accelerated by some forward force, they no longer respond by increasing their speed (which cannot exceed that of light) but by increasing their mass. If such "extreme relativistic" particles are charged, they will produce both electric and magnetic fields, which are in a nearly fixed relationship to each other. All these effects are predicted in detail by the special theory of relativity, and their presentation is included in Møller's book.

The general theory of relativity, undoubtedly one of the most brilliant contributions to our understanding of the physical universe by a single scientist, has led a Cinderella life. Since its original formulation in 1916, it has been drawn on but rarely to contribute to neighboring disciplines and it has remained a field of endeavor for relatively few groups of theoretical physicists. Its positive accomplishments to date are, briefly, the explanation of the equal acceleration of all bodies in a field of gravitation (the so-called principle of equivalence); the explanation of a minute irregularity in the orbit of the planet Mercury; the prediction of the bending of light rays near the limb of the sun, observable during total eclipses and verified beyond doubt only this past year; and the prediction of a red shift of spectral lines originating close to very dense masses, such as are found in white dwarfs. In addition, the general theory of relativity has given rise to speculations in the realm of cosmology and cosmogony. These speculations rest both on theoretical possibilities unearthed by relativity and on observations on the red shift of distant nebulae. These latter observations are just being re-evaluated, and recent estimates concerning the "age of the universe" will probably have to be doubled. That such a change is at all necessary would seem to indicate that present techniques of observation and evaluation, in spite of the availability of the giant telescopes on the West Coast, are still on the ragged edge when it comes to extremely distant objects, and that the whole field of cosmology will remain speculative, although extremely fascinating, for another few decades.

In this reviewer's opinion, this period of relative obscurity is about to come to an end. Quantum field theory has now reached a stage where the emergence of new points of view alone will lead to real progress. A considerable step forward was made immediately after the second world war when Schwinger in this country and Tomanaga in Japan, and with them several other investigators, introduced consistently (special) relativistic procedures into the quantum theory of the electromagnetic field. Further progress on a fundamental level will very likely be brought about by the introduction of general-relativistic approaches into quantum field theory. This opinion is based on the fact that general relativity gives us a deeper understanding of the nature of fields and their relationships to particles than has been achieved anywhere else in theoretical physics. This understanding will be preserved by any theory that will maintain the principle of equivalence (similarity of gravitation with inertial effects, such as centrifugal "forces"), even though it may deviate in its specific details from the general theory of relativity as it was originally conceived by Einstein.

Professor Møller, whose own principal researches have been in other fields, feels sufficiently strongly about the significance of the theory of relativity that he has undertaken to write a text of the present scope. His book devotes some 200 pages to the special theory, and another 150 pages to the general theory. Beginning

with a brief historical sketch of the antecedents, he moves through the kinematic aspects of the special theory, mechanics, Minkowski universe, and electrodynamics, to complete the foundations of the special theory (five chapters). He then goes into somewhat more specialized aspects, the mechanics of continuous matter, electrodynamics in the presence of dielectric and paramagnetic substances (without nonlinear effects), and thermodynamics. In this last chapter (VII), this reviewer was slightly unhappy about the absence even of a hint that the elementary developments given are apt to be quite insufficient to deal with real situations. This is so because, for instance, the quoted early results by Planck and Einstein on relativistic thermodynamics cover only a very limited and not even very interesting class of situations, those in which one equilibrium situation develops into another equilibrium situation. Even the very moderate extension of thermodynamic inquiries usually associated with the name of Onsager (thermodynamics of stationary processes) leads beyond the framework covered by these results. Although it would be unfair to expect an introductory text to burden the reader with relatively advanced and as yet not generally known results, some indication concerning possible limitations would appear in order.

Beginning with Chapter VIII, Møller moves on to the general theory of relativity. He lays a physical and mathematical foundation in three chapters, to present and discuss the field equations of the gravitational field in Chapter XI. In this chapter, Møller discusses both the weak-field (linear) approximation and the rigorous Schwarzschild solution. In the concluding Chapter XII, finally, Møller discusses both the experimental tests referred to above and the applications to cosmology. A series of brief appendices is devoted to a derivation of a number of useful mathematical and geometric results. The two indices consume a total of three pages.

PETER G. BERGMANN
Department of Physics, Syracuse University

Theoretical Nuclear Physics. John M. Blatt and Victor E. Weisskopf. xiv + 864 pp. Illus. $12.50. Wiley, New York; Chapman & Hall, London. 1952.

IN THE terminology of another field of criticism the book *Theoretical Nuclear Physics,* by Blatt and Weisskopf, easily rates four stars. Presumably intended as a textbook for second-year graduate students—or at any rate, graduate students with a knowledge of quantum mechanics—it is an excellent text. The amount and scope of the material presented are in generous excess of what would be presented in most one-year courses, and so ample opportunity for selection is afforded. The book is more than a textbook of nuclear physics, however. It should constitute a useful reference for both graduate students and the more mature research physicist and, in the opinion of this reviewer, it will retain its usefulness for a long time to come.

A mere listing of the table of contents does not convey an adequate idea of the thoroughness nor even of the completeness with which nuclear physics has been treated in this volume. In fact, of course, it is easier to cite the justifiable omissions: meson theories of nuclear forces (the status of which is such that a discussion of this topic is best deferred for some future book) and a few borderline fields which are, for the most part, applications of nuclear theory or auxiliary topics that are useful in the interpretation of some experiments but that do not properly belong to the main subject matter (stopping of charged particles, neutron diffraction, molecular and atomic beams, and reactor theory).

Specifically, the discussion includes: A careful discussion of general properties of the nucleus, wherein the reader obtains a first orientation as to what factors are important in nuclear structure; a fairly exhaustive account of the two-body problems at both low and high energies, in which it is brought out clearly just what one may learn concerning nuclear forces from the pertinent experimental data; the conclusions with regard to nuclear forces, with emphasis on the question of saturation; the three- and four-body problems; nuclear spectroscopy, both from a general point of view, wherein symmetry considerations alone are invoked, and on the basis of the several specialized models introduced in the past; a comprehensive treatment of nuclear reactions, in terms of both the Weisskopf and Wigner theories, and the application of the theory to experiments; the spontaneous decay of nuclei, which includes α-decay, emission and absorption of γ-rays, including the capture process, and associated electromagnetic phenomena such as internal conversion, internal pairs and forbidden (nonradiative) transitions, and finally β-decay, which is presented as completely and authoritatively as any review article; a concluding chapter on the (*j-j* coupling) shell model. The comparative brevity of this last chapter is understandable in view of the fact that a finite time interval must necessarily exist between final revision of a manuscript and the date of publication (spring of 1951 to late summer of 1952, in this case).

In any book of this kind there is always the question of compromise between the desire to present adequate detail and the desire to keep the size of the book within reasonable bounds. It is a truism that a measure of the skill of the writing is the degree of success with which burdensome details have been sacrificed without sacrificing clarity. This is especially important in nuclear physics, with its vast literature and the necessarily rather complicated and lengthy treatments of the various theoretical problems which arise in this field. The present authors are to be congratulated in this respect. Throughout the book the arguments are presented by means of a formalism that is nicely adjusted to the requirements of clarity. The discussion of many topics has been improved, as compared to hitherto existing treatments, and a considerable amount of new material has been added in the way of a critical analysis of the various theoretical developments. Adequate references to the

literature provide ample opportunity for more detailed examination of all topics. The liberal use of figures and the lists of symbols used, which appear at the end of each chapter, are particularly helpful.

If there is any adverse criticism, it lies in the fact that there are no problems in this book. The number of misprints is reasonably small, and most of these are easily detected.

M. E. Rose
Oak Ridge National Laboratory

FRONTIERS OF RESEARCH

Progress in Organic Chemistry, Vol. I. J. W. Cook, Ed. viii + 287 pp. $7.80. Academic Press, New York. 1952.

THE goal which the editor has set himself is indicated by the following excerpt from the foreword:

> Organic chemistry has become a vast subject and new knowledge is accumulating at an ever-increasing pace. For these reasons, comprehensive accounts of advances in every phase of the subject are apt to take so long to prepare that they tend to become somewhat out of date even before they are published. The present volume has limited objectives. An attempt has been made to present in a concise but adequate form descriptions of recent developments in selected fields of organic chemistry. The selection has been based on the importance or topical interest of the subjects chosen, and it is hoped that the book will have a widespread appeal to all classes of organic chemists. It has been written primarily for research workers and advanced students, and the liberal provision of references to the original literature will be of particular value to the former class of reader. Each of the contributors is an active worker in the field which he has reviewed, and this has ensured that every chapter is fully authoritative.

This, the first volume of a new venture, consists of eight chapters covering a wide range of topics: Molecular Structure of Strychnine, Brucine and Vomicine, by Sir Robert Robinson; Chemistry of Some Heartwood Constituents of Conifers and their Physiological and Taxonomic Significance, by H. Erdtman; Photodynamically-Active Natural Pigments, by H. H. Brockmann; Chemicals from Petroleum, by S. F. Birch; Acetylene Chemistry, by B. C. L. Weedon; Drugs Inhibiting Symptomatic Stimulators, by F. Bergel and M. W. Parkes; Free Radicals as Intermediates in Organic Reactions, by D. H. Hey; Starch and its Products of Amylolytic Degradation, by I. A. Preece.

The task which Professor Cook and his collaborators have attempted is a difficult one, and chemists cannot but be grateful to them. The majority of the chapters are well written, and several are especially fine. Among the particularly clear and interesting essays are Photodynamically-Active Natural Pigments, Acetylene Chemistry, and Starch. There are, however, two chapters that are not well written and that are something less than "fully authoritative."

One of these is Chemicals from Petroleum. For example, the discussions of the hydrolysis of 1,3-dichlorohydrin (p. 111), the mechanism of vapor phase nitration (p. 128), and the pyrolysis of diallyl ether (p. 111) are not good. And one would hardly say that the section on hydration of olefins (pp. 101–105) is either accurate or modern.

The chapter on Free Radicals as Intermediates in Organic Reactions is neither lucid nor critical. For instance, the Franck-Rabinowitsch effect is invoked where it should not be invoked (p. 227); without any reservations at all, hydrogen atoms are shown as the products of aromatic substitution by free radicals—sometimes with and sometimes without a bracket around the H· (the significance of the bracket is never explained); the ionization of hexaphenylethane is incorrectly described (p. 221). Topics are discussed without any indication that they are, in reality, only poorly understood—e.g., the Meerwein reaction (p. 234). The *ad hoc* nature of the discussion is typified by the fact that on page 230 a free radical (bromine atom) is stated to be "functioning as an electrophilic reagent," whereas on page 225 a free radical (phenyl) is described as "electrically neutral" and its behavior is contrasted with that of electrophilic reagents.

The success of this series will depend on the extent to which the various contributions are critical and lucid evaluations of recent developments. It will also depend on the topics discussed. For a volume that is entitled *Progress in Organic Chemistry* there would appear to be an unduly large emphasis on natural products and pharmacology.

Nathan Kornblum
Department of Chemistry, Purdue University

Advances in Catalysis and Related Subjects. W. G. Frankenburg, V. I. Komarewsky, and E. K. Rideal, Eds. Vol. III: xi + 360 pp. $7.80; Vol. IV: xi + 457 pp. $9.50. Academic Press, New York. 1951 and 1952.

ONE of the important functions of catalysts is to overcome the inertia exhibited by many reactions in their approach to equilibrium. Another concerns the manner of approach toward equilibrium and is described as the "selectivity" of the catalyst. Where more than one reaction path is thermodynamically possible, catalysts may be found that will favor one path above others. These two functions—namely, increasing the reaction rate and selectivity—make catalysis an intriguing and important subject in science and engineering.

In 1943 a compilation of data and thinking on the entire subject of catalysis was published (*Handbuch der Katalyse,* 6 vols. G. M. Schwab, Ed. Vienna: Springer-Verlag [1943]). The current publications, *Advances in Catalysis and Related Subjects,* are intended to be chiefly reports from the frontier of research in catalysis and to serve as a stimulus for

critical thinking and experimentation in this field. Two chapters which illustrate these purposes very well are Chemical Characteristics and Structure of Cracking Catalysts, by A. G. Oblad and others in Volume III, and Chemical Concepts of Catalytic Cracking, by R. C. Hansford in Volume IV. The first paper presents a novel explanation of the mechanism of cracking of hydrocarbons by silica-alumina and similar catalysts. In this mechanism the authors postulate a periodic change of 6-coordinated aluminum to the 4-coordinated state caused by the adsorption of a weak base (such as the hydrocarbon), and return to the 6-coordinated state upon desorption of the products. In the latter paper, Hansford is critical of this mechanism and argues strongly for the existence of protons on the catalyst surface at cracking temperatures, so that the chemical reactions in cracking of hydrocarbons on this basis are essentially similar to the "carbonium ion" reactions in homogeneous acid catalysis of hydrocarbon isomerization, alkylation, etc. To orient the reader in his thinking on this subject, Volume III includes a paper on Catalytic Cracking of Pure Hydrocarbons, by V. Haensel, and Volume IV includes a paper on the Structure and Sintering Properties of Cracking Catalysts and Related Materials, by H. E. Ries, Jr., and one on Acid-Base Catalysis and Molecular Structure, by R. P. Bell. It is to be expected that these correlations of homogeneous and heterogeneous catalysis will be extended to include the important field of hydrogenation. Although examples of homogeneous catalysis of hydrogenation reactions are as yet very few in number, a comparison of their mechanisms with those in heterogeneous catalytic hydrogenation would be of much scientific and practical value.

Volume III contains also an interesting presentation of steric factors in contact catalysis in Balandin's Contribution to Heterogeneous Catalysis, by B. M. Trapnell; and a clear presentation of the use of magnetic measurements to determine the degree and type of dispersion of catalysts on various supports in Magnetism and the Structure of Catalytically Active Solids, by P. W. Selwood. Other important parts of Volume III are The Poisoning of Metal Catalysts, by E. B. Maxted; Reaction Rates and Selectivity in Catalyst Pores, by A. Wheeler; Nickel Sulfide Catalysts, by W. J. Kirkpatrick; and Catalytic Oxidation of Acetylene in Air for Oxygen Manufacture, by J. H. Rushton.

Volume IV contains, also, two outstanding contributions to our thinking on adsorption on surfaces of solids—namely, Theory of Physical Adsorption, by T. L. Hill, and The Role of Surface Heterogeneity in Adsorption, by G. D. Halsey. Also in Volume IV are chapters on Decomposition of Hydrogen Peroxide by Catalysts in Homogeneous Aqueous Solutions, by J. H. Baxendale; The Free Radical Mechanism in the Reactions of Hydrogen Peroxide, by J. Weiss; The Specific Reactions of Iron in Some Hemoproteins, by G. George; and Twenty-Five Years of Synthesis of Gasoline by Catalytic Conversion of Carbon Monoxide and Hydrogen, by H. Pichler.

One volume per year of *Advances in Catalysis and Related Subjects,* has been published since 1949. These books are of great value to those scientists and engineers interested in keeping well informed in this field. The presentation of the subject matter could be improved by restricting each volume to a particular part or branch of catalysis and including some pertinent discussion of each chapter by specialists. When one remembers that "catalysis and related subjects" includes virtually the whole realm of chemistry, it is apparent that some specialization of each volume is desirable.

Henry H. Storch

Fuels-Technology Division
Bureau of Mines, Region VIII
Bruceton, Pennsylvania.

THE REPUBLIC NEEDS SCIENTISTS

Antoine Lavoisier. Douglas McKie. 440 pp. $6.00. Illus. Henry Schuman, New York. 1952.

NO WONDER that Lavoisier is a favorite scientist of the biographers. In the language of his own time, what a "natural philosopher" he was: a chemist who knew how to ask of an original research the right questions so as to get significant answers, but also a physicist and a mathematician—even a geologist and a botanist of sorts.

Known quite rightly as the Father of Chemical Science, Lavoisier did not disdain to improve the manufacture of gunpowder and the street lighting of Paris. He replaced the deliberately mystic jargon of the alchemists with a sensible chemical nomenclature. He checked and revised the metric weights and measures. He outlined educational reforms far in advance of his day. In his clever hands the clumsy laboratory apparatus at his command yielded results of famous accuracy. He marshaled his facts—chemical, financial, or political—with proverbial French logic, and he wrote easily and well.

But Lavoisier was also a practical financier and a sound theoretical economist, a successful industrial executive and a prosperous farmer who turned his rather infertile acres into one of the first experiment stations. In a corrupt era, he served a bureaucrat-ridden government with wholehearted devotion to public service and with scrupulous personal honesty. He was a patriot who worked hard for the material prosperity and military security of France; a humanitarian who strove to lighten the burdens, especially the tax burden, of the poor.

Withal Antoine Lavoisier was a handsome, personable man of means, a patron of the opera, married to a beautiful, talented woman whom he adored. And in his prime, at fifty-one, he died under the guillotine.

Dr. McKie's new life of this amazing man has three great merits. It presents Lavoisier in the round. It contains new facts from original sources. It is pleasantly, effectively written.

Reading this book you need no chemistry to under-

stand Lavoisier's epochal experiments and what they meant. Neither need you be expert in French history to realize vividly the atmosphere in which he lived, the people who touched his life, the onrush of events that destroyed one of the founders of our scientific age. Here is Lavoisier as he truly was, an eighteenth-century Leonardo da Vinci; compeer and contemporary of our own great men of multiple genius, Franklin and Jefferson. While thoroughly enjoying yourself with this book, you are bound to pick up a lot of chemistry and history, to say nothing of a refreshed appreciation of the spirit of science laboring in the service of mankind. It is heartening today to recall, as McKie aptly does, how much greater influence, in the long run and for the good of us all, Lavoisier's scientific discoveries and humanitarian ideals have had during the past century and a half that did the brutal struggle for power between stubborn conservatism and stark radicalism in which he innocently lost his life.

WILLIAMS HAYNES

Stonecrop Farm
Stonington, Connecticut

MONUMENT REVISED

Illustrated Flora of the Northeastern United States and Adjacent Canada. Henry A. Gleason. Vol. 1, lxxv+482 pp.; Vol. 2, 655 pp.; Vol. 3, 589 pp.; 4660 text figures. $27.50 postpaid until July 1; thereafter, $30.00 plus 50 cents postage. New York Botanical Garden, New York. 1952.

TWO thousand years ago a Greek philosopher cautioned critics that their function was not to air their own knowledge, "not to make talk and discourse, but to weigh and consider." To do either with regard to the new *Illustrated Flora,* which will certainly become a classic, does need a little knowledge of what led to the first edition in order to understand this one.

Over fifty years ago the late Nathaniel Lord Britton conceived the ambitious plan of a flora of the Northeastern states in which every species would have an illustration. It was a revolutionary idea for America, although it had been done for several countries in Europe. Much more revolutionary than having a picture for each plant was the Brittonian concept of what constituted a species, a genus, or a family of plants—not to mention his highly controversial attitude regarding Latin nomenclature.

Under this aegis appeared the first edition of the *Illustrated Flora* in 1896–98, and a second in 1913, which has never until now been revised, although knowledge of our flora has increased enormously. Both were issued under the joint authorship of N. L. Britton and Addison Brown, but everyone understood that the books were written by Dr. Britton, who founded, and was the first director of, the New York Botanical Garden.

The prestige of the book's author, and the brilliant future of the newly established garden, made its issuance a landmark in American botanical history. But it was a landmark with many difficult peaks, some jagged promontories, a badly eroded coastline, and a few pitfalls too deep for comfortable acceptance. One condoned these because there was no other completely illustrated flora of the region. It never deceived the knowing that these handicaps were serious, because the book embodied the Brittonian ideas outlined above, most of which have since been repudiated.

The book has long been out of print, and it was peculiarly fitting that the Board of Managers of the New York Botanical Garden, in May 1939, should have authorized the publication of a completely rewritten and revised edition of Britton and Brown's *Illustrated Flora.* It is this that has just been issued.

The selection of the author for such a major undertaking was most fortunate. Henry A. Gleason, by lifelong training in systematic botany, seems the one inevitable man to make the revision what it is, and perhaps the best qualified to appraise all that was worth retaining from the old edition, reject most of the Brittonisms, and add the salient features that modern research has uncovered since 1913. He has done this immense job with such brilliant success that the book will be a classic for the next fifty years.

Competence to complete such a task calls for qualities that are not often found among botanists. The book is necessarily technical, for how could it be otherwise when 4660 species, and many varieties, are described and keys provided for their identification? If that were all, the amateur might think the book too difficult to use or too time-consuming to be worth the effort. Dr. Gleason has made hundreds of decisions that help to simplify an admittedly difficult problem—that of making the book technically above reproach and at the same time making it available to the inquiring amateur.

THE SCIENTIFIC MONTHLY is not the place, nor does it have the space, to go into the details of such a large book, but one illustration of Gleason's viewpoint is worth quoting. For years sharp-eyed specialists have detected about 400 of what they call "species" of blackberries and their relatives. It is a truism that most of these exist only in the minds of their authors, and, as Gleason points out in his introduction, "it is not necessary that all such minor groups be given names." He admits just 24 species of *Rubus,* knowing well that each of them is a complex of highly variable brambles, the nomenclatorial designation of which does not help science but enormously confuses everyone.

Although comparisons are usually invidious, they are necessary for anyone who wants to decide whether to buy the new *Illustrated Flora* or its only possible rival —the recently issued eighth edition of *Gray's Manual,* by the late M. L. Fernald. Perhaps the best answer is that Fernald admits 205 "species" of *Rubus,* Gleason 24. To specialists who will say that the Gleason treatment is inadequate, one might venture the reminder that there is such a thing as knowing more and more about less and less. There are scores of other genera in both books that differ in this fundamental concept,

the best illustration of which is that *Gray's Manual* contains 5523 "species" whereas the new *Illustrated Flora* comprises only 4660. "Species," as Gleason well knows, are merely concepts of convenience, not immutable entities in nature. He has kept that steadily in mind in his revision.

Users of the new *Illustrated Flora* will find many changes. The illustrations are now grouped by genera, on pages and half-pages, and this makes for convenience in contrasting related species. All the old synonyms have been omitted, but any change in name, as between this edition and older standard reference works is always noted.

A valid English name is supplied for all plants that have one, but hundreds of so-called common names, like "cordate-leaved aster," "setose violet," etc., have rightly been killed. A curious omission is for *Albizzia julibrissin,* which Gleason calls "acacia," but which is universally called "mimosa" in Washington—both names, of course, being incorrect! Also, he uses *Wisteria* instead of *Wistaria,* thought by most to be the correct spelling for a plant named for Caspar Wistar. The correct vernacular name for *Daphne mezereum,* also, is "mezereon," not "mezereum."

Other misprints could doubtless be found by anyone who wants to look for such flyspecks upon a monumental and outstanding undertaking, but such minutiae will never obscure the fact that the Garden and Dr. Gleason have put us all under that kind of obligation for which there are no adequate words except, perhaps, "Well done, and thank you!"

NORMAN TAYLOR

Elmwood, Princess Anne, Maryland

PALEOECOLOGIC SYNTHESIS

Histoire Géologique de la Biosphère, la Vie et les Sédiments dans les Géographies Successives. Henri Termier and Geneviève Termier. 721 pp. Illus.; maps. Paperbound, 9600 French fr; clothbound, 9200 French fr. Masson & Cie., Paris. 1952.

PROFESSOR and Mme. Termier, of the University of Algeria, have written a scholarly, original, and timely book. It is the best paleoecologic synthesis to date and may well initiate a brand-new academic approach to the teaching of earth history. The authors set themselves the enormous task of summarizing the vast data on the biosphere through all geologic time. To have produced a useful work on so immense a subject, the data for which are so diffusely seeded through the literature of stratigraphic geology and paleontology, within the covers of a 700-page book, bespeaks immense powers of concentration and synthesis. As with all such attempts, the specialist will instantaneously find ample grounds for complaint. Carefully prepared bibliographies will help in some measure to furnish documentation otherwise omitted. To the European–North African experience of the authors can be attributed the consistently more satisfactory treatment of the Old World than of the New and of Asia.

The book is divided into two unequal sections. The first (260 pp.) treats in an excellent manner with general geographic, geologic, sedimentary, and ecological matters as related to the biosphere. The second part traces the successive geographies of the earth's history. The 35 colored world paleogeographic structural maps that illustrate this section are valuable generalizations. To be sure, like all such attempts, the representations are highly controversial, the while being undoubtedly the best thing so far drafted. To this reviewer the Termier treatment of trans-Atlantic passage of the Paleozoic geosynclines is quite unsatisfactory. However, to each his own bias! Too bad the French language (as well as the price) will stand as a barrier to North American students. The world picture, painted in broad strokes so well by the Termiers, is exactly the general knowledge lacking in current English language texts of historical or general geology. Happily, there will be no language barrier to fullest understanding of the fine maps.

In addition to her labors as co-author, Mme. Termier has prepared the excellent illustrations. The maps are of striking conception and excellent execution, although some other projection than the Mercator would have been a better base for the geographies. The line figures are clear and telling. However, the full-page original lithographs, although of undoubted artistic merit, do not contribute sufficiently to warrant the considerable additional cost which their inclusion has most certainly meant. It is a pity that a book of such potential usefulness is so exorbitantly priced.

KENNETH E. CASTER

Department of Geology, University of Cincinnati

INTUITIVE GEOMETRY

Geometry and the Imagination. D. Hilbert S. Cohn-Vossen, xi + 357 pp. Illus. $5.00. Chelsea Publishing Co., New York. 1952.

Auschauliche Geometrie has been a classic for twenty years. Its breadth of outlook is reminiscent of Klein's *Elementary Mathematics from an Advanced Standpoint.* Although it deals with elementary topics, it reaches the fringe of our knowledge in many directions. This excellent translation by Dr. Neményi may help to restore interest in geometry, a subject that seems lately to have lost favor in America.

The first chapter (The Simplest Curves and Surfaces) provides a synthetic introduction to conics and quadrics, including the construction for a central quadric by means of a loop of thread pressing against the focal conics: the solid analog of the familiar thread construction for an ellipse.

The second (Regular Systems of Points) is an original treatment of lattices and quadratic forms, sphere packing, crystallographic groups, and regular polyhedra. The drawings of polyhedra are unusually perspicuous because of the clever device of exaggerated perspective: an

edge not parallel to the picture plane appears thicker at the near end and thinner at the far end.

The third chapter (Projective Configurations) gives a clear account of the configurations n_3 consisting of n points and n lines, 3 points on each line, 3 lines through each point. These exist in the complex plane for $n > 7$, and in the real plane for $n > 8$. The cases $n = 9$ and $n = 10$ include the figures for the theorems of Pappus and Desargues, which play an important role in the foundations of geometry. Then comes a well-illustrated description of Reye's three-dimensional configuration of 12 points, 16 lines, and 12 planes, and a superb drawing (p. 152) of the related four-dimensional polytope $\{3, 4, 3\}$. Another very successful drawing is that of Schläfli's double-six of lines on page 165.

The fourth chapter (Differential Geometry) leads the reader gently into various ramifications of the theory of curvature, including the spherical image, monkey-saddles, minimal surfaces, stereographic projection, and non-Euclidean geometry, all without any explicit use of coordinates or vectors or tensors. Figure 204 is a reproduction of Klein's model of the Apollo Belvedere, with all the parabolic curves marked on the face and neck. Another remarkable feature of this chapter is the section on "Eleven Properties of the Sphere," some of which serve to characterize the sphere and others are unexpectedly found to be shared by different surfaces.

The fifth chapter (Kinematics) begins with Peaucellier's inversor (which enables one to draw a straight line in a given plane without using a ruler) and an analogous three-dimensional linkage, with universal joints, for constructing a plane. This leads naturally to a discussion of continuous rigid motion and of roulettes, including R. C. Yates' instrument for constructing the meridian of a surface of revolution having constant positive Gaussian curvature.

The sixth chapter (Topology) contains a classification of two-sided and one-sided surfaces in terms of the "connectivity" h, which is related to the Euler-Poincaré characteristic by the formula $V - E + F = 3 - h$ (*cf.* W. W. R. Ball's *Mathematical Recreations and Essays*, 11th ed., p. 232). We find here a description of Boy's surface, which is closed and one-sided with $h = 2$ (like the projective plane) and yet has no singular points nor any points of discontinuity of curvature, although of course it intersects itself along certain curves. (The only way to avoid self-intersection is to work in four dimensions; this is done in an appendix.)

The only serious error of translation is the omission of the words, "of motions" from page 87, fourth line from the bottom. Some readers might be confused by a typographical error in the first two lines of page 264, where z and $\bar{z}$ should be interchanged. In a few places errors of the German edition have been carried over. The most serious of these is on page 92 (second line from the bottom), where "octahedral" should be "tetrahedral." In the footnote to page 290 we read that "whereas every closed convex polyhedron is rigid, there are closed non-convex polyhedra whose faces can be moved relative to each other." The only such figure known to the reviewer (see W. W. R. Ball, *op. cit.*, p. 153) is not a legitimate example, as some of its edges coincide in pairs.

But these are mere criticisms of detail, hardly worthy of mention in a book having such an extraordinarily high concentration of interesting ideas and information. It is a sad thought that neither the original publisher nor the heirs of the authors can benefit from the sale of this edition.

H. S. M. Coxeter
Department of Mathematics
University of Toronto

BRIEFLY REVIEWED

Unit Processes in Organic Synthesis. (4th ed.) P. H. Groggins, Editor-in-Chief. xiii + 937 pp. Illus. $12.50. McGraw-Hill, New York. 1952.

THE fourth edition of this well-known text resembles the third in general excellence and differs from it in only a few respects. The principal contributors, W. P. Hamner (Hydrolysis), and M. Orchin and W. C. Shroeder (Hydroformylation), have replaced four others who contributed to the previous edition. A new chapter on the currently interesting oxo reaction (Hydroformylation) has been added. Each of the old chapters has been retained but brought reasonably well up to date. There is increased emphasis on aliphatic compounds, on the economics of manufacture, and especially on theoretical aspects such as the kinetics and thermodynamics of industrial reactions.

The book is fairly free of errors, although the reviewer objects to such things as the repeated use of two charges on ions that are actually singly charged—e.g., pp. 21 and 26; the statements, "the volume of which is not over 2,000 lb."—p. 153; and, "When the alcohol has a neopentyl group as in *tert*-butyl alcohol . . ." and the inconsistencies in nomenclature and reaction mechanism which always seem to be present in compilations of the works of a number of different authors.

This member of the McGraw-Hill Chemical Engineering Series belongs in the private libraries of most chemical engineers and many organic chemists.

G. B. Bachman
Department of Chemistry, Purdue University

Polarography, Vol. II. (2nd ed.) I. M. Kolthoff and James J. Lingane. 588 pp. Illus. $11.00. Interscience, New York–London. 1952.

THE first volume deals with Theoretical Principles, and with Instrumentation and General Methods. Part Three, which begins Volume II, consists of a thorough systematic review of the polarography of 72 elements. In most cases sufficient details and data are

given so that recourse to the original literature becomes unnecessary. This part is concluded with a chapter in which selected procedures are presented in detail for the analysis of various types of alloys and other technical materials.

Part Four, which deals with the polarography of organic substances, was written with the assistance of Stanley Wawzonek, who is thoroughly acquainted with polarography of organic compounds. This section is exceptionally well done in 10 chapters. One may obtain an idea of the wide variety of compounds discussed by mentioning a few section headings: aldehydes, 3-ketosteroids, organic acids, nitro compounds, amines, pyran derivatives, alkaloids, penicillin, and chlorophylls.

Biological applications of polarography are discussed in Part Five, written with the aid of W. Stricks. Part of this chapter is devoted to Serological Applications of Catalytic Waves for the Diagnosis of Cancer. Part Six presents amperometric titrations. This section clearly indicates the tremendous progress of this new technique since the first edition of this text.

An author and subject index for both volumes is given at the end of Volume II.

The authors have performed a great service to those who use polarographic methods of analysis by systematically arranging a voluminous amount of material scattered in the literature. These volumes are also an excellent source for those who wish to know more of this intriguing method of analysis.

THOS. DE VRIES

Department of Chemistry, Purdue University

The Growth and Development of the Negro in Dentistry in the United States. Clifton Orrin Dummett. \$5.00. Printed by the Stanek Press, Chicago, for the National Dental Association. 1952.

THE story of the struggles of the American Negro to throw off the shackles of slavery and prejudice and to take his rightful place in world affairs has always been an impressive saga. This book describes the difficulties and obstacles met by those of that race who seek to elevate themselves to a professional level in the field of dentistry. These obstacles are dealt with in a frank and forthright manner, and the present status of the Negro in dentistry is very clearly presented.

Through the years the Negro, especially in the South, has been handicapped by inadequate educational facilities and financial resources from engaging in graduate study. Those who were able to continue their education in order to qualify in the fields of science and literature were faced with racial prejudice and the inability to gain entrance to schools of higher learning. All along the way their progress has met with opposition, and they have succeeded only by overcoming great odds.

Although handicapped by inferior preliminary education and lack of financial support, many Negroes have overcome these difficulties and have become well-qualified practitioners and teachers in the field. At the present time there are two Negro schools, Howard and Meharry, and 22 other schools have accepted a limited number of Negro students.

The Negro dentists have their own national organization, the National Dental Association, and in many states are accepted in the American Dental Association. In Howard University five Negro teachers hold Ph.D. and M.D. degrees, and in Meharry there are four teachers who hold Ph.D. degrees. Three other dental schools have Negro teachers on their staff, and eleven have signified their willingness to employ qualified Negro teachers. Negro dentists are also represented in the U. S. Public Health Service and the Veterans Administration.

With present-day decreasing racial discrimination and the recognition of the equality of man, many of the former hindrances to dental education for the Negro are disappearing. Now there is need for encouragement of young men of that race who have the educational background to engage in the study and practice of dentistry. They are greatly needed for the care of their own race, and many could serve the dental needs of the white population.

Opportunities should be opened for a larger enrollment of Negroes in dental schools. Negro leaders do not believe that this can be accomplished by the creation of additional segregated schools. Rather, they favor increased support and enlargement of the two existing Negro schools; also, that all dental schools should open their doors to well-qualified Negro students on the same basis as to whites.

Dr. Dummett's book is well worth reading. The history of the advancement of our Negro confreres in dentistry cannot fail to stir our sympathy and admiration for what they have been able to accomplish. It should also stimulate greater interest and active co-operation in helping them to take their rightful place in dental health services.

R. W. BUNTING

School of Dentistry
University of Michigan

Child Psychotherapy. S. R. Slavson. 332 pp. \$4.50. Columbia University Press, New York. 1952.

THIS is a veritable guidebook and manual for social workers, psychiatrists, case workers, psychologists, intelligent parents, and all others who are concerned with children. The author is director of group therapy at the Jewish Board of Guardians of New York, and consultant for the Youth Consultation Service, the Newark Child Guidance Clinic, and the Council Child Development Center. He is also editor of the *International Journal of Group Psychotherapy* and chairman of the Commission on Group Therapy of the World Federation of Mental Health.

In the preface to *Child Psychotherapy,* he says that, although his book is primarily intended as a clinical study, with special emphasis upon the treatment process of the emotionally disturbed and socially maladjusted child under twelve years of age, he found it advisable

to point out other elements as well. The book is divided into three main parts—Development, Pathogenesis, and Psychotherapy—and is the outcome of four decades of work with people in many relationships and settings (the majority were children and adolescents). It also reflects twenty years of clinical experience that favored learning, experimentation, and reflection.

Slavson himself says that many of the ideas contained in the volume just published may not be new, but he hopes that new vigor and meaning are given by a "fresh restatement in a different context, in new relations and in the integrative approach to the bio-psycho-social entity that is man."

A very important chapter outlines the personal qualifications, educational background, skills, and functions of the psychotherapist. The last chapter, Treatment of a Neurotic Nine-Year Old Boy with Organic Deficiency, is a case study (25 pp.) of Harry Peters, with a summary of the psychiatric case workers' treatment. The case is taken from the files of the Child Guidance Institute of the Jewish Board of Guardians, New York. The volume contains other case histories, although they are not as complete as the one describing the treatment of the neurotic nine-year-old.

Your reviewer wishes to emphasize that Slavson's *Child Psychotherapy* is a "must" book for social workers, particularly group workers and all others who deal intensively with youth.

PHILIP L. SEMAN
Sociology Department
The University of Judaism, Los Angeles

Hydrogen Ion Concentration. John E. Ricci. xxxvi + 460 pp. $10.00. Princeton University Press, Princeton, N. J. 1952.

PROFESSOR RICCI begins by stating that the formulas usually used for calculating the hydrogen ion concentration of aqueous solutions have been obtained by derivations from loosely held postulates. He proposes that it is in a certain sense simpler and certainly better to derive exact mathematical relations for the hydrogen ion concentration and then to simplify these, if warranted. He has been able to derive these exact relations without significant reference to special theories of ionization. He has given us a rigorous and thorough mathematical treatment of the hydrogen ion concentration of various aqueous solutions. The reader is required only to attend diligently to the lengthy arguments, which are presented in an ordered manner.

This book cannot easily be used as a reference (and this seems to be its primary purpose) for a particular question about pH or a related phenomenon until the user has rather carefully perused the entire book. Such a condition could be cause for criticism, but in this case it follows as a necessary consequence of the rigorous manner in which the subject is treated, although the occasional use of unfamiliar terms for familiar concepts and the very large number of symbols used, listed, and defined (on pp. xxvii–xxxv) tend to make the difficult task of understanding somewhat less pleasant than it could have been. The humorless and occasionally disputatious style used by the author also does nothing to enhance the readability of the work.

Briefly, the relation between hydrogen ion concentration and the concentrations of solutes present in solution is derived by combining equations pertaining to the net ionic charge, degree of ionization of the solute, the ion product of water, etc. Generally, this yields an equation of high degree in $[H^+]$, simple to derive, but laborious to solve for a numerical value of $[H^+]$. The author describes a simple procedure for solution of the equations. The last five chapters treat solubility of electrolytes and its relation to pH as applied to the separation of one solute from another and to precipitation titrations. Among the chapter headings are:

Definitions and Fundamental Relations; the Theorem of Isohydric Solutions; Limits of Ionization Fractions and the Iso-electric Point for Solutes and Ampholytes; Calculation of the Numerical Value of *H* from the General Equations Titration (with Strong Base or Strong Acid) in Mixtures Involving any Number of Independent Ionization Constants; Titration with Weak Acid or Weak Base; Problems Involving Two Interdependent Ionization Constants; Tribasic Acids; Salts with Strong and Weak Bases; Saturation with Respect to Acids and Bases; Pure Saturated Aqueous Solution of a Salt; Saturation with Salts of Monobasic Acid; Saturation with Salts of Dibasic Acid; and Saturation with Ampholytes and their Salts.

A thorough treatment of hydrogen ion concentration relations in aqueous solutions has long been due, and Ricci is to be complimented for undertaking the arduous task of laying out the broad principles of approach. It is hoped that others will follow him in further developing this approach.

JAY A. YOUNG
Department of Chemistry, King's College
Wilkes-Barre, Pennsylvania

The Petroleum Dictionary. Lalia Phipps Boone. xiii + 338 pp. $5.00. University of Oklahoma Press, Norman. 1952.

HERE is a rich and rewarding book for the dialectician, the colloquialist, and the folklorist, a disappointing book for the scientist or the engineer. The author's field is linguistics. She has taught school in the oil fields, is married to an oil man, and has lived in the oil country for many years. Her long-time interest has been the picturesque language of the oil man; she says in her preface, "Its freshness, its peculiarities, and its vividness fascinate me today." Those qualities she has succeeded in capturing, first in the discussion of usage and sources in Part One, "The Language of the Oil Field" (37 pp.), and, second, in many of the definitions in Part Two, "The Petroleum Dictionary" (299 pp.).

Unfortunately, in her technical and semitechnical

definitions, she has not always captured the accuracy on which the smooth operation of the oil industry depends. This is not surprising; chemistry is the only science to which the author confesses—and some of her chemical definitions are questionable. She says, "I have concentrated my attention primarily upon non-technical usage." Her technical definitions attest the truth of the statement; so do some of her semitechnical ones.

In the following random examples, technical, semitechnical, and nontechnical, the italics are the reviewer's:

Acre-foot: An acre of land 1 foot wide and 43,560 feet long.

Anticlinal axis: The line on a geological map which indicates the axis of a fold.

A.P.I. gravity: The gravity specified by the American Petroleum Institute.

Asymmetric anticline: An anticline without plane or symmetry which lacks proportion in the parts.

Bituminous sandstone: Any rock formed of coherent or cemented sandstone.

Catalyst: The chemical agent in the presence of which petroleum products are cracked.

Oil refining: The process of removing undesirable impurities from crude or partly finished oils; generally a distillation process combined with a simple chemical treatment.

Old age: Strata which are considered to be many ages old. Some strata have eroded or weathered more than others.

Red beds: A sticky red shale belonging to the Wichita formation of the Permian series.

A *geologist* is a "bird dog, exploration crew, geoilogist, mud smeller, pebble picker, pebble pup, rock hound, rocksy, roxy, sand smeller, smeller, wrinkle chaser." Each of these terms is defined, but under none of them is a geologist defined. Your reviewer still wonders what he really is.

MAX W. BALL

1025 Vermont Ave., N.W.
Washington, D. C.

Sun, Moon, and Planets. Roy K. Marshall. x + 129 pp. Illus. $2.50. Henry Holt, New York. 1952.

THIS reasonably priced booklet is written for the general public and contains answers to many of the questions that intelligent laymen might ask about the solar system. As far as this was the aim of the author, he succeeded and has produced an account which reads easily and should not be beyond the grasp of anyone who is sufficiently interested to wish to read about our solar system at all.

The art of writing popular books on scientific topics is extremely difficult, and one seldom finds a volume that does not fall into either of two traps. These two pitfalls are, on the one hand, the oversimplification and underestimation of the powers of elementary reasoning of the public and, on the other hand, assuming knowledge which is elementary to the expert but often unavailable to the layman. In my opinion, Dr. Marshall has not been able to avoid either of these. To give a few examples, all from Chapter 2: the author states Kepler's third law and discusses as an instance the pair Pluto-Mercury and then goes on to say "arithmetic is a little more difficult for other pairs," which seems to me to be an instance of treating his readers as if they were nursery school pupils. On the other hand, the foci of an eclipse are mentioned (and the phonetic spelling of the word "foci" given) without explaining the meaning of the focus of an eclipse, and similarly the term "eccentricity" is introduced without further explanation. A final instance is the following statement in the discussion of Kepler's third law: "Newton showed that there were small deviations to be found in working out Kepler's Third Law, and that these were due to different masses of the planets. Because the sun is so much more massive than any of the planets . . . the earlier investigators did not have a chance to detect these small deviations." This statement seems to be rather cryptic for laymen.

Altogether, I feel that this book, which probably will reach a vast public, could have gained very much if the author had taken slightly more trouble in preparing it. Popularizing science is not equivalent to abandoning the rigor of an argument, and this booklet contains too many loosely phrased sections which could easily be rigorized without destroying their understandability.

Algebraic Technique of Integration. (2nd ed.) Harris F. MacNeish. viii + 133 pp. $2.50. Wm. C. Brown Co., Dubuque, Iowa. 1952.

THIS booklet gives a comprehensive survey of integration methods. The methods of integrating rational functions, rational fractions (by using a streamlined method), and functions of trigonometric, hyperbolic, and irrational functions are treated in detail, and many examples are given. A new method, that of "logarithmic integration," is discussed, and examples of its application are given.

The book includes a discussion of elliptic integrals and beta and gamma functions, and should be a useful guide for anyone who needs to evaluate integrals that are sufficiently different from the standard ones not to be included in the usual tables.

Radio Astronomy. Bernard Lovell and J. A. Clegg. 238 pp. Illus. $4.00. Wiley, New York. 1952.

IT IS seldom realized that the earth's atmosphere is practically completely opaque to electromagnetic waves of nearly all wavelengths. There are, however, two "windows," one in the visible region extending only very slightly into the infrared and ultraviolet, the other extending from about 0.25 cm to 20 m. Although the first window has been used since time immemorial for astronomical observations, only the great improvement in radio techniques during the past war made it possible to use the second window and gave rise to the new

science called "radio astronomy." The present account of this science is written by the professor of radio astronomy in Manchester University and one of his associates at the Jodrell Bank Experiment Station. The authors have attempted successfully to write in such a way that as wide a circle of readers as possible may get an idea of the importance, aims, and results obtained so far by radio astronomy.

After a few introductory chapters on the basic ideas of astronomy and radio techniques, the study of meteors by radio techniques is discussed in great detail, and it is shown how this new technique has greatly improved our knowledge of the "shooting stars." The emission of radio waves from the sun, their possible origin, and the problems connected with these phenomena are discussed in Chapters 12–15.

Although in meteor and solar physics radio astronomy merely supplements the older astronomical methods, radio emission from our galaxy has led to many new and still not completely understood phenomena. The most striking example was the discovery of the so-called radio stars—that is, centers of greatly enhanced radio emission at positions where no ordinary stars can be found. It is amusing to read that these radio stars twinkle as do their commoner relations, and for the same reason—atmospheric irregularities.

The last chapters of the book deal with the aurora borealis, the moon, planets, and Gegenschein.

Altogether this book gives a comprehensive, lucid account that can be recommended to all people interested in recent developments in physics or astronomy. The style is easy, and mathematics is kept to a minimum. The only point where I differ from the authors is on the subject of references, and I would like to have seen a list of references as a separate appendix, which would not have broken the continuity of the text and would have increased even further the usefulness of this volume.

Advances in Electronics, Vol. IV. L. Marton, Ed. x + 344 pp. Illus. $7.80. Academic Press, New York. 1952.

THE fourth volume in this series lives up to the high standard the editors have set themselves. It contains seven contributions dealing with subjects as varied as electron scattering in solids and multichannel radio telemetering. Extensive indices enhance the usefulness of the volume.

In the first article Massey gives a comprehensive account of electron scattering in solids. He deals especially with elastic, inelastic, and multiple scattering, energy loss and electron mobility in metals, alloys, and semiconductors. The article is a pleasure to read. The author points out deficiencies in our knowledge and gives an extensive bibliography.

The second article, by Morton, deals with the scintillation counter. It is hard to imagine that this widely used piece of equipment is only five years old. After dealing first with more general principles, the article concludes with a discussion of applications of the scintillation counter.

Van der Ziel discusses in his article fluctuation phenomena and, more specifically, the different kinds of noise that occur in diodes, triodes, gas-discharge tubes, photocells and photomultipliers, semiconductors, transistors, and so on. The discussion is mainly theoretical and seems to cover nearly every conceivable case of noise. A comprehensive bibliography in three sections (textbooks, survey articles, and research papers) concludes this contribution.

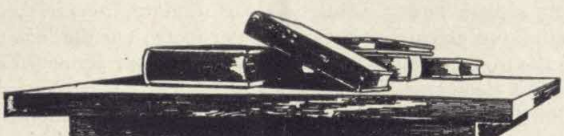
The next contribution, by C. V. L. Smith, discusses electronic digital computers. Alas, there is no bibliography, but the reader is referred to another treatise on the same subject. After a general introduction dealing with the principles involved in the planning of an electronic computer, the author describes in detail the Whirlwind and SEAC computers, which are now operating satisfactorily. Apart from the omission of references, this contribution is one of the best and most instructive in the volume. Donal describes in his article the modulation of continuous-wave magnetrons.

One of the methods used during the last war to spot submarines was by measuring the change in the earth's magnetic field that are due to the ferromagnetic body of the submarine by means of magnetic airborne detectors. These instruments, their mode of operation, and the difficulties encountered in their use are discussed by Fromm.

Another subject that owes much of its development to wartime requirements is that of multichannel radio telemetering. A telemeter is an instrument for measuring one or more quantities, transmitting the result to a distant station and then indicating or recording the quantities measured. From this definition one sees the importance of telemetering for aircraft development, since these instruments can be used to record data during the flight of pilotless models. The development of multichannel telemetering is described by Pawley and Triest in the last article of this excellent volume.

D. TER HAAR

Department of National Philosophy
The University, St. Andrews, Scotland



ASSOCIATION AFFAIRS

CONSTITUTION AND BYLAWS

AT ITS first session at St. Louis on Dec. 27, 1952, the AAAS Council passed the revised Constitution and new Bylaws without a dissenting vote. Although both documents were published in SCIENCE (116, 575 [1952]), the Constitution stipulates that they be reprinted in both Association journals and also that they become effective one month from the date on which Council action was taken—January 27, 1953. Hereafter, and until amended, the work of the Association and its officers will be governed by the Constitution and Bylaws as printed below:

Constitution

Article I

Section 1. The American Association for the Advancement of Science was incorporated by an act of the General Court of the Commonwealth of Massachusetts in 1874. The Association is a nonprofit scientific and educational body.

Section 2. The objects of the American Association for the Advancement of Science are to further the work of scientists, to facilitate cooperation among them, to improve the effectiveness of science in the promotion of human welfare, and to increase public understanding and appreciation of the importance and promise of the methods of science in human progress.

Article II

Section 1. The membership of the Association shall consist of Members, Fellows, and Associates. Individuals in any of these three groups may become life members, emeritus members, and sustaining members in accordance with the provisions of Section 5 of this Article and with such relevant rules as the Board of Directors shall have prescribed.

Section 2. Members. Any person, institution, or organization may be admitted to the grade of Member. Each Member shall have such rights and privileges and shall pay such annual dues and fees as the Council shall have prescribed.

Section 3. Fellows. Any person who shall have made a meritorious contribution to science may become a Fellow of the Association under such procedures as the Board of Directors shall have prescribed.

Section 4. Associates. Any person who shall have a record of leadership in any field related to science and who wishes to cooperate in the advancement of science may become an Associate of the Association under such procedures as the Board of Directors shall have prescribed.

Section 5. (a) Life Members. Any person making the Association a life-membership contribution of such amount as the Board of Directors shall have prescribed may be admitted to life membership. Each Life Member shall be exempt from the payment of annual dues and shall have all the privileges of an annual member throughout life.

(b) Emeritus Members. Any individual annual member may be admitted to emeritus membership under such conditions as the Board of Directors shall have prescribed. Each Emeritus Member shall be exempt from the payment of annual dues and shall have all the privileges of an annual member throughout life.

(c) Sustaining Members. Any person making to the Trust Funds of the Association a sustaining membership contribution of such amount as the Board of Directors shall have prescribed shall be the founder of a Sustaining Membership, which shall bear his name and shall be maintained in perpetuity as a trust. Each incumbent of a sustaining membership shall have all the privileges of a life member. The first incumbent of a sustaining membership may be either the founder himself or another person named by him, as he may choose. On the death or resignation of an incumbent, the Board of Directors shall name another person to hold the membership throughout life.

Article III

Section 1. The officers of the Association shall be (*a*) general officers elected from among the Fellows by ballot of the Council, and (*b*) administrative officers elected by the Board of Directors as prescribed in Section 3 of this Article.

Section 2. General Officers. The general officers of the Association shall be a president-elect, a president, a retiring president, and a vice president for each section. The term of office of the president-elect and of the vice presidents shall begin on the January 15 following their election. At the close of the one-year term of the president-elect he shall become president, and at the close of the one-year term of the president he shall become retiring president. In the event of a vacancy in the office of the president, the president-elect shall become president. In the event of a vacancy in the office of president-elect, the Board of Directors shall make a pro tempore appointment to hold until the vacancy shall have been filled by ballot of the Council. In the event of a vacancy in the office of vice president the Board of Directors shall fill the vacancy by appointment.

Section 3. Administrative Officers. The administrative officers shall be an administrative secretary, one or more associate or assistant secretaries, a treasurer, and, in addition, a secretary for each section. The administrative secretary, the associate or assistant secretaries, and the treasurer shall be elected by the Board of Directors. The secretaries of the sections shall be nominated from among the Fellows by the respective section committees and elected by the Board of Directors. The terms of office of each administrative officer shall be determined by the Board of Directors. The Board of Directors shall fill vacancies in the administrative offices.

Section 4. The duties of the officers shall be customary to those of the office and as further defined in the bylaws.

Article IV

Section 1. The Council shall perform duties prescribed

in the constitution and shall act as an advisory body in matters pertaining to the general policies of the Association.

Section 2. The Council shall consist of (*a*) the president-elect, the president, the retiring president, the vice presidents, the secretaries of the sections, the administrative secretary, the treasurer, and the eight (8) elected members of the Board of Directors; (*b*) one Fellow elected by each regional division of the Association; and (*c*) the representatives of affiliated organizations as provided in Article VIII of this constitution. Each Council member shall serve until his successor shall have taken office. The president shall be chairman of the Council; if the president shall be unable to serve as chairman at any session, the Council members in attendance shall elect a chairman for that session. Twenty (20) members of the Council shall constitute a quorum for the transaction of business.

Section 3. The Council shall meet during the annual meeting of the Association and at other times on the call of the president or upon the written request of twenty (20) members of the Council.

Article V

Section 1. The Board of Directors is the legal representative of the Association and as such shall have, hold, and administer all the property, funds, and affairs of the Association.

Section 2. The Board of Directors shall consist of eleven (11) members, the president-elect, the president, the retiring president, and eight (8) Fellows elected by the Council, two each year, for a term of four years. At any election of members of the Board of Directors not more than one Fellow serving his fourth consecutive year as an elected member may be re-elected. In the event of a vacancy in the office of an elected member of the Board of Directors, his successor for the remainder of the year shall be elected from among the Fellows by the Board of Directors and, for the remainder of the unexpired term, his successor shall be elected by the Council at the next annual election. Five (5) members of the Board of Directors shall constitute a quorum for the transaction of business. The retiring president of the Association shall be chairman of the Board of Directors. If he shall be unable to serve at any session of the Board, the Board members in attendance shall elect a chairman for that session. The administrative secretary and treasurer shall be ex officio members of the Board of Directors without vote.

Section 3. The Board of Directors shall hold four (4) meetings each year, one of which will be at the annual meeting. The Board of Directors shall also meet at the call of the chairman.

Section 4. The Board of Directors shall appoint such committees as may be necessary to aid in the management of the Association. The duties of standing committees shall be stated in the bylaws.

Section 5. The term of office of each of the eight (8) regularly elected members of the Board of Directors shall begin on January 15 following his election, and each shall serve until his successor shall have taken office.

Article VI

Section 1. The Association shall be organized in sections in accordance with the fields of interest of its members, as determined by the Council. Each member of the Association may designate the section in which he wishes to be enrolled and may designate an additional section in which he is interested.

Section 2. The vice president for a section shall be ex officio chairman of that section.

Section 3. The affairs of each section shall be managed by a section committee consisting of (*a*) the chairman and secretary of the section; (*b*) other members of the Council whose professional interests are in the field covered by the section or who represent societies affiliated with the section; and (*c*) four (4) Fellows, one elected each year by the section committee for a term of four (4) years. No person shall serve concurrently in more than one section committee. If an elected member of a section committee shall have resigned or died, his successor for the remainder of the unexpired term shall be elected from among the Fellows by the Board of Directors from nominations made by the section committee. One third of the members of the section committee shall constitute a quorum for the transaction of business.

Section 4. The section committee of each section shall promote the work of the Association in its own field and may organize subcommittees for that purpose. It shall arrange such section programs as it shall deem desirable for meetings of the Association, either separately or in cooperation with other sections of the Association or with independent societies. With the approval of the Board of Directors a section committee may arrange section meetings to be held at places and times other than those of Association meetings.

Article VII

Section 1. Regional divisions and local branches of the Association may be authorized by vote of the Council, for the purpose of promoting the work of the Association in their respective territories.

Section 2. Each regional division or local branch shall elect its officers for such terms as it shall prescribe and shall hold its meetings and conduct its affairs as it shall deem desirable, subject to the relevant provisions of this constitution and of the bylaws of the Association, and to such special provisions as the Board of Directors of the Association shall have established.

Article VIII

Section 1. To facilitate cooperation between the Association and other organizations, and among the latter, the Council may, on recommendation of the Board of Directors, elect an organization to be an official affiliate.

Section 2. Each organization thus designated an affiliate shall be entitled to name one Fellow of the Association to represent it on the Council; if it has more than 100 members who are Fellows of the Association, it shall be entitled to name an additional Fellow to represent it on the Council.

Section 3. On recommendation of the Board of Directors, the Council may elect an organization to be an official associate. Associated organizations shall have the same rights and privileges as affiliated organizations except for representation on the Council.

Article IX

Section 1. The Association shall hold an annual meeting at such time and place each year as the Board of Directors shall have determined. Other meetings of the Association or of its sections may be authorized by the Board of Directors.

Article X

Section 1. The publications of the Association shall be

issued in such manner as the Board of Directors may direct.

Article XI

Section 1. Funds of the Association shall be classified as Current Funds, Investment Funds, and Trust Funds.

(*a*) *Current Funds* shall include all dues of annual members, all receipts from publications, and all other funds received in the continuing operations of the Association.

(*b*) *Investment Funds* shall include all gifts and bequests received without special restriction concerning the use to be made of principal and income, and such other funds as may be designated by the Board of Directors as investment funds.

(*c*) *Trust Funds* shall consist of all life-membership contributions, all sustaining-membership contributions, all funds appropriated by the Board of Directors for establishing special life memberships, all gifts and bequests accepted with specific restrictions prohibiting their allotment to either Current Funds or Investment Funds, and such other funds as may be designated by the Board of Directors as Trust Funds.

Section 2. The deposit, investment, and disbursement of all funds shall be subject to the direction of the Board of Directors.

Article XII

Section 1. Amendments to this constitution shall be approved by the Board of Directors after publication in substance in SCIENCE and THE SCIENTIFIC MONTHLY at least one month prior to an annual meeting of the Association and ratified by a two-thirds vote of the Council members present in a Council session of that meeting. Ratified amendments shall be effective upon adoption and shall be published promptly in SCIENCE and THE SCIENTIFIC MONTHLY.

Bylaws

Article I

Section 1. The objects of the Association shall be accomplished by conducting meetings and conferences of those interested in various branches of science and education, producing and distributing publications, administering gifts and bequests as prescribed by the donors thereof, supporting research, making awards to recognize accomplishments in science, cooperating with other organizations in the advancement of science, and engaging in such other activities as shall have been authorized by the Board of Directors.

Article II

Section 1. Members who have paid dues for fifty years may be excused from further payments and still retain all the privileges of membership.

Section 2. Members may be elected by the Board of Directors to be Fellows of the Association and Fellows so elected shall remain Fellows only so long as they retain membership. If a Fellow discontinues his membership and subsequently rejoins the Association, he shall automatically again become a Fellow from the time of rejoining, without another election. Members are eligible to nomination for fellowship if they have contributed to the advancement of science either by the publication of original research or in other significant manner. Nominations for election to fellowship may be made by any three Fellows or by the administrative secretary or by the section committee in whose field the nominee's scientific work mainly lies.

Section 3. A Member may be dropped from membership for conduct which in any way tends to injure the Association or to affect adversely its reputation or which is contrary to, or destructive of, its objects. Charges of injurious conduct shall not be entertained against a Member unless the precise nature of the charges be submitted in writing to the president of the Association by not fewer than two Members. Upon receipt of such charges, the president shall refer them to the Executive Committee, which shall have the power to determine whether the charges shall be dropped, whether the accused shall be given an opportunity to resign, or whether the charges shall be referred to the Board of Directors for review and for final disposition. Whenever charges are referred to the Board of Directors, no person shall be dropped from membership except after opportunity to be heard and then only by a three-fourths vote of those members of the Board of Directors present and voting at a regular or special meeting.

Article III

Section 1. The administrative secretary shall serve as secretary to the Council and to the Board of Directors; he shall be in charge of the Association's offices and shall manage the affairs of the Association in accordance with procedures determined by the Board of Directors. He shall be an ex officio member of all standing committees.

Section 2. The treasurer shall perform the usual duties and those assigned in the bylaws.

Section 3. Reports of the administrative secretary and the treasurer shall be made in the manner prescribed by the Board of Directors.

Article IV

Section 1. The committees shall be standing as provided in the bylaws or special as the Board of Directors approves.

Section 2. During the interim between meetings of the Board of Directors, an Executive Committee consisting of the retiring president, the president, the president-elect, and such other directors or administrative officers as the Board of Directors may designate shall act on behalf of the Board of Directors. All actions taken by the Executive Committee shall be submitted for review and action at the next following meeting of the Board of Directors.

Section 3. The Investment and Finance Committee shall advise the Board of Directors regarding purchases and sales of securities for the Association, shall make recommendations to the Board of Directors on financial questions, and shall have the authority to buy or sell securities under such limitations as the Board of Directors may set. The Investment and Finance Committee shall consist of the treasurer, the administrative secretary, and five (5) members appointed by the Board of Directors. Each appointed member shall serve a term of five (5) years, the term of one member expiring on January 14 of each year. Each shall serve until his successor shall have taken office.

Section 4. The Committee on Affiliation and Association shall review applications for affiliation or association with the Association and make recommendations thereon to the Board of Directors. The committee shall consist of five (5) members appointed by the Board of Directors.

Each member shall serve a term of five (5) years, the term of one member to expire on January 14 of each year. Each shall serve until his successor shall have taken office.

Section 5. The Publications Committee shall give continuing scrutiny to the publications of the Association and the policies pertaining thereto and shall make recommendations thereon to the Board of Directors. The committee shall consist of five (5) men appointed by the Board of Directors. Each member shall serve a term of five (5) years, the term of one member to expire on January 14 of each year. Each shall serve until his successor shall have taken office.

Article V

Section 1. Council representatives of affiliated organizations which are not specifically related to an established section of the Association may be assigned to section committees in accordance with their requests.

Article VI

Section 1. Regional divisions authorized by the Council have full control of their meetings, of their affiliations with other scientific organizations, and of all activities to promote the advancement of science in their territory.

Section 2. The Pacific Division (organized in 1915) includes members of the Association resident in British Columbia, Washington, Oregon, California, Idaho, Nevada, Utah, and the Hawaiian Islands.

Section 3. The Southwestern Division (organized in 1920) includes members of the Association resident in Arizona, New Mexico, Colorado, Sonora, Chihuahua, and Texas west of the 100th meridian.

Section 4. The Alaska Division (organized in 1951) includes members of the Association resident in Alaska.

Section 5. Each division shall receive for its expenses an annual allowance not to exceed one dollar for each of its members in good standing and shall make an annual report to the Board of Directors covering its financial situation and other activities.

Article VII

Section 1. The names of affiliated and associated organizations shall be published from time to time as directed by the Board of Directors.

Section 2. Affiliated academies of science shall receive for research an annual allowance of fifty cents for each of their members who is also a member in good standing of the Association. The minimum annual allowance shall be fifty dollars. If any academy fails to utilize the research funds made available to it in any one year, these funds shall revert to the Association's treasury on December 31 of the second calendar year following the year in which the allowance was computed.

Article VIII

Section 1. The programs and arrangements for the Association meetings shall be under the general direction of the Board of Directors.

Article IX

Section 1. The publications of the Association shall be (*a*) SCIENCE, (*b*) THE SCIENTIFIC MONTHLY, (*c*) *Proceedings*, and (*d*) such other special publications as the Board of Directors may direct.

Section 2. The Association shall not be responsible for statements or opinions advanced in papers or in discussions at meetings of the Association or its sections, divisions, or branches, or printed in its publications.

Section 3. The Association reserves the right to copyright, at the discretion of the Board of Directors, any of its papers, discussions, reports, or publications.

Article X

Section 1. All funds shall be paid into the business office of the administrative secretary, where they shall be entered in the books of the Association, and deposited in a bank designated by the Board of Directors. The treasurer shall be the custodian of all Investment Funds, Trust Funds, and such other funds as may be placed in his charge by the Board of Directors. The administrative secretary shall be the custodian of the current funds.

Section 2. All bills against members and others shall be made and collected by the business office of the administrative secretary.

Section 3. All expenditures shall be made in accordance with the budget of appropriations as adopted by the Board of Directors.

Section 4. All payments shall be made by the business office upon competent certification as to their correctness and proper authorization.

Section 5. Checks against the accounts of the Association will bear two signatures, from a list of individuals determined by the Board of Directors.

Section 6. The securities of the Association may be bought, sold, or exchanged only upon the written order of two of the following: the chairman of the Investment and Finance Committee, the vice-chairman of the Investment and Finance Committee, the treasurer, and the administrative secretary.

Section 7. The business office of the administrative secretary shall keep proper accounts of all financial transactions of the Association.

Section 8. The accounts of the Association shall be audited and approved annually by a certified public accountant selected by the Board of Directors.

Section 9. The administrative secretary shall have the authority to enter into contracts for the Association, but contract authorizations must be within the budget authorizations made by the Board of Directors.

Section 10. The activities of the Gordon Research Conference shall be administered according to procedures established by the Board of Directors.

Article XI

The bylaws may be amended by majority vote of the Board of Directors, provided notification of the proposed amendment has been mailed to each member of the Board at least twenty (20) days prior to the meeting. Changes made in the bylaws by the Board of Directors shall be subject to approval, by majority vote, of the Council.

HOWARD A. MEYERHOFF

Administrative Secretary, AAAS

THE SCIENTIFIC MONTHLY

MARCH 1953

The Changing Concept in Microbiology*

SELMAN A. WAKSMAN

Dr. Waksman was born in Priluka, Russia, on July 2, 1888, but received his higher education in this country, of which he is a naturalized citizen. He has been at Rutgers University since 1918 and has been head of the Department of Microbiology since 1942. He has been the recipient of many honors: the Passano award (1947); Emil Christian Hansen award and medal of the Carlsberg Laboratorium (1947); Albert and Mary Lasker award (1948); Amory award (1948); John Scott award (1949). His distinguished career was climaxed by the award in 1952 of the Nobel prize in medicine and physiology, for his part in the discovery of streptomycin. In his honor, also, the French antibiotic manufacturers have established the Waksman Foundation, with Jacques Trefouel, director of the Institut Pasteur, as chairman. Each year the foundation sends a young French microbiologist to Rutgers for a year's training and supports a Rutgers student in France.

The Golden Age of Bacteriology

SHIBASABURO KITASATO, the one hundredth anniversary of whose birth was celebrated in December, entered the field of microbiology at a critical period, at a time when the foundations were being laid for the phenomenal development of this science. Only a very few years had elapsed since Louis Pasteur, the French chemist and bacteriologist, established the importance of microbes as causative agents of fermentation and disease, thus opening wide the door for the development of microbial physiology and industrial microbiology, on the one hand, and of medical bacteriology, on the other. This was soon followed by the work of the German bacteriologist Robert Koch, who isolated and identified the causative agent of tuberculosis, which came to be known as Koch's bacillus, and who introduced new techniques for the isolation and cultivation of bacteria from natural substrates, techniques which were to revolutionize the development of bacteriology.

Kitasato, a pupil and later a colleague of Koch, added greatly to the rapidly growing knowledge of bacteria as disease-producing agents. He succeeded, in 1889,[1, 2] in cultivating the tetanus bacillus, which he found to be an obligate anaerobe. His inability to find this organism in animals that had died from tetanus suggested that the disease was a result of intoxication rather than of bacterial multiplication. This was soon followed by his studies, in collabora-

* Based on an address delivered in Tokyo, December 20, 1952, in commemoration of the centenary of the birth of S. Kitasato.

tion with Behring in 1890,[3] on the tetanus antitoxin. His subsequent discovery of *Clostridium* (*Bacillus*) *chauvoei*, the causative agent of blackleg in cattle, led to the enlargement of the concept of anaerobic bacteria as causative agents of disease and strengthened Pasteur's ideas of anaerobiosis as a whole. His later work on the plague bacillus[4] aided in the development of methods of combating disease-producing organisms.

Kitasato's contribution to the control of tetanus, which did much to develop the new science of immunology, may be considered as by far his most important. Immunity to infection was shown to depend on the capacity of the blood, or rather the blood serum, to render innocuous the toxin produced by the infecting organism—in this case, the tetanus bacillus (*Clostridium tetani*). The neutralizing effect of the serum was so great that it could be transferred from one animal to the bodies of other animals. This formed the basis of serum therapy and of antitoxin therapy, the last term, incidentally, being used for the first time in that classical paper of Behring and Kitasato. The word "antitoxin" thus coined took its place in the language of therapy, along with "immunotherapy" and later "chemotherapy." It was Ehrlich and

Baron S. Kitasato

Hata, the latter a pupil of Kitasato, who later discovered the first important and widely used chemotherapeutic agent, salvarsan.[5]

Thus, although Pasteur may be considered as the pioneer in microbiology and Koch as the one who introduced careful techniques in the isolation and cultivation of microorganisms, Kitasato and Behring carried these ideas further into practical immunotherapy, and Ehrlich is usually considered as the father of chemotherapy. The golden age of bacteriology had now reached its midpoint.

I am not concerned here with the classical concepts of bacteriology begun by these pioneers and soon followed by numerous investigators in every civilized country in the world, with the result that many agents causing infectious diseases of man, animals, and plants were uncovered, and the principles of immunity and immunotherapy were firmly established. I should prefer to direct attention to another aspect of microbiology which has recently gained great prominence. I refer to the antagonistic relations among microbes, or the effects of one organism upon another in a mixed microbiological population, and the utilization, in the treatment of infectious diseases, of those substances, now known as antibiotics, which are responsible for the antagonistic phenomena.[6-10]

Pasteur was not particularly interested in pure cultures, which soon became recognized as the essential requirement for a proper understanding of the causation and treatment of infectious diseases. It was Robert Koch, with his theoretical "postulates," soon to be followed by Kitasato, Behring, and many others, who firmly established by new techniques the importance of pure cultures in determining the role of a particular organism in the causation of a given disease. The influence of these pioneers upon the subsequent developments in this new field of science, designated as bacteriology or microbiology, was so great that one other of Pasteur's classical observations, concerning the potential utilization of one organism for the control of a disease caused by another, remained largely unexploited for many years, either by his own students or by those of Koch and Kitasato. More than six decades were to elapse before this approach to microbiology came into its own.

According to the pure culture concept, it was necessary to isolate the causative agent of a disease in pure culture and produce the infection in experimental animals. This was followed by the recognition and, in many instances, isolation of certain toxins produced by the organism. Attempts were then made to develop specific antitoxins. This

approach led directly to developments in the new and highly important field of immunotherapy and later to an attempt at chemotherapy.

The second approach led to the recognition that in a mixed population one organism may exert a marked destructive effect upon another. This was followed by the isolation of specific chemical compounds which were produced by one organism and which had the capacity to inhibit the growth of other organisms. These compounds came to be known as antibiotics. This approach led to phenomenal developments in chemotherapy, which has often been called bacteriotherapy or antibiotic therapy.

Thus the pure culture concept in microbiology and the practical developments of disease control resulting from the work of Pasteur, Koch, and Kitasato were supplemented and even partly replaced, so far as the therapeutic aspects are concerned, by a new approach resulting from a better understanding of complex interrelationships among microorganisms. The practical utilization of these microbiological reactions has resulted in developments that have revolutionized medical practice and raised microbiology to a status never before dreamt of.

The Antibiotic Concept

The word "antibiotic," in the present accepted sense, was redefined in 1942 to embrace the products of microorganisms that have an inhibitive or destructive effect upon the growth of other microorganisms. Beginning in 1939, a number of new compounds that had antimicrobial properties were isolated in rapid succession from cultures of bacteria, fungi, and actinomycetes. The isolation of gramicidin and tyrocidine in 1939 was followed by the reisolation of penicillin in 1940–41, by the isolation of actinomycin and of streptomycin in 1940–43, and later of numerous new compounds. Before 1940, the actinomycetes were known to possess antimicrobial properties, but only two preparations, of questionable importance as antibiotics, were recognized. At present, nearly 100 compounds and preparations are known to be produced by this group of organisms. These products comprise some of our most valuable antibiotics, and new ones are being added to the list constantly.

These antibiotics possess certain peculiar properties which differentiate them sharply from ordinary antiseptics and disinfectants. They came to supplement the salvarsan of Ehrlich and Hata and the sulfa drugs of a quarter of a century later. One of the most important properties of these agents is their selective action upon different cells, be they cells of microscopic forms of life or cells of higher organisms. By utilizing this selective action, one can obtain antibiotics that are able, without injuring the tissues of a higher organism, to affect the pathogenic organisms invading such tissues. This makes possible their therapeutic use.

Ehrlich was the first to recognize that the efficacy of a chemotherapeutic agent depends upon the difference in its toxicity to the parasite and to the host. He was carried too far afield by his side-chain theory, and did not recognize clearly, largely because of a lack of sufficient information at that time, that this important difference in the action of a chemical compound is due to the variation in effect that it exerts upon the metabolism of the parasite as compared to that of the host cells. The compounds that he used were too "toxic," since they exerted their lethal effects upon the invading organism and upon the patient in about equal degrees. The finding of Woods and Fildes in 1940 that the sulfonamides exert their effect upon bacterial growth by interfering with the normal metabolism of the bacteria was a marked step forward in our understanding of the mechanisms of growth inhibition thus involved.

Many years elapsed between the discovery of the potentialities of antibiotics and their clinical application. Prior to 1939, numerous uncoordinated observations concerning the antagonistic effects of one organism upon others were to be found in the literature. Although these observations have frequently been catalogued, and the impression may thus be given that all of them have contributed directly or indirectly to the development of the science and application of antibiotics, the truth is that most such ancient observations can be properly interpreted only in the light of modern knowledge. In this respect many of them are on a par with folklore, which is pleasant to consider but which has hardly served to advance our present knowledge. Some of the observations are found in the works of men with keen scientific minds, who had too little interest to pursue the ideas further and were satisfied with speculations concerning potential significance. The observations of Roberts in 1874, of Tyndall in 1876, of Pasteur in 1877, and of the many investigators during the early part of this century, such as Frost in 1904, Vaudremer in 1912, and Gratia and Dath in 1924, remained uncoordinated, and the great practical potentialities of the phenomena involved went unrecognized.

Most of the observations of microbial antagonisms were limited to the bacteria, since these organisms were believed to be the major group of disease-producers and received the major attention of the medical bacteriologist. The contributions of Babes, Emmerich, Garré, Freudenreich, Doehle, and Kitasato appearing before 1890 are particularly important in this connection. Kitasato, for example, reported in 1889 that *Pseudomonas aeruginosa* (*Bacillus pyocyaneus*) inhibited the growth of cholera vibrio, which, in turn, had the capacity to inhibit the growth of a variety of other organisms, including *B. anthracis.* Subsequent studies, notably those on the aerobic spore-forming bacteria, which culminated in the work of Dubos in 1939, enlarged greatly upon this concept.

Students of mixed populations in soils and in water basins, as well as plant pathologists, also frequently observed the repression of one group of microorganisms by another. Their attention was directed primarily to the fungi in their relation to bacteria and to one another. These studies resulted in some rather striking observations, the complete elucidation of which had to wait for many decades. They culminated in the isolation of chemical compounds that were responsible for the growth-inhibiting effects. Thus, in 1896, Gosio isolated an antibacterial substance, designated as mycophenolic acid, from a species of *Penicillium.* In 1913, Alsberg and Black isolated penicillic acid from another species of *Penicillium.* Fleming, in 1929, isolated penicillin from still another species of this genus. Weindling isolated gliotoxin in 1932–34 from species of *Gliocladium* and *Trichoderma.*

The actinomycetes received the least attention, although in 1925 Gratia described mycolysate produced by a *Streptothrix,* and Russian investigators described actinomyces lysozyme. It was the bacterial products that received the greatest consideration. The pyocyaneus and fluorescens groups among the gram-negative bacteria gave pyocyanase, pyocyanin, and a host of other preparations. The spore-forming bacteria yielded sentocym and a variety of other products. Some of these were considered as lipids and others as proteins.

All these investigations remained uncoordinated and hardly pointed to a new broad field of science and application. A synthesis was required. This was brought about in 1939–40, when three series of investigations, one influenced to some extent by the other, but each dealing with specific groups of microorganisms, resulted in outlining clearly the new field of antibiotics. These made possible broad generalizations concerning a new concept in microbiology, which laid the basis for the wide developments in the field.

The microorganisms considered as potential producers of antimicrobial substances were the spore-forming bacteria, fungi, and actinomycetes. It is of particular interest to note, from a historical point of view, that these organisms remain even at this moment the most important producers of antibiotics, although not necessarily in the order given here.

1. *Spore-forming Bacteria.* Dubos established, in 1932, that the soil harbors certain spore-forming bacteria that are capable of producing enzymes that hydrolyze the capsular carbohydrate of the pneumococcus. This suggested a possible new approach to the treatment of pneumonia. He then proceeded with the study of the occurrence in the soil of organisms that had the capacity to produce chemical substances which would destroy bacteria as a whole and not merely attack specific chemical constituents of the bacteria. Having been trained in the field of soil microbiology, he utilized one of its classical methods, which consisted of enriching the soil with specific materials to facilitate the isolation of organisms that have a particular effect upon such materials. He utilized this method in the successful isolation of the organism which produced the capsular carbohydrate-splitting enzyme. He next enriched the soil with living bacteria. After numerous enrichments, which lasted two years, he succeeded, in 1939, in isolating from the soil a spore-forming bacterium (*B. brevis*) which produced a group of compounds (polypeptides in nature) that had a destructive effect upon gram-positive bacteria. He designated the first substance thus isolated as gramicidin and the group of polypeptides as tyrothricin. The latter was active against various bacteria not only in vitro but also in vivo. Although it was somewhat toxic and possessed certain other limitations, such as a narrow antibiotic spectrum, it exerted a marked therapeutic effect in experimental animals. Thus the basis was laid for a new approach to the isolation of a type of chemical compound which could be used in the treatment of infectious diseases.

2. *The Rediscovery of Penicillin.* After Fleming demonstrated in 1929 that certain preparations obtained from cultures of *Penicillium notatum* and designated as penicillin possessed antibacterial properties, several attempts were made to isolate the active substance in a purified state. Its great potential usefulness for the control of infectious diseases was not recognized until 1940, when Florey and Chain at Oxford, stimulated by their interest

in lysozyme, decided to investigate various molds for their ability to produce lysozymelike substances. They turned their attention first to the *Penicillium* studied by Fleming. Not only did they succeed in producing penicillin in the culture broth and in isolating a very active preparation, but they also established its very low toxicity and great effectiveness in the treatment of human and animal diseases. These results served to focus immediate attention upon the great therapeutic potentialities of microbial products, notably those of molds, and heralded another period of the new era in chemotherapy.

3. *Actinomycetes as Antibiotic-producing Organisms.* Although the bacteria have now yielded several antibiotics which have assumed an important place in human and animal therapy, and although the molds have contributed penicillin—probably the most valuable antibiotic so far discovered—it is the actinomycetes that have given to the medical profession the largest number of effective antibiotics. This group of organisms is still leading the field of research as potential producers of new antibiotics possessing antibacterial, antifungal, antiviral, and antitumor properties. It is of further interest to note here that although before 1939 much was known of bacterial products possessing antimicrobial properties, as pointed out previously, and although a number of active products produced by fungi were obtained as crude preparations and were isolated and crystallized, very little was known of the antibiotics of actinomycetes.

A systematic study of these organisms was begun in 1939, in the Department of Microbiology of Rutgers University. The first antibiotic isolated in 1940 was actinomycin. This was followed by streptothricin in 1942, and by streptomycin in 1943. These discoveries laid the foundation for still another phase of antibiotic research, the end of which is not yet in sight.

The Period of Accomplishment

What has been accomplished since 1940, or in this brief period of about twelve years? One could hardly be accused of undue optimism or of excessive generalization if one were to say: A great deal. A new field of science has been opened up. The pharmaceutical industry has undergone tremendous change: It is said that at least 50 per cent of all drugs sold over pharmacy counters, in the United States at least, consist of or contain antibiotics. Medical practice has been revolutionized: Many diseases that appeared to be beyond control only a decade or so ago have now become innocuous, and some have been virtually eliminated. The fear of great epidemics, such as those of plague and cholera, of typhoid and typhus fever, of pneumonia and a great variety of other infections, has either been eliminated or greatly reduced. Even tuberculosis, the Great White Plague of man, is now about to be brought under control. Diseases of childhood no longer breed the fear they once bred: The antibiotics have brought about their complete or virtual elimination. Who could dream of such potentialities a mere dozen years ago?

To be sure, other diseases have come to the front, especially virus diseases, neoplastic diseases, many fungus diseases, and the numerous physiological and psychological diseases associated with old age and with our present mode of life. But with the advent of the antibiotics, even these diseases no longer produce the dreadful spectra of fear which faced the human race only a few years ago, and there is hope that these as well will sooner or later be brought under control.

Penicillin is now used successfully in the treatment of syphilis and of diseases caused by gram-positive bacteria. Streptomycin is used in the control of many of the diseases caused by gram-positive bacteria that have become resistant to penicillin, as well as diseases caused by gram-negative bacteria, and of tuberculosis. Chloramphenicol, aureomycin, and terramycin are used effectively in the control of rickettsial diseases, typhoid, and a variety of other diseases that are either not sensitive to penicillin and to streptomycin or have become resistant to both these antibiotics. Tyrothricin, bacitracin, polymyxin, neomycin, and erythromycin have come to fill the gaps among diseases which the previously mentioned antibiotics do not affect or for which they cannot be used for various reasons.

There are numerous other fields of importance to human and animal health, or to human economy, in which the antibiotics have found or are finding important applications. Only a few need be mentioned here: the successful treatment with antibiotics of many animal diseases and of a few plant diseases; the use of antibiotics in the feeding of nonruminant animals; the use of antibiotics in the preservation of biological material, such as bull semen and virus preparations.

One may look with pride and wonderment at the great accomplishments in this very brief period.

What About Tuberculosis?

Not so long ago—even after the sulfa drugs had been introduced as chemotherapeutic agents and the potentialities of penicillin for combating a vari-

ety of infections had gradually been unwrapped before the eyes of the amazed world—the prevailing opinion was that no chemotherapeutic agent significantly effective in the treatment of tuberculosis would ever be discovered. Only about eight years have elapsed since a leading authority in the field said to me: "Perhaps all diseases of man may sooner or later become subject to therapy, through the use either of synthetic compounds or of antibiotics, but tuberculosis alone will resist all such efforts; it will remain with us, and its treatment will have to continue to depend upon bed rest, proper nutrition, and other old-time remedies." Meanwhile, streptomycin has revolutionized the medical approach to this disease. At first, this antibiotic was used alone; later it was supplemented with PAS. More recently, the introduction of isoniazid points to a complete change in the mode of treatment of various forms of tuberculosis. Thus, the introduction of antibiotics and synthetic compounds has placed in the hands of the medical profession powerful tools for combating tuberculosis.

I can do no better than cite from two recent reports. One from Great Britain[11] states:

> Whatever the ultimate picture may be, it is clear that chemotherapy has given us a big new advantage over the old adversary. Just as anaesthetics cleared the way for the surgeon by giving him time for careful and intricate work, so the new drugs have given chest physicians and surgeons time to consider the next move and develop new techniques. Their influence on the rate of decline in mortality is apparent, though the acceleration may not be maintained if the effect of the new drugs is merely to prolong life for a few years.

Another from the United States[12] emphasizes:

> Isoniazid and streptomycin both rank high as antimicrobial drugs in terms of such attributes as ability to interfere with vital activities of a parasite, distribution and maintenance of activity within mammalian hosts, and reasonable toleration on long-continued administration. Both substances compare very favorably with the antimicrobial drugs used in the treatment of other infections. The limitations on what may be accomplished by isoniazid and streptomycin in pulmonary tuberculosis arise not from any special lack of effectiveness on their part as drugs but from the nature of pulmonary tuberculosis with its destructive component.

What of the Future?

Antibiotics are so new that numerous problems still remain unsolved. Among the most puzzling is their mode of action. There is no doubt that the action varies with each type of antibiotic; otherwise all would act alike. Aside from different degrees of toxicity, which may depend upon differences in the chemical nature of the materials, one is justified in asking: Why is one antibiotic active against certain types of organisms and not against others? The most important substances chemotherapeutically, such as penicillin, streptomycin, aureomycin, chloramphenicol, terramycin, neomycin, bacitracin, and polymyxin, are active upon certain bacteria and actinomycetes, but not upon fungi. On the other hand, some of the other antibiotics, like fradicin, actidione, fungicidin, and candicidin, are active upon filamentous or yeastlike fungi, but not upon bacteria and actinomycetes. Still others, like actinomycin, clavacin, and streptothricin, are active upon bacteria and actinomycetes and also upon fungi, with great variations between the individual species and even strains. The antiviral agents, such as ehrlichin and abikomycin, are even more specific: they are active upon certain viruses, but not upon fungi and bacteria.

Why should an organism develop resistance to an antibiotic to which it was originally sensitive? Why should certain organisms become dependent for their growth upon a particular antibiotic? What is the relation between the chemical structure of the antibiotics and their mode of action? Fortunately, an organism that has developed resistance to one antibiotic still remains sensitive to others. This and the fact that more than one antibiotic is now available for the treatment of most types of infectious diseases render the above questions largely of theoretical rather than of immediate practical importance.

The great majority of infectious diseases caused by bacteria and actinomycetes are now subject to therapy by antibiotics and various synthetic compounds. The existence of a large number of antibiotics active upon fungi makes one feel fairly certain that these organisms likewise will soon come under control. Further, the protozoan and insect diseases appear to be well taken care of by various synthetic chemical agents, such as arsenicals, plasmoquine and paludrine, atabrine, DDT, and the like. The main problems that remain, aside from the purely physiological conditions, are concerned primarily with virus and neoplastic diseases. Here again one may feel hopeful, although it is difficult to foretell the exact time when these diseases can be attacked by antibiotics and other chemotherapeutic agents.

Conclusions

The two approaches to microbiology opened by Pasteur and continued so successfully by Koch, Kitasato, Behring, and others, on the one hand, and by Ehrlich, Domagk, Tréfouël, and finally by the discoverers of the antibiotics, on the other, have

greatly enriched human society. They have served, first, to provide an understanding of, then to eradicate, the infectious diseases of man and his domesticated plants and animals. Let us hope that they will finally serve to create a better, richer, and more healthful world in which to live.

References

1. Kitasato, S. *Z. Hyg. Infektionskrankh.*, **6**, 1 (1889).
2. *Ibid.*, 7, 225 (1889).
3. Behring, E., and Kitasato, S. *Deut. med. Wochschr.*, 16, 1113, 1145 (1890).
4. Kitasato, S. *Lancet*, 2, 428 (1894).
5. Brieger, L., Kitasato, S., and Wassermann, A. *Z. Hyg. Infektionskrankh.*, **12**, 137 (1892).
6. Waksman, S. A. *Microbial Antagonisms and Antibiotic Substances.* New York: Commonwealth Fund (1947).
7. Florey, H. W., *et al. Antibiotics: A Survey of Penicillin, Streptomycin, and other Antimicrobial Substances from Fungi, Actinomycetes, Bacteria, and Plants.* London: Oxford Univ. Press (1949).
8. Welch, H., and Lewis, C. N. *Antibiotic Therapy.* Washington, D. C.: Arundel Press (1951).
9. Baron, A. L. *Handbook of Antibiotics.* New York: Reinhold (1950).
10. Karel, L., and Roach, E. S. *A Dictionary of Antibiosis.* New York: Columbia Univ. Press (1951).
11. Simmonds, F. A. H., Laird, R., and Macdonald, N. *Lancet*, **263**, 347, 372 (1952).
12. Clark, C. M. *Am. Rev. Tuberc.*, **66**, 411 (1952).

HOW CAN YOU SAY?

How can you say,
You thinking ones
Who live upon a
Space-borne orb,
Whether beautiful
Long strands
Of seaweed
Grow upward
From land slid under sea,
Or hang
From blue-bayed rocky ledges
Downward
Toward that other blue
Of sky?

Is granite-terraced
Green-wooded
Shipstern Island
A pendant tourmaline
Hung on a chain
Of surf and spindrift?
And snow but
Vapor-stars
Rising to a waiting
Earthly paradise?

Rachael Graham

Harrington, Maine

Early American Geology*

GEORGE W. WHITE

Until he went back to his native Ohio as professor of geology at Ohio State in 1941, George White had spent most of his academic career at the University of New Hampshire, which may account for his interest in the early history of geology in the United States. He left his alma mater (Ph.D., 1933) in 1947, to become head of the staff of the Department of Geology, University of Illinois.

THE early English voyagers sailed to America with certain information about the new continent, its aspect, and its resources. At first the information was vague and partly erroneous, and some of it was fantastic. They modified, amended, and added to the slim store of recorded facts on topography and mineral resources as Thomas Hariot[1] did so significantly in 1588.

After the first period of exploration, when actual settlement had begun, information which included geological data, both exact and inexact, was compiled for those in England contemplating settling in the new land—as, for example, by John Smith in his several works from 1608 to 1631, and by William Wood in *New Englands Prospect* in 1634.

After the settlements were well established, men of taste, thought, and observation commented on natural phenomena—including geological features—quite as penetratingly as their English cousins were doing in the late seventeenth and eighteenth centuries. A surprising number of the colonists were members of the Royal Society; one was a charter member.[2] Some of the American geological observations and theories, such as that on isostasy by Lewis Evans in 1743,[3] were fully as advanced as English ones of the same period.

This long period of geological observation, first by English visitors, but soon by colonial Americans, has been strangely neglected by historians of science and by historians of geology. Goode[4] discusses at length early biological observations; Osborn[5] and Simpson[6] have noted early observations on vertebrate fossils. The historians of geology from the time of the early outlines of the development of geology in America by Van Rensselaer[7] and Mather[8] to Merrill[9, 10] all agree that the beginning of geology in America was early in the nineteenth century and that William Maclure was its father. Samuel L. Mitchill is mentioned for his work of the same period, but the first book devoted entirely to American geology, that by Schoepf in 1787[11] is given only the barest mention. These early workers of the "Maclurean epoch," important as they certainly are, did not invent American geology out of thin air, or discover it in a vacuum. For more than two hundred years before 1800 facts had been accumulating upon which, in some cases, respectable theories had been constructed. It is my purpose to sketch the expanding knowledge of geologic fact recorded in English concerning the American colonies, and to cite examples from each of the colonial centuries.

The Earliest Voyagers and their Information

We actually know very little about the observations of John Cabot in his voyages to Newfoundland in 1497, but he is reported to have observed copper in possession of the natives.[12] Hakluyt in 1582[13] gave an English translation of the report of Giovanni de Verrazano of his sailing along the North Atlantic coast in 1524, in which copper possessed by the natives was noticed. As the reports of these voyages did not contain information on rich minerals, it is of interest to note just where the epic English voyagers of the late sixteenth and early seventeenth centuries—Frobisher, Gilbert, Lane, Grenville, Brereton, and Smith—did get their information about natural conditions, metals, and other products and their high hope of rich mineral resources.

Richard Eden[14] in 1555 brought together and translated a miscellany of travels by Peter Martyr, Oviedo, and others, and part of Biringuccio's book on metallurgy of precious metals, under the title of *The Decades of the New World*. Eden obviously is presenting these earlier reports on the richness of the New World in such a way as to encourage English expeditions and the support of such undertakings by those who could invest "venture capital."

Peter Martyr, as translated by Eden, refers to gold on almost every page by pounds, by tens of pounds, and by hundreds of pounds. Oviedo in his

* Address of the retiring vice president and chairman, Section E, AAAS, at St. Louis, December 29, 1952.

"Generall [natural] historie of the West Indies," which in Eden's compilation follows Martyr's work, begins with a very lucid description of placer gold mining, and makes it clear that the gold has come from veins ("mother lodes") in the mountains at the headwaters of the streams. Among other minerals described is bitumen used for caulking ships in Cuba and in other islands (Trinidad?), in what may be the earliest note on hydrocarbons in the New World and is almost certainly the first in English.

Oviedo tells of one "grayne [nugget] of golde of great weight" weighing 32 pounds, another of 7, and another of 5 pounds. He comments on a large amount of gold in other places; his detailed description of the golden armor of the Indians and of the "principall women" who wore brassieres of golden bars weighing more than 200 ducats must have fired the imagination of the English would-be voyagers. Eden's book of 1555 and the revision by Willis in 1577 were sources of information and of exciting stimulation to the English. When we realize the quarts of pearls, pounds of gold, and quantities of golden armor and lingerie the English expected to find, we can visualize their great disappointment when they found the Indians of Virginia and New England not only with no golden raiment, but without any armor or brassieres at all.

The Frenchman Jacques LeMoyne, reporting on the Laudonniere expedition to Florida in 1564–65 tells of the (placer) gold mining in the "Apalatcy Mountains" and painted a picture of this mining activity, an engraving of which was printed in 1591 by DeBry.[15] This appears to be the first picture of any part of the United States having to do with anything geological.

The "Minerall Men"

The personnel of the English expeditions of the latter part of the sixteenth and early part of the seventeenth centuries included economic geologists and mining and metallurgical experts. Some were English (Cornish?) and others were Germans; they are referred to as "minerall men" or "refiners." We even know the names of some of them: Captain Vaughan with Grenville, Lane, and Hariot; the Saxon Daniel with Sir Humphrey Gilbert; and William Callicut, William Dawson, and Abraham Ransacke, plus two goldsmiths with Captain John Smith. Frobisher, who explored in 1576–78 north of the present United States, appears to have had more than one such expert. They were of various abilities, even as present-day "minerall men."

Recent critical editions and translations of the sixteenth-century works of the mining geologist and engineer Agricola,[16] the metallurgist Biringuccio,[17] the assayer Ercker,[18] and the assayers of the *Probierbüchlein*[19] make it easier for us to examine the kind of knowledge that could have been available to the mineral men and to those others who had interest in metals and their ores in the New World. It is certain that not all the voyagers were equipped with even a small part of this knowledge, but the professional "minerall men" should have had at least a part of it. (Note, however, the repeated complaint of Captain John Smith about the ineptness of his mineral experts.)

Early Observations in Virginia

THOMAS HARIOT (1560–1621). Thomas Hariot's *A briefe and true report on the new found land of Virginia* of 1588[1] is the earliest publication on natural history of what is now the United States.[4, 20, 21] Biologists have heard of his accurate and lively observations on plants and animals, but geologists have only recently known of his considerable and respectable geologic observations.[22]

Hariot accompanied the Raleigh expedition of 1585 to Virginia (North Carolina) as historian, scientist, and adviser. Upon his return to England in 1586 he prepared the report which was published in 1588 and which is now one of the rarest and most desirable of Americana. In his list of "Marchantable commodities" Hariot mentions several minerals, including alum, and notes the occurrence of iron and copper. His observations are confirmed in the report of the governor, Ralph Lane,[23] who tells of the concurring opinion of Captain Vaughan, "minerall man" of the party.

In his "Conclusion" Hariot gives the first description of the changing character of the Coastal Plain toward the Piedmont. We are impressed by his geological knowledge when we summarize what Hariot knew from personal observation and actual experiment about the geology of Virginia:

1. There was a wide flat coastal region (Coastal Plain) without prominent stones.
2. Along a distinct line (Fall Line) hard (crystalline) rocks appeared and continued an unknown distance into the country (the Piedmont).
3. The character of the crystalline rock varied.
4. The rocky ground at places contained iron, which he had seen and knew on professional advice to be ore.
5. Copper occurred someplace beyond the limit of exploration but supposedly not far away. The material was positively copper on the basis of its melting character.
6. The copper was silver-bearing, as determined by assay by Hariot and the "minerall man."
7. Excellent and plentiful clay for brick making existed in the Coastal Plain.

8. The fossil shells in the Coastal Plain sediments may have been recognized as remains of actual organisms. We know, from other sources, of Hariot's interest in fossils.[24]

JOHN BRERETON (fl. 1602). After the voyages supported by Sir Walter Raleigh in 1584–91 there was little exploration by the English along the Atlantic Coast until 1607, except for that of John Brereton, who passed along the New England Coast in 1602, and the same year published a little book on his "discoverie."[25] Brereton's observations on natural history consist mainly of those on minerals and topography of southern New England, Martha's Vineyard, and Nantucket. He was much impressed by what we today call industrial minerals —especially by building stone and clay.

CAPTAIN JOHN SMITH (1580–1631). Captain John Smith's claim to notice in any history of early American natural history[4] and in any history of early American geology rests upon the scientific and geographic observations recorded in his several books on Virginia and on New England which appeared between 1608 and 1631.

It is hard to realize that the Captain John Smith who was the actual and later the titular leader of the Jamestown colonists was in 1607 a man not yet twenty-seven years old.[26] All his incredible adventures as seaman, soldier, shipwrecked mariner, ambassador, Turkish slave, escapee, and traveler in Europe, Turkey, North Africa, and Ireland took place between his thirteenth and his twenty-fourth birthdays. When he sailed to Virginia with the settlers it was as one of those who had invested in the project and not as a hired member of the party.

From Smith's voluminous writings and his compilations of the writings of others, it appears that Smith himself was from the outset skeptical about finding precious metals or a route to the Indies. One gets the impression that he felt from the first that the future of Virginia lay in colonization and the development of trade. His attention to soil types confirms his interest in agricultural resources. Indeed, he is honest enough on one occasion to write that the hope of gold was used only to secure backing for his voyage to New England.

The principal source of information about the Virginia seen by Smith and his associates is the book[27] first published in 1612, *A Map of Virginia, with a description of the Countrey. . . .* The map, whether made by Smith or by members of his party under his direction[13, 28] is a remarkable production considering the conditions under which it was made. It is tolerably accurate geographically, shows the Indian territories and towns, and—a point I have not seen emphasized—clearly indicates which parts are based on actual exploration of the ground and which based on reports. The flat Coastal Plain is differentiated from the more hilly Piedmont. Falls are indicated where the various rivers cross the boundary from the harder rocks of the Piedmont to the softer ones of the Coastal Plain (the Fall Line).

Smith describes crystalline rock at "the head of the Bay" near Baltimore. His description of soil—probably the first one in English for America—clearly recognizes the variation of vegetation with soil types. After describing the rivers, their valleys, and their inhabitants, Smith passes on to a lengthy description of trees, fruits, plants, animals, fishes, and birds. He concludes this summary of the natural history of Virginia by a paragraph on "The Rocks":

> Concerning the entrails of the earth little can be saide for certainty. There wanted good Refiners: for these that tooke upon them to have skill this way, tooke up the washings from the mounetaines and some moskered [disintegrated] shining stones and spangles which the waters brought down; . . . The crust also of these rocks would easily perswade a man to beleeve there are other mines than yron and steele, if there were but meanes and men of experience that knew the mine from spar [i.e., knew ore from worthless gangue].

Smith's lack of certainty "Concerning the entrails of the earth" is refreshingly modern; we are still uncertain about them. Smith is again contemptuously sure that the "moskered shining stones" are not gold and is disdainful of the "refiners," of whom he had three.

Captain John Smith left Virginia for England in 1609, never to return to the colony that could not have survived the rigors of the winters of 1607 and 1608 without his guidance. After several years in England, during which he wrote his famous book on Virginia and defended himself from the charges of his enemies, he returned to British America, this time to a more northeasterly part of it. Smith briefly explored the New England shore in 1614, but his return in 1615 was prevented by storms and his capture by the French. Quite characteristically, during his captivity and upon his return he wrote a book: *A Description of New England . . .*,[29] in the first page of which he names New England for the first time. He contrasts the rocky coast of Maine with the sandy shores of Massachusetts, and generalizes on mineral resources.

The description of the geography and natural history of New England is not quite so smoothly organized as the earlier one of Virginia, but nevertheless is full of information. He apparently did not have an assayer ("minerall man" or "refiner") along

on this voyage to New England, but acted as his own assayer. Again the common attitude "there must be gold in them thar hills" is expressed by Smith, although we get the impression he himself did not really believe it. Always one to emphasize the more immediate and known resources, such as fish and furs, he knew at firsthand about iron ore and how important it could be to a new colony.

John Smith—soldier, sailor, explorer, administrator, colonizer, writer—was less credulous than most; he realized that in America the English had a world for settlement as well as for exploitation of any precious metals. In contrast to fertile soil and fundamental resources of iron and fuel to smelt it, he felt that precious metals were unimportant. He had the keen eye of the successful soldier for topography, but not the scholar's contemplative consideration of its continuity and origin, as had Hariot. He had the scientific spirit of true report of data, but left to others analysis and theory.

Observations in New England

William Wood (fl. 1629–39). William Wood, in his famous 1634 description,[30] *New Englands Prospect,* devotes Chapter 4 to "the nature of the Soyle," which he says "is for the generall a warme Kinde of earth," and after describing meadows and hay crops, tells of soil derived from clay, sand, and gravel.

Wood did not take "great notice" of "commodities as lie underground," and gives scant space to meager secondary material on minerals. The attitude toward precious metal had changed markedly in the twenty-five years since the settlement of Jamestown, when the settlers, fired by the tales of Martyr and Oviedo as translated by Eden, expected to find gold and silver at every bend in the rivers. Now, although he asserts that precious metals may yet be discovered in the "barren mountaines," Wood says "nobody dare confidently conclude" that they are present. The New Englanders had turned to more obvious resources—slate, building stone, and clay for bricks—which were to be of real use for their comfort.

Wood had considerable firsthand information of water supply and discusses in a fascinating manner the quality of water. His remarks on water-supply are the earliest in America so far known to me. He comments in detail on the ease of finding wells and asserts that the quality of the water is so superlative that it is almost as good as "good Beere" and that "any man will choose it before bad Beere, wheay, or Buttermilk."

Thomas Morton (d. 1646). Thomas Morton was a man who flaunted his impertinent actions before his Puritan neighbors and satirized them in his book *New English Canaan.*[31] His book, published in 1637, makes clear his high approval of the country and the Indians, but also—and not guardedly—his disapproval of the Puritan settlers.

Morton's contribution is more to our amusement than to our geological knowledge. Slight though his geological observations were, his book does show that some minerals were known, and that industrial minerals—lime, whetstones, and building stones—were coming into use.

He notes the excellent supply of ground water and tells of the ease of digging satisfactory wells. He overestimates the curative value of certain medicinal waters, but in so broad a way that there is little doubt he does so facetiously.

John Josselyn (fl. 1630–75). Over the next forty years about a dozen publications contain material on American natural history and include geological and topographic observations. Among these is *New Englands rarities discovered . . . ,* by John Josselyn,[32] in 1672. He describes the White Mountains and the topography of Mount Washington in considerable detail, especially the valleys cut into that mountain. He ascribes at least part of the cutting of these "Gullies" to running water, the first record I have found of such assumption in the New World.

Josselyn's chapter on "Stones, Minerals, Metals, and Earths" is disappointing in its brevity, merely listing several kinds of clay, metal ores, and gems (two of them mistakenly identified). The concluding paragraph of the mineral chapter on a strange cure for cancer is as out of context as it is startling.

John Clayton (fl. 1688–93). About twenty years later John Clayton in 1693 wrote in the *Philosophical Transactions* on several American natural history subjects.[33] He also was impressed with the excellent ground-water resources of Virginia. He says that the waters in the springs are "somewhat more eager" than those in England and require "some quantity more of malt to make strong beer."

Clayton's observations on both invertebrate and vertebrate fossils in Virginia and Maryland are the equal of those across the Atlantic and were quoted approvingly in England.[34] He describes the concentration of fossil oysters in certain strata, and their association with huge teeth (shark's teeth). His description of a fossil whale is noteworthy. Clayton says the shells may be remains of animals or may be figured stones, exactly the attitude of Ray, Lister, and Lhwyd, who at this time described English fos-

sils at length, but could not bring themselves to decide their origin.[35, 36]

First Half of the Eighteenth Century

ROBERT BEVERLEY (1673?–1722). Robert Beverley, the Virginia planter, was the first native American to write on Virginia.[37] In 1705 he published *History and present State of Virginia* in which he tells of the expedition of Captain Henry Batt to the Appalachian Mountains. There appears in this record a suggestion of the recognition of the parallel ridges of the folded Appalachians, the wide valleys between the ridges, and of the Appalachian Plateau, where a "Rivulet descended backwards."

In the chapter "Of the Earths, and Soil," Beverley shows a keen discrimination of soil varieties based on parent material—clay, sand, gravel, stones, or marl; and on topographic situation—lowland or upland. He is particularly specific in his references to the various shrubs and trees that grow on the different soils.

He is thorough in his listing of several varieties of clay and pigments, and mentions coal, slate, and building stone. He differentiates between Coastal Plain and Piedmont, and contrasts the region of "lower parts," flat, and free from stones (Coastal Plain) with the higher country where, especially near the "Falls of the Rivers," are found "vast quantities of Stone." He recognizes the continuity of the regular boundary between the Coastal Plain and Piedmont, and indicates that the Piedmont gives way farther west to mountains.

Beverley's reference to Colonel Byrd's exploration for the extension and reopening of the iron mine at Falling Creek by "boring, and searching after the richest Veins, near the Place of the former Work" is almost certainly the first record of exploratory drilling in America.

Beverley's sense of appreciation of amount and quality of water supply is repeatedly evident. His consciousness of rivers for navigation and springs for water supply and mills reflects his needs as a planter with large interests and operations. He also knows of "several Mineral Springs" but only briefly describes them, for he says he is "not Naturalist skilful enough, to describe them with the Exactness they deserve."

His account is a valuable one because it tells us what an educated man of 1705 knew of the topography and geology of Virginia. There is no speculation about causes or origins, but we are glad to have his literate and pleasant observations.

JOHN BARTRAM (1699–1777) and PETER KALM (1716–1779). At least 40 more books and papers on natural history were written in the next fifty years. The quality and amount of geologic observation vary, but the bulk is considerable. These must be passed over, for reasons of time and space, to discuss the observations and publications of three men who show great advance, not only in observation, but also in synthesizing data and proposing explanations. It is difficult to understand how their work has been neglected, for it provided a foundation—both acknowledged and unacknowledged—for those better-known men who followed.

These three are John Bartram, Philadelphia botanist and natural historian; Peter Kalm, Swedish traveler and scientist; and Lewis Evans, surveyor, cartographer, and geologic observer. Bartram's book on natural history of 1751[38] describes topography, rocks, fossils, and raised beaches near Lake Ontario. Of Kalm's several publications, the chief is the account of his travels first published in Swedish in 1753, in English in 1770, and available in a recent translation.[39] His more than 150 excellent geological observations and interesting speculations aggregate tens of pages and cannot be discussed adequately in a short space. It should be noted, however, that he speculates that Ireland and North America were once either united or else an island chain existed between them. He repeatedly mentions erratics and at one place till, but does not theorize on their origin.

LEWIS EVANS (1700–56). Lewis Evans' work[40] will serve as an example of the very respectable American geologic knowledge of the middle of the eighteenth century. Evans was born in Wales in 1700, but his entire professional career as surveyor, cartographer, and pamphleteer was spent in America.[41]

Evans traveled widely and observed acutely. In 1743 he made a trip from Philadelphia to Onondaga and Lake Ontario in the company of John Bartram and Conrad Weiser, interpreter and ambassador to the Indians. Evans carefully recorded his observations in a journal,[42] from which we note his certainty that fossil shells are remains of marine organisms, his recognition of the exhumation of the present Appalachian ridges from a former plain (peneplain!), the erosion of valleys to leave mountains, the linear character of the Appalachian ridges, the Coastal Plain, the former greater extent of the Great Lakes, and the isostatic uplift consequent on unloading the earth's crust by partial draining of the lakes.

Evans' maps[43] of Pennsylvania and adjacent states are very rare, but facsimiles are now available.[44] In these maps Evans filled in blank spaces

with notes on weather, roads, streams, and geology. His "Remarks on the Endless [Appalachian] Mountains, etc." are excellent geologic descriptions.

The 1755 Evans' *Map of the Middle British Colonies*[44] is so full of geographic data that, except for spot indication of "Coals" and "Freestone," all geologic notes are presented in a separate booklet of 36 pages: *An analysis of a general map of the Middle British Colonies.*[45] In the preface Evans regrets not being able to include on the map profiles and geologic sections, stating:

> But Want of Room in the Plate, has obliged me to leave out what would have very much assisted my Explanation of the Face of the Country, I mean a Section of it in several Directions; such would have exhibited the Rising and Falling of the Ground, and how elevated above the Surface of the Sea; what Parts are level, what rugged; where the Mountains rise and how far they spread. Nor is this all that a perpendicular Section might be made to represent; for, as on the upper Side, the Elevations, Depressions, outer Appearances and Names of Places may be laid down; on the lower, the Nature of the Soil, Substrata and particular Fossils may be exprest. It was with Regret I was obliged to omit it. But in some future Maps of Separate Colonies, I hope to be furnished with more Room.

We share Evans' regret for the omission, for it was more than fifty years before Maclure[46] presented geologic sections, and these did not show all the features Evans proposed to include had there not been "Want of Room in the Plate." Neither these sections, nor "Maps of the Separate Colonies," were published, for Evans died under tragic circumstances the following year.

In the *Analysis* is a clear statement of the physiographic and geologic provinces of the eastern United States.[47] It is the first attempt at delineating physiographic provinces of the whole country and is valid today. Although his description of New England is brief, for the country west of the Hudson he describes in very considerable detail what we now call the Coastal Plain, the Fall Line, the Piedmont Plateau, the Blue Ridge, the Folded Appalachians, the Allegheny Front, and the Allegheny Plateau.

Evans deserves an important place in the history of American geology because of his maps with their geological notes; his *Analysis* with geological descriptions; and his journal, with theories on Great Lakes drainage, isostasy, and stream origin. He is perhaps equally important because he furnished additional geological information to his contemporaries John Bartram, Kalm, and others.

We can trace Evans' geologic contributions for sixty years after his death. Kalm widely disseminated them. Books of travel display knowledge of them as, for example, that of J. F. D. Smyth.[48] The idea of deposition of sedimentary rock and fossils in inland bodies of water, often attributed to Volney[49] and to Mitchill,[50] who developed the concept in great detail, traces back to Evans.

It is unfortunate that Evans did not live to publish the planned maps of separate colonies with geologic cross sections. If he had done so his just place in the history of American geology would have been earlier recognized.

Last Half of the Eighteenth Century

It is necessary to pass over many other men of the eighteenth century who contributed geological observations, descriptions, and even hypotheses, for their consideration would require a lengthy paper in itself. We can only note that more or less extensive geologic references are to be found in the writings of John Winthrop (Hollis professor at Harvard), Benjamin Franklin and several of his circle, William Bartram, Peter Collinson, Jonathan Carver, Thomas Hutchins, Thomas Jefferson, Jeremy Belknap, Thomas Pownall, and many others. I pass over the first geological map of North America by Jean Ettienne Guettard in 1752[51] because it was made by a man who was never on this continent, but who compiled his map from information from French correspondents.

The extensive observations in 1784 of the very little-known J. F. D. Smyth[48] occur in a two-volume book of travels. In addition to describing the Appalachian and Cumberland mountains he pays much attention to waters, streams, and valleys, noting in detail natural levees in some of the valleys.

The two-volume travel book[52] of 1787 by the philosopher-soldier François Jean, Marquis de Chastellux (1734–88), second in command of the French forces in the Revolution, provides unexpectedly rich geologic references. His approach is genetic, and he seeks and proposes many clever explanations to account for geologic phenomena. He is not the first to notice erratics, but he is the first I have found who speculates on their origin; although he could produce no hypothesis that satisfied him, he was thoroughly aware of the problem.

The first book entirely devoted to the geology of the United States is by the Hessian physician Johann David Schoepf (1752–1800) in 1787.[11] Much of the material in it is included in a more general travel book published the next year.[53]

With the mention of Schoepf we make contact with already known history of American geology. In the first decade of the nineteenth century appeared the works of Maclure,[46] Mitchill, Cleave-

land,[54] and others with whose names we are at last familiar,[9] and my project of pointing out the hitherto unsuspected colonial interest in, and knowledge of, geologic phenomena comes to an end. Before 1800 the scores of publications by more than 40 different men contain important geological descriptions and observations, and include respectable hypotheses. A list of the first observations of various phenomena or of first proposal of concepts may provide a useful summary (Table 1).

TABLE 1

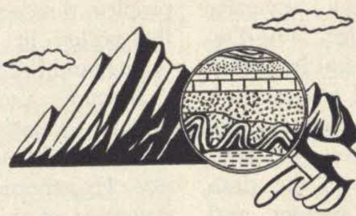
1524	First reference to copper—Verrazano
1588	First book on natural history and geology—Hariot
1591	First picture of mining (Appalachian region)—DeBry from LeMoyne
1612	First maps differentiating Piedmont from Coastal Plain—John Smith
1612	First description of soil types—John Smith
1634	First ground-water report—Wood
1672	First recognition of valley cutting by water—Josselyn
1705	First exploratory drilling—Byrd
1705	Physiographic description of Virginia and ground-water report—Beverley
1743	Great Lakes more extensive (shown by beach ridges) and isostatic rebound on their draining—Evans
1752	First geologic map of any part of the United States—Guettard
1753	Land bridge from North America to Europe—Kalm
1753	First mention of erratics—Kalm
1755	First division of America into physiographic provinces—Evans
1784	Description of natural levees—J. F. D. Smyth
1787	Speculation on origin of erratics—Chastellux
1787	First book on geology of United States—Schoepf

While recognizing the importance of the early nineteenth-century work of Maclure, Cleaveland, Mitchill, and their contemporaries, we must realize that these men really had a very considerable foundation of earlier observation and hypothesis upon which to build.

References

1. HARIOT, T. *A briefe and true report on the new found land of Virginia. . . .* London. (1588); (facsimile ed., Ann Arbor: [1931]).
2. STEARNS, R. P. *Notes and Records of the Royal Society of London,* 8, 178 (1951).
3. WHITE, G. W. *Science,* 114, 302 (1951).
4. GOODE, G. B. *Rept. U. S. Natl. Museum for 1897,* Pt. 2, 357 (1901).
5. OSBORN, H. F. *Cope: Master Naturalist.* Princeton, N. J.: Princeton Univ. Press (1931).
6. SIMPSON, G. G. *Proc. Am. Phil. Soc.,* 86, 130 (1942).
7. VAN RENSSELAER, J. *Lectures on geology. . . .* New York: Bliss and White (1825).
8. MATHER, W. W. *Elements of geology. . . .* Norwich: Wm. Lester (1833).
9. MERRILL, G. P. *Contributions to the History of American Geology.* Washington, D. C.: GPO (1906).
10. ———. *The First One Hundred Years of American Geology.* New Haven, Conn.: Yale Univ. Press (1924).
11. SCHOEPF, J. D. *Beiträge zur mineralogischen Kenntniss des östlichen Theils von Nord Amerika und seiner Gebürge.* Erlangen: J. J. Palm (1787).
12. RICKARD, T. A. *A History of American Mining.* New York: McGraw-Hill (1932).
13. FITE, E. D., and FREEMAN, A. *A Book of Old Maps Delineating American History. . . .* Cambridge, Mass.: Harvard Univ. Press (1926).
14. EDEN, R. *The Decades of the Newe Worlde . . . with particular description of the Moste ryche and large landes . . . by Peter Martyr of Angleria. . . .* London: Guilhelmi Powell (1555); (another ed., 1577; also ARBER, E., Ed. Birmingham: [1885]).
15. LORANT, S. *The New World.* New York: Duell, Sloan & Pearce (1946).
16. HOOVER, H. C., and HOOVER, L. H. *Georgius Agricola, De re metallica, translated from the first Latin edition of 1556. . . .* London: *Mining Magazine* (1912); New York: Dover (1950).
17. SMITH, C. S., and GNUDI, M. T. *The Pirotechnia of Vannoccio Biringuccio. . . .* New York: Am. Inst. Mining Met. Engrs. (1942).
18. SISCO, A. G., and SMITH, C. S. *Lazarus Ercker's Treatise on Ores and Assaying, translated from the German edition of 1580.* Chicago: Univ. Chicago Press (1951).
19. ———. *Bergwerk- und Probierbüchlein.* New York: Am. Inst. Mining Met. Engrs. (1949).
20. MEISEL, M. *A Bibliography of American Natural History,* 3 vols. Brooklyn: Premier (1924–29).
21. JAFFE, B. *Men of Science in America.* New York: Simon & Schuster (1946).
22. WHITE, G. W. *Trans. Illinois State Acad. Sci.,* 45, 116 (1953).
23. HAKLUYT, R. *The principal navigations. . . .* London: (1598–1600); Vol. 8, Glasgow: (1903).
24. PARKS, G. B. *Richard Hakluyt and the English Voyages.* New York: Am. Geogr. Soc. (1930).
25. BRERETON, J. *A briefe and true relation of the discoverie of the north part of Virginia [New England] . . . made this present yeere 1602. . . .* London: Geor. Bishop (1602).
26. ARBER, E. *Travels and Works of Captain John Smith,* 2 vols. Edinburgh: John Grant (1910).
27. SMITH, CAPTAIN J. *A Map of Virginia. With a description of the Countrey. . . .* Oxford: Joseph Barnes (1612).
28. FORD, W. C. *Geogr. Rev.,* 14, 433 (1924).
29. SMITH, CAPTAIN J. *A Description of New England. . . .* London: Humfrey Lownes (1616).
30. WOOD, W. *New Englands Prospect. A true, lively, and experimental description. . . .* London: Iohn Bellamie (1634); (also Boston: [1865]).
31. MORTON, T. *New English Canaan. . . .* Amsterdam: J. F. Stam (1637).
32. JOSSELYN, J. *New-Englands rarities discovered. . . .* London: G. Widdowes (1672).
33. CLAYTON, J. *Philos. Trans.,* 17, 941 (1693); 18, 121 (1694).
34. WHITEHURST, J. *An Inquiry into the original state and formation of the earth. . . .* London: J. Cooper (1778).
35. GUNTHER, R. T. *Life and Letters of Edward Lhwyd.* Oxford: Univ. Press (1945).
36. ZITTEL, K. A. v. *Geschichte der Geologie und Paläontologie.* München: R. Oldenbourg (1899).
37. BEVERLEY, R. In L. B. Wright (Ed.), *The History and Present State of Virginia.* Chapel Hill: Univ. North Carolina Press (1947); (1st ed. [1705]).
38. BARTRAM, J. *Observations on the inhabitants, climate, soil, rivers. . . .* London: J. Whiston and B. White (1751).
39. KALM, P. In A. B. Benson (Ed.), *Peter Kalm's Travels in North America. . . .* New York: Wilson-Erickson (1937).

40. White, G. W. *Trans. Illinois State Acad. Sci.* **44**, 152 (1951).
41. Gipson, L. H. *Lewis Evans.* Philadelphia: Historical Society of Penna. (1939).
42. Pownall, T. *Topographical description. . . .* London: J. Almon (1776).
43. Evans, L. *A map of Pensilvania, New-Jersey, New-York. . . .* Philadelphia: (1749, 1752).
44. ———. *A general map of the middle British colonies. . . .* Philadelphia: (1755).
45.———. *An analysis of a general map. . . .* Philadelphia: B. Franklin and D. Hall (1755).
46. Maclure, W. *Observations on the geology of the United States. . . .* Philadelphia: A. Small (1817).
47. Mather, K. F., and Mason, S. L. *A Source Book in Geology.* New York: McGraw-Hill (1939).
48. Smyth, J. F. D. *A tour in the United States of America,* 2 vols. London (1784).
49. Volney, C. F. *View of the climate and soil of the United States. . . .* London: J. Johnson (1804).
50. Mitchill, S. L. *Observations on the Geology of North America . . . ,* pp. 319–431 of Jameson's translation Cuvier, *Essay on the theory of earth.* New York: Kirk and Mercein (1818).
51. Ireland, H. A. *Bull. Geol. Soc. Am.,* **54**, 1227 (1943).
52. Chastellux, F. J. de. *Travels in North America . . . ,* 2 vols. London: Robinson (1787); (French ed., 1786).
53. Schoepf, J. D. *Reise durch einige der mittlern und sudlichen Vereinigten nordamerikanischen Staaten . . . ,* 2 vols. Erlangen: J. J. Palm (1788); (English trans., Philadelphia: [1911]).
54. Cleaveland, P. *An elementary treatise on mineralogy and geology.* Boston: Cummings and Hilliard (1816).



COELACANTH

Heavy with centuries, vastly unaware
Its future had been prisoned in its past;
Caught like the simple fishes, in a snare
Of Time, but uncondemned except to last;
Weary with waiting for it knew not what,
Its destiny entangled with its doom
So that it had to die to live—it cut
The ties that held it safe within the gloom.

It rose, but gave the rising little thought
And mused on other trivia instead,
Though least, on mere survival. Then it fought
And threshed about, was beaten, and was dead.

The ages are bereaved but must not weep
For, weeping, they would drown the world in tears
While we invoke formaldehyde to keep
What nature kept 300,000,000 years.

Milton Bracker

New York City

Caste Determination in the Social Hymenoptera*

STANLEY E. FLANDERS

Dr. Flanders, professor of biological control at the Citrus Experiment Station, University of California, Riverside, is a native Californian and took his Ph.D. at UC in 1935. He has explored for beneficial insects and has studied the interaction of host-parasite insect populations in the laboratory and the field. His article is based on an invited address given at the first meeting of the American Section of the International Union for the Study of Social Insects, Philadelphia, December 15, 1952.

TEN years ago, S. F. Light, of the University of California, in a paper entitled "The Determination of the Castes of Social Insects,"[1] reviewed and analyzed the recorded observations and experiments on caste determination in the termites and in the social Hymenoptera. He concluded that, except for the honeybee, the data obtained were inconclusive, being meager and statistical in nature. He pointed out, however, that such data as had been obtained during the period since 1928 tended to indicate that the causal factors were somatic rather than genetic. In the social Hymenoptera the demonstrated capacity of the queen under certain environmental conditions to repeat a production sequence of workers, males, and queens[2, 3] indicates that the determination of such castes is subject to environmental control.

It is well known that all honeybees developing from fertilized eggs possess the complete genic complex that would permit them to develop into either one of the female castes, and today it is generally assumed that this is likely to be the case in other social Hymenoptera, the structural, physiological, and behavioristic peculiarities of the worker caste being phenotypic deviations from the original female genotype. The general problem presented by the polymorphism of social insects is, therefore, as stated by Light, the nature of the mechanism which functions in each generation of each species to cause the offspring of the same parent to develop into different types of individuals in relatively constant numbers.

It appears from the studies of Haydak[4] and others that the factor initiating caste in the social Hymenoptera is the undernourishment or inanition of the individual during its development. The problem thus resolves itself into one of ascertaining the period in development at which inanition occurs, and the means by which it is accomplished.

Haydak's and Light's reviews of the problem of caste formation came to my attention at a time when, as a result of studies on ovisorption in parasitic Hymenoptera,[5] I had concluded that the regression of the ripe ovarian egg and its absorption into the blood stream are essential factors in the reproductive economy of all hymenopterous species in which ovigenesis is more or less continuous throughout the life of the female, and in which ovulation is externally induced.[6] It was inevitable, therefore, that ovisorption should be given consideration as a mechanism that would permit the determination of caste during embryonic development. Such a mechanism would provide a means of reconciling the divergent views of Emery and Forel[7] regarding caste determination in ants, since the undernourishment of the individual is as likely to occur in the egg stage as it is in the larval stage.

Limiting Factors in Caste Determination

In most social Hymenoptera the eggs develop into males if unfertilized, and the male sex is lacking in castes. This lack may or may not be genetic, but in certain nonsocial Hymenoptera (*Melittobia*) trophic polymorphism of both sexes does occur. It is quite possible, therefore, that the capacity to produce the male caste may be inherent in the social Hymenoptera but not realized, the developing male not being subjected to undernourishment. The fact that in many if not all Hymenoptera the ovarian eggs of the individual female apparently develop consecutively and gradually up to a certain size indicates that it is very unlikely that significant nutrient differences occur in such eggs prior to ripening.

* Paper No. 748, University of California Citrus Experiment Station, Riverside, Calif.

Except in certain bees, and possibly certain wasps, the castes apparently are not determined after egg deposition, since in no species is there any evidence that the workers which nurse and feed the developing larvae possess the inherent capacity to provide a certain amount or quality of food to particular female larvae. The production of workers and queens does not appear to be the result of discriminate biased feeding. This is evident with the ant *Oecophylla longinoda,* in which eggs deposited by the worker develop into queens, workers, and males, whereas those deposited by the queen develop only into workers and males.[8] It is significant that the differential feeding of the female larvae of the honeybee is entirely an effect of differences in the size of the cells occupied by such larvae. In the chalcid *Melittobia,* differential feeding is the result of the fact that the feeding of numerous larvae is limited to an individual host, the nutritional quality of which changes during such feeding. The absence in many ant species of a stereotyped structure or situation to ensure differential feeding is therefore strongly indicative of prelarval caste determination, a time of determination which receives support from the numerical dominance of workers in all nonparasitic ants, regardless of the wide variation in the larval nutrition of the various species.

In the social Hymenoptera it is obvious that caste control is closely correlated with sex control. It is known that the sex of the individual is environmentally determined. Sex determination usually occurs during the process of oviposition, the unfertilized egg developing into a male, the fertilized egg into a female, the utilization of the sperm being a function of the environment.[9] Since caste formation is limited to individuals from fertilized eggs, it is logical to assume that the situation that initiates the fertilization of the egg also initiates caste formation. This assumption seems to be verified by the existence of the needed mechanisms in many if not all the social Hymenoptera.

Environmental conditions that regulate the amount of nutriment extracted from the ripe ovarian egg by ovisorption also regulate the activities of the spermatheca, so that, as a rule, all the eggs that have been subjected to the ovisorptive process are fertilized, and most of those not so subjected are unfertilized.

The make-up of a typical colony (a relatively large number of workers, a small and more variable number of males, and one or several queens) is based on the reproductive responses of the queen to environmental conditions. However, in the honeybee, the low proportion of reproductives is an effect of the relatively few queen and male brood cells; in the ant, it may be an effect either of the deposition of a relatively low number of nutrient-complete eggs or of the destruction of a high proportion of such eggs by cannibalism, an exaggerated trophallactic reaction of the workers to the more highly nutrient egg.

The ovisorption process by its graduated effect on the eggs (prior to the point of injury) allows for the fullest expression of the polymorphic potentialities inherent in the germ plasm. This may result in the occurrence of caste intergrades in certain species of social Hymenoptera, whereas there are none in other species.

Ovisorption and its Effect on the Egg

Ovisorption or regression of the ovarian egg, according to Wigglesworth,[10] is probably the result of the interaction of nutrition, metabolism, and specific hormones. It characterizes hymenopterous species in which ovigenesis is more or less continuous throughout the life of the female, and in which ovulation is externally induced and immediately precedes oviposition. The processes of ovigenesis and ovisorption form a more or less continuous cycle.

In many Hymenoptera, ovisorption is the principal means of disposing of the ripe egg. This has been conclusively demonstrated in certain parasitic Hymenoptera in which oviposition is highly specialized.[6] Such species include those of the family Encyrtidae, whose eggs are equipped with an aeroscopic plate that is never completely absorbed and that remains in the ovariole as a readily recognizable remnant of the egg. The honeybee, likewise, may not deposit many of the eggs that she produces.

The extent to which the regression of the egg is completed varies with the species. In some, no visible remnant of the egg remains in the ovariole; in others, the entire content of the egg is extracted, but the eggshell remains intact and is never ovulated. With the honeybee and the ant the eggshell is completely absorbed.

During the process of regression in the honeybee the contents of the undeposited egg separate into a major portion consisting of a white, coarsely granular substance, and a minor portion consisting of a yellow, finely granular substance restricted to the posterior end of the egg. This yellow material remains in the ovariole as a small yellow disc, so that the yellow coloration of the ovaries increases with the number of eggs absorbed and, consequently, with the age of the female.

The ovisorptive process permits the deposition

of viable, partially regressed eggs. Such eggs may or may not be noticeably smaller than the unregressed eggs.

The complete regression of the ovarian egg is apparently accomplished in about the same length of time as is its genesis. In the chalcid *Metaphycus helvolus* (Comp.), the development of a primary oogonium into a ripe egg and the deposition of that egg require only 3 days at 80° F. At this temperature, the regression of the egg, assuming that this begins at the time that the egg becomes ripe, is completed in less than 2 days.[11]

Whiting[12] has found that the ripe ovarian egg may be retained in the ovary of the chalcid *Microbracon* for 36 hours without injury. Nonviable eggs in the process of regression, but with the chorion intact and with fluid content, can be ovulated and deposited. A female of *Metaphycus luteolus* (Timb.), after being withheld from her host for 3 weeks at about 80° F., deposited viable as well as nonviable eggs, the latter being partially collapsed; the completely collapsed eggs were retained in the ovary.[5]

If ovisorption begins as soon as the egg becomes ripe, the amount of material extracted from the egg will vary with the length of the preovulation period and the rate at which ovisorption occurs. The length of the preovulation period in species characterized by ovisorption is determined by the rate of oviposition, which, in turn, is determined by the environmental conditions that influence the oviposition responses of the female. Similarly, the rate of ovisorption is determined by the physiological state of the female, which, in turn, is determined by such environmental conditions as relative humidity, etc.

Differences in rates of ovisorption may be responsible for the fact that the ovaries of the gravid female of *M. helvolus* contain more ripe eggs when she is ovipositing continuously than when she has been prevented from ovipositing for a day or two. It is possible, however, that this difference is an effect of a reduction in the rate of ovigenesis, since the female of *M. helvolus*, when kept from her host, is deprived of nitrogenous food.

Whiting has shown that, with *Microbracon*, limited embryonic development occurs in about 10 per cent of the eggs deposited by out-crossed females, and in about 50 per cent of the eggs from close-crossed females.[13] The percentage of eggs that may have been injured by the absorption process was considerably greater, however, since the deposited egg "shells" that dried up quickly were not included in the hatchability ratios. Presumably, the inhibition of embryonic development is the effect of the lack of sufficient nourishment. Since the nutritional needs of the developing male are less than those of the female, a greater mortality of the female is to be expected. In this connection it should be noted that both Salt and Rempel[4] observed that an upset of nutritional balance may initiate reactions that produce sex reversal and intersexes.

The Ovisorptive Process as a Factor Affecting the Developing Individual

It has been demonstrated experimentally that in the Hymenoptera the undernourishment (inanition) of the developing individual is the primary cause of behavioral and morphological differences in the adult. In the honeybee, undernourishment is brought about by progressive-type feeding of the larvae;[4] in the gregariously developing wasp *Melittobia*, by a change in the nutritional qualities of the host;[14] and in the prepupal queen ant, by the parasitic extraction of assimilated food.[15]

Ovisorption appears to be the undernourishing mechanism in the bee *Melipona*. In this bee there is a distinct worker caste, although the brood cells are the same in size and are mass-provisioned with the same kind of food.[16] Kerr[17] found that differences in either the quantity or the quality of food ingested during the larval stages had no effect on differentiation of the castes. The worker population is constant, but the male population varies with the age of the colony. The virgin queens are killed by the workers if the mother queen is present. Kerr[18] suggested that the ratio of queen-producing eggs to worker-producing eggs is a possible effect of ovisorption.

In the army ant, *Eciton*, whose colonies multiply by division, the queen produces brood after brood consisting entirely of workers. Occasional broods consisting of a large number of males preceded by a few queens appear only during the dry season. Schneirla[3] states that a reasonable hypothesis to account for the occurrence of the sexual brood would be a reflex-physiological change in the gravid queen as a result of her first impact with dry-season conditions. It is probable that this change consists of a marked reduction, either in the rate of ovisorption—this being a possible effect of reduced oxygen supply resulting from the closing of the spiracles to prevent desiccation—or in the time between the ripening of the egg and its ovulation—this being an effect of an increased responsiveness to oviposition stimuli. The egg under such conditions would suffer little if any regression, and consequently would develop into either a queen or a male. The high population of males that follows

the appearance of the few queens in the occasional bisexual brood indicates a reduced responsiveness of the spermathecal gland to oviposition stimuli, so great that the hydrostatic pressure needed to accomplish fertilization[9] is not maintained after the deposition of the first few eggs.

This hypothesis of a reflex-physiological change in the gravid queen resulting from the abrupt exposure to dry conditions is equally applicable to aculeate colonies that are established by single queens. The queen has a high capacity for ovigenesis, but the number of eggs she lays and the rate of egg laying are dependent on the size of the worker population, the amount of food utilized, and the space occupied. In the seasonal growth of such a colony the general sequence of events is that the first brood consists of small workers and the succeeding broods of larger workers. After the largest workers have appeared, the queen and males are produced.

Falconer Smith informs me (*in litt.*) that he observed that mature colonies of *Camponotus* found in small, isolated logs may contain minors, medias, and majors, as well as winged males, whereas at the same time of year in much larger colonies of the same species living in larger logs all members of the worker caste may be represented but no males.

In the growth of a claustral-type colony most if not all the eggs deposited early are fertilized; consequently, it is probable that colony conditions that regulate the interval between the ripening of the egg and its ovulation also regulate the activity of the spermathecal gland, conditions causing a short preovulation period after ripening also causing a decrease in the responsiveness of the gland. If the eggs produced early are deposited more slowly than those produced later, the rate of deposition of the former would expose them to the process of absorption and at the same time ensure the fertilization of each.

This type of colony formation is well illustrated in the pollen-feeding bee *Halictus malachurus,* as observed by Stockhert.[19] During the spring the overwintering female lays eggs that develop into worker females morphologically distinct from the mother. As the season progresses the overwintering female deposits eggs that develop into short-lived males and overwintering females. The males mate only with the overwintering females. The old overwintering female lives until the end of the season, still with ripe eggs in the ovary and with an abundance of sperm in the spermatheca. The peculiar character of a sequence of events of this type is illustrated by the experiments of Goetsch[1] with the ant *Pheidole pallidula.* He showed that eggs of young queens transferred to old colonies still gave rise to nanitic workers. The determination of such workers he considered to be a matter of quantity of food available during their embryonic development.

It may be suggested that the extraction of nutriment from the deposited eggs by the workers caring for them may result in undernourishment and thus determine the caste. If such were the case, it is not likely that the castes in all species of social Hymenoptera would be limited to the female sex, since it is improbable that the worker can distinguish between fertilized and unfertilized eggs. It would appear, also, that the eggs of parasitic species should develop into workers, males, and queens in about the same proportion as with the eggs of the host species, the eggs of both species being exposed to the same environmental influence. Significantly, the only species of ants lacking workers are certain parasitic species. The parasitic species that occasionally produce workers are characterized by individuals that are intermediate, as to some characters, between the queen and the typical workers.[16]

Undernourishment of the embryo may or may not be indicated when a female deposits eggs that differ noticeably in size. The structure and the physiology of the hymenopterous ovariole evidently preclude the deposition of eggs before they attain the normal maximum in size. The assumption that a small egg is an effect of precocious deposition is unwarranted, since it is known that the ovarian egg can become smaller *after* it obtains its maximum size.

It is well known that in an ant colony the workers may deposit eggs that vary considerably in shape and volume. This variation is probably an effect of different workers producing eggs of different size. Weyer[20] observed worker eggs that were larger than those produced by the queen. According to Autuori,[21] the queen of the fungus-feeding ant *Atta sexdens,* during the early life of her colony and before any fungi are present, deposits two kinds of eggs, a small egg that develops into a worker and a large egg ten times the volume of the small egg. The presence of fungi and a correlated high humidity may result in a reduction in size, through absorption, affecting all the eggs produced by the queen. According to the ovisorption hypothesis the biparental queen-producing egg of a given species is always constant in size.

Differences in egg size may not result in corresponding differences in the size of the embryo. According to Rosenberg,[22] the eggs of the ichneu-

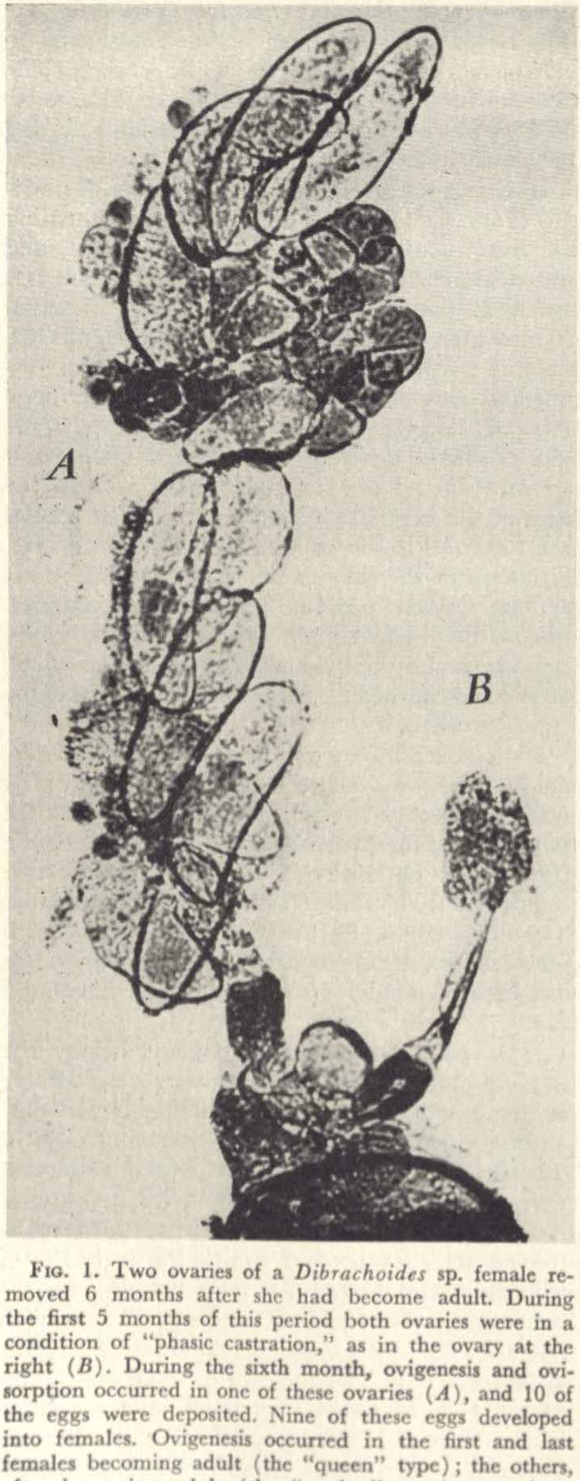


FIG. 1. Two ovaries of a *Dibrachoides* sp. female removed 6 months after she had become adult. During the first 5 months of this period both ovaries were in a condition of "phasic castration," as in the ovary at the right (*B*). During the sixth month, ovigenesis and ovisorption occurred in one of these ovaries (*A*), and 10 of the eggs were deposited. Nine of these eggs developed into females. Ovigenesis occurred in the first and last females becoming adult (the "queen" type); the others, after becoming adult (the "worker" type), were in a condition of "phasic castration."

mon *Ephialtes extensor* also differ in size, the length of those deposited late in the life of the female being about one half that of eggs deposited early in life. Rosenberg's data indicate that larvae, when newly emerged from the long and short eggs, are themselves quite uniform in size. Aside from the shrinkage of the eggshell, the occurrence of ovisorption is indicated by the fact that certain females commonly deposited nonviable "collapsed" eggs. Rosenberg points out that "eggs which are extremely short may hatch; but some evidence was obtained indicating a lower viability in these eggs."

Caste development in the wasps and bumblebees may represent a primitive stage in the evolutionary development of the stingless bees, the honeybee, and the ant. The difference between the workers and the queens of wasps is small, and intermediate forms are numerous. According to Wheeler,[19] quantitative feeding during development probably accounts for the production of these intermediate forms, but it should be noted that quantitative feeding, if undernourishment is not involved, will result only in a difference in size, not in structure or in behavior. Experiments by Falconer Smith,[23] with the incompletely polymorphic ant *Camponotus pennsylvanicus*, show that adult stature can be altered by conditions which exist between the time that the imaginal size is determined and the time of pupation. The basic imaginal size appeared to be determined at an early stage in the life history of this ant. The effect of larval overfeeding is illustrated in *Eciton*, the sexual brood exhibiting a marked increase in individual size over that of the worker brood.[3]

Eighteen years ago, while experimenting with the propagation of the alfalfa weevil parasites *Peridesmia, Spintherus,* and *Dibrachoides,* I noted two types of adult females, gravid and nongravid, that might well be representative of the prototypes of the female castes in the wasps. As in the wasp, the gravid and nongravid states were temporary. A female of *Dibrachoides,* for example, that had been in a nongravid condition for five months at room temperature became gravid in one ovary (Fig. 1) and deposited ten eggs during the following month. The diet of this female consisted of honey and the body fluids of the host. Nine females were reared from her eggs. Only the first and the last, upon becoming adult, were gravid; the others were nongravid or in a condition of diapause, or phasic castration.[24] There was no noticeable difference in the size of these adults, so that undernourishment during the larval stages was improbable. Simmonds[25, 26] has observed the occurrence of a larval diapause in the chalcid *Spalangia drosophilae* to be a maternal effect: the older the female at the time of oviposition, the greater the proportion of her progeny that entered a diapause.

He also noted that this proportion decreased at higher temperatures. It was evident that the incidence of diapause was influenced by "prenatal" factors acting on the ovarian egg. We may assume, therefore, that the condition of phasic castration, or diapause, which characterizes the adult workers in certain social wasps also is initiated in the egg. The worker, which closely resembles the queen, readily becomes gravid when egg laying by the queen is temporarily suppressed.[27]

The temporary cessation of ovigenesis in many Hymenoptera is correlated with the phenomenon of ovisorption. The black scale parasite *Metaphycus helvolus* is an excellent example of a species having the capacity to stop ovigenesis. The stimuli causing the cessation of egg production in *M. helvolus* are not purely psychological, as appears to be the case in *Peridesmia*;[24] this response is derived in part from the lack of protein food. In both species the cessation of ovigenesis and the complete regression of all ovarian eggs at room temperature is not immediate but requires a period of 3 weeks.

The periodic cessation of ovigenesis in the *Eciton* queen, according to Schneirla,[28] is an effect of periodic underfeeding, which occurs when the nutritional needs of her developing larval brood are high. Ovisorption in this ant is undoubtedly an essential and regular physiological process.

Ovipositional Factors Effecting Ovisorption

Since, in the social Hymenoptera, ovisorption is either limited or precluded by ovulation, and since ovulation and oviposition in such insects are almost simultaneous responses to the same environmental stimuli, it is necessary, for an understanding of the role of ovisorption in caste determination, to recognize the factors that determine the responsiveness of the female to oviposition stimuli—factors that may cause delayed oviposition. Such factors include lack of supplemental stimulation, a low quality of stimulation, a low relative humidity, aging of the female (as in *Ephialtes extensor, Microbracon hebetor,* and *Spalangia drosophilae*), low frequency of oviposition, preferential oviposition (as in *Pimpla examinator*), the amount of foraging and nursing activities (as in the wasps and ants), spatial requirements (as in *Peridesmia*), presence or absence of sperm in the spermatheca (as in *Melittobia*), and possibly differences in source of sperm, as from related or unrelated males (as may be the case in the honeybee).

The psychological effect on the female of the presence of sperm in the spermatheca is revealed in a number of parasitic species. In certain braconids that oviposit as readily before mating as after, as in *Macrocentrus ancylivorus* Roh., the presence of sperm in the spermatheca causes a psychological reaction that inhibits further mating. In this species, impregnation of the female is accomplished by means of a spermatophore. Mating is not inhibited until the spermatophore is connected with the spermathecal duct. In certain chalcids that oviposit as readily before mating as after, as in *Coccophagous cowperi* Gir., the female without sperm in the spermatheca deposits her eggs only externally on the larval body of a hymenopterous parasite inhabiting the mummy of a mealybug or scale insect; this same female when impregnated deposits her eggs only in the body fluids of a living scale insect.[29]

The influence of the condition of the spermatheca on the psychology of the female is well illustrated in the chalcid *Coccophagus ochraceus* Howard, parasitic on black scale. The progeny of the mated female, unlike that of *C. cowperi,* consists of both sexes. When the spermatheca contains sperms, the condition of the spermathecal gland determines whether the female stands on top of its host and oviposits in the blood or stands beside the scale and places an egg underneath it. When the rate of oviposition is high, the gland becomes nonfunctional (probably because it is temporarily depleted), and a male egg is placed under the host. When the gland again becomes functional, a female egg is placed in the body cavity of the same host.[29]

Since the condition of the spermatheca markedly affects the behavior of the hymenopterous female, it would not be surprising if her responsiveness to oviposition stimuli were determined by very slight differences in the spermatic fluids of different stocks of the same species. Experiments by Schmieder and Whiting[30] indicate that the fecundity of the female, when mated with a closely related male, is decreased in the case of the parasite *Microbracon* and increased in the case of the parasite *Melittobia,* in comparison with the fecundity of a female mated with an unrelated male.

When a female contains sperm from a male with which she would not normally mate, her responsiveness to oviposition stimuli may be lessened and the amount of ovisorption correspondingly increased. In this connection, observations by Mackensen[31] on the oviposition responses of queen bees that are artificially inseminated are significant. In the honeybee, as in other social Hymenoptera, the unmated, gravid female does not oviposit readily. The queen bee usually begins to oviposit from 3 to 4 days after mating. According to Mackensen, however, artificial insemination has very little if

any effect on initiating oviposition, the impregnated queens laying at the same age as virgin queens. This indicates that spermatic material, when so transferred, loses its capacity to stimulate oviposition. The probable nature of this loss is revealed by Mackensen's experiments showing that, when carbon dioxide gas is properly applied to artificially inseminated queens and to virgin queens as well, oviposition begins about 20 days earlier than is the case with untreated queens.

It is evident that the oviposition response of a hymenopterous female in which ovisorption is precluded only by oviposition is highly specialized and finely adjusted.

On the basis of the evidence presented herein, it is considered that polymorphism in the Hymenoptera is largely if not entirely limited to species in which ovulation is externally induced; that in such species ovisorption is precluded by oviposition; and that in some if not most social species caste determination is an effect of undernourishment of the embryo brought about by the extraction of nutriment from the ovarian eggs. The occurrence of such undernourishment would confirm Wheeler's[19] observation that in social insects starvation is exquisitely regulated and exploited.

Polymorphism in the Hymenoptera is not genetically limited to one sex, yet in the social species all castes are female, the male not being subjected to conditions that cause polymorphic differentiation—that is, the male is never undernourished during development.

In the social Hymenoptera whose nests lack structures for forcing the undernourishment of a portion of the larval brood, the determination or control of the worker caste appears to be a function of the environment acting through the queen.

The ovisorptive process appears to be an adequate mechanism for the determination of caste in social Hymenoptera, fulfilling all the requirements for such a purpose. It appears to be the only explanation for the deposition, by a single female, of eggs that differ either in volume or in content. It is the principal means by which species highly restricted in the deposition of their eggs can dispose of such eggs and become nongravid.

Partial ovisorption may result in undernourishment of the embryo, a condition that apparently determines its course of development, the end result being the occurrence of the worker caste, the gradations of caste as exhibited by the weakly differentiated castes of the wasps and bumblebees and the highly developed castes of the ants being genetically determined.

The factors that may regulate the amount of nutriment extracted from the ripe ovarian egg are environmental, the relative humidity affecting the rate of ovisorption, and the oviposition responses of the female regulating the amount of exposure of the egg to the ovisorption process.

The gravid and nongravid types of female in pteromalid parasites of the alfalfa weevil, and the poorly developed worker castes in the wasps and bumblebees, may be the equivalents of the evolutionary prototypes of the castes in the ant.

References

1. Light, S. F. *Quart. Rev. Biol.*, **17**, (4), 312 (1942).
2. Haskins, C. P., and Haskins, E. F. *Ann. Entomol. Soc. Amer.*, **43**, (4), 463 (1950).
3. Schneirla, T. C. *Zoologica*, **33**, 89 (1948).
4. Haydak, M. H. *J. Econ. Entomol.*, **36**, (5), 778 (1943).
5. Flanders, S. E. *Ann. Entomol. Soc. Amer.*, **35**, 251 (1942).
6. ———. *Can. Entomologist*, **82**, 134 (1950).
7. Wheeler, W. M. *Mosiacs and other Anomalies among Ants*. Cambridge, Mass.: Harvard Univ. Press (1937).
8. Ledoux, A. *Ann. sci. nat., Zool.*, **12**, 313 (1950).
9. Flanders, S. E. *Sci. Monthly*, **71**, 237 (1950).
10. Wigglesworth, V. B. *The Principles of Insect Physiology*. New York: Dutton (1940).
11. Flanders, S. E. *J. Econ. Entomol.*, **35**, (5), 690 (1942).
12. Whiting, A. R. *Am. Naturalist*, **89**, 193 (1945).
13. *Ibid.*, **74**, 468 (1940).
14. Schmieder, R. G. *Biol. Bull.*, **65**, 338 (1933).
15. Vandel, A. *Bull. biol. France et Belg.*, **64**, 457 (1930).
16. Wheeler, W. M. *The Social Insects*. New York: Harcourt, Brace (1928).
17. Kerr, W. E. *Anais escola super. agr. "Luiz de Querioz,"* Univ. São Paulo, **3**, 299 (1946).
18. ———. *Genetics*, **35**, 143 (1950).
19. Wheeler, W. M. *Colony-founding among Ants*. Cambridge, Mass.: Harvard Univ. Press (1933).
20. Weyer, F. *Z. wiss. Zoöl.*, **Abt. a, 131**, 345 (1928).
21. Autuori, M. *Rev. entomol.*, **11**, 215 (1940).
22. Rosenberg, H. T. *Bull. Entomol. Research*, **35**, 201 (1934).
23. Smith, F. *Ann. Entomol. Soc. Amer.*, **37**, 401 (1944).
24. Flanders, S. E. *Ibid.*, **28**, 438 (1935).
25. Simmonds, F. J. *Bull. Entomol. Research*, **37**, 95 (1946).
26. ———. *Trans. Roy Soc. London*, **B 233**, 385 (1948).
27. Marchal, P. *Compt. rend. soc. biol.*, **49**, 556 (1896).
28. Schneirla, T. C. *Anales inst. biol.* (Univ. nacl. Méx.), **20**, 1 (1949).
29. Flanders, S. E. *Quart. Rev. Biol.*, **21**, (2), 135 (1946).
30. Schmieder, R. G., and Whiting, P. W. *Genetics*, **32**, 29 (1947).
31. Mackensen, O. *J. Econ. Entomol.*, **40**, (3), 344 (1947).

Professional Education and the Disciplines:

An Open Letter to Professor Bestor*

WILLIAM CLARK TROW

Dr. Trow (Ph.D., Columbia, 1924) is professor of educational psychology at the University of Michigan. A revised edition of his Educational Psychology *and one of the "Junior Citizen Series," of which he is coauthor, have recently been published. He is also coauthor of* Human Values in Elementary Education, *and editor of and contributor to* Educational Psychology in Teacher Education. *During the war he directed the University Civil Affairs Training School and later was a member of the first United States Education Mission to Japan. He is associate editor of the* Journal of Educational Psychology *and president of the Educational Psychology Division of the American Psychological Association.*

I HAVE just finished reading your article in the SCIENTIFIC MONTHLY attacking various aspects of the American educational system. It led me to wish you had read "Bury the Hatchet," by James B. Conant, in the National Education Association *Journal* for May 1951. I hope you will read it, for it is in the spirit of that article that this reply is written. The point Dr. Conant makes is that continued feuding between professors of education and professors of the traditional academic subjects is not an effective method of improving our schools.

A better procedure might be to try to find some areas of agreement and, as we move out from these, check on our facts and note some of their implications. It may then be possible to work out plans that would have some influence of a beneficial sort.

Areas of Agreement

The first of such areas of possible agreement would, I think, be our common concern for public education, our recognition that it is far from perfect, and that improvements can and should be made. This is fundamental, for there are those in our country who are attacking the public school as an institution and seeking to destroy it. This hostile view is as old as tax-supported education itself, but I assume that it is not held by you or by most of the critics of the schools in our colleges and universities. If this point is not clarified, American scholars are likely to find themselves unwittingly included with the ignorant and the malicious.

We can agree, too, I believe, that there are many teachers in our schools who are not qualified to teach. I would suggest that the numbers of teachers required, the conditions of their work, the salaries paid, and the attractiveness of other kinds of employment are probably more important reasons for the unfortunate situation than the one you emphasize—i.e., the multiplicity of education courses they are required to take.

Closely connected with this is your statement with which I, for one, would agree, that the professors in academic fields are at least partly to blame for some of the weaknesses of our public school program (catastrophe seems a little too strong a word). I believe their failure is not so much that they have allowed various things to go on, as that they have tended to lose contact with the schools. One consequence of this is that academic course offerings (not all by any means) are unrealistic, in that the teachers do not see how the courses they are required to take will help them to do the things they have to do.

And I am sure you would agree that some education professors are not as competent as one could wish. This is true, of course, of other professors, and of the members of other professions. It is difficult to judge the competence of men in other fields, and we might be willing to agree to leave the mat-

* Arthur E. Bestor, Jr., is professor of history at the University of Illinois and author of an article in the August 1952 SCIENTIFIC MONTHLY entitled "Aimlessness in Education."

ter to their colleagues, or at least confine our indictments to individual cases where the facts are available, and avoid too broad generalization.

Finally, I wonder if we can agree to dispense with name-calling. I regret as much as you the language used by Willard B. Spalding, although in your footnote which quoted him you said nothing of his provocation. And your reference to "medicine men" does nothing to strengthen your case. Let's have our say without sticking out our tongues at each other.

Consider the Facts

Now let us consider some facts. The trouble with such a complex subject as public education is that there are so many facts that must enter into any judgment on specific issues. But three or four will suffice for the present purpose.

There is actually no single body of knowledge and there are no disciplines that "throughout history . . . have been rightly considered fundamental to education." This is a fact, and since it is contrary to what is perhaps your basic contention, it is an important one to consider. Early education in China emphasized memorizing the Chinese literary classics, and the present rulers of China require "brain-washing" and Marxian dogma. The ancient Persians taught their young men to ride, to shoot, and to speak the truth, whereas modern Persians emphasize, among other things, memorizing the Koran in Arabic. The classical Greek curriculum consisted primarily of music and physical education. During the Middle Ages the trivium and quadrivium constituted the *pièce de résistance,* a program which few would advocate for American schools today. And so it goes. Each generation in each culture is called upon to select from the past and present what seems best and most important for the future, and naturally enough there is much honest disagreement as to what is best and most important. Even Jefferson, whose ideas were most enlightened for his day, advocated a selectivity that is quite different from the kind to which we subscribe, when in his Virginia plan he wrote: "By this means twenty of the best geniuses will be raked from the rubbish annually. . . ." Different cultures have developed different curricula, and modifications are constantly being made.

The second fact is that "genuine education" is not necessarily "intellectual training." There is ambiguity in the subject, and there is further ambiguity in the word "training," which to many is the antithesis of education. And even if one accepted your proposition that genuine education is intellectual training, there would be legitimate differences of opinion as to the process. To educators, it means the automatic transfer of training which Reeder picturesquely caricatured in the quotation you cited. True, "the traditional curriculum offered a clear-cut answer," but what evidence have we that it was the correct answer under all circumstances, even though its commendable aim was "to cultivate sound judgment based upon critical thinking and thorough knowledge"?

The third fact comes as a partial explanation for the incompleteness of the intellectual training idea, and it is this. The human brain is not separate from the rest of the organism. Teachers, and even college professors, have been a long time in realizing the significance of this truism. In spite of the academic concern for intellectual training, children persisted in bringing their bodies to school, and with them their interests and attitudes, their likes and dislikes, their ambitions, and their frustrations. Granted that the intellectual values are the ones the schools should emphasize, they are not developed in vacuo. There is something in the phrase "the whole man" suggesting that the implications would perhaps be better realized if critics of the educational program would actually spend some time in the schools and get acquainted with Joe and Oskar and Minnie and Julie. The educators feel no disrespect toward the disciplines as such; rather, they are concerned about what the pupils really learn and how they learn. Familiarity with the inside of a school building would make it impossible for anyone to state quite so glibly that "the intellectual power that mankind has accumulated throughout its entire history can be passed on to successive generations." It can and it can't. It is not on a tray to be passed along. It must somehow be re-experienced in the neuromuscular systems of flesh-and-blood youngsters.

And this leads to the fact of human variation: the children and young people in school differ widely in ability. Only about a quarter of the young people in high school go to college or are capable of profiting from the traditional college curriculum. The interest of those who speak for the intellectual disciplines seems to be with this group, and they could undoubtedly be of help in working out schemes to see that capable young people go to college, and in developing adequate programs for them, just so long as they do not insist that *all* young people in school follow these programs, and so long as the departments do not act merely as antagonistic pressure groups each for its own discipline!

Carrying the variation fact a step farther, we know by definition that half the people in the world are below average in intelligence. How shall children in the lower range be educated? And even if we grant that the disciplines are important at the college and graduate level, do they provide the best form of education at the elementary and high school levels? As for the pupils in the lower 50 per cent, most of them are capable of doing little more than memorize some of the terms and definitions. Theirs is a concrete world in which the disciplines are not separated, as in college departments. And it is conceivably the responsibility of the school to introduce them to this concrete world of gas stations and farms, of consumer buying and of housing, yes, of public health and sanitation. This is not "anti-intellectualism," and the schools are not being "wrecked" by it. (Though I would not be one to condemn a little hyperbole on occasion!) Instead, it is a serious attempt to help young people meet the kinds of problems they are likely to meet, just as work in the "disciplines" is a similar serious attempt to help other young people meet the kinds of problems that are likely to confront them.

Interpreting Results

So much for some of the facts and their implications. Now as to your critical comments based on your homework. I would certainly show myself to be hopelessly prejudiced if I came to the support of the questionnaire, at least as you described it. But before stringing up the Superintendent of Public Instruction by his thumbs, I should want to be sure that in the interpretation of the results the items were given equal weight, as you imply. It sounds to me as if the basic subjects were bunched together as commonly accepted material, but that information was sought about some of the more irregular items which somebody had been pressing for. In any case, isn't it a rather dubious procedure to select two studies (certainly the 1936 Michigan study you reported had very little influence) and imply that they are typical? The matter of sampling bothered Fuller, too. The selection of data to fit one's thesis, with the neglect or suppression of contrary data, is a practice I am sure neither of you would follow in your own disciplines. Why do it when you step out of your field? If your contention is that some educational writings are pretty poor, I will regretfully have to agree with you; but, frankly, the generalization from one or two samples not only throws suspicion on your argument, but also on the value of the intellectual training you advocate, which aims "to cultivate sound judgment based upon critical thinking and thorough knowledge." The practice may be one of the reasons that the educational process has moved along without as much benefit from the academicians as we might wish.

Finally—and here I run still closer to the danger of seeming to continue "feuding' "—I come to another point which makes us suspicious of specialists operating outside their field of specialization. I am sure that as a historian you would be very careful when you assign a cause to any event. In partisan oratory, it is of course the opposite party that is responsible. But the historical method decrees greater caution. Yet in the field of public education you do not hesitate to write: "There is no mystery about the source . . ." and then launch into a series of statements that give the case away with a fantastic and almost paranoid array of accusations, and with the facts selected and warped to fit the delusional system.

Let's calm down and look at the situation rationally. Why should all these educational bureaucrats gang up on the college professors? The "source" doesn't lie in any conspiracy. It lies in the culture. The superintendents, principals, and teachers are in contact with the people in the cities, towns, and villages of our country. And they are the ones who determine through school boards and legislatures what kind of education their children shall have. The professors of education are the ones who have studied the situation day in and day out, who have thumbed through the studies, hundreds of them, good and bad, and whose responsibility it is to *help,* not to stand aloof and criticize (pardon me, but that's the way it looks). And the "federal officials and bureaucrats," who have no legal power, call meetings and conferences, get out publications (of unequal value to be sure, but so are other publications), and put in long hours trying to help the teachers with their task of directing the development of the millions of children crowding our schools.

From this point of view, which is at least a reasonable one, the statements made in your last two pages frankly seem off the beam. Just read them over with such questions in mind as the following: Is this really so? Does prejudice enter in here? Do these facts really prove my contentions? Take one example—the departments of Education and Chemistry in the University of Illinois compared on the basis of size and estimated competence. The number of staff members is usually largely determined by the number of students. How many students, graduate and undergraduate, are enrolled

in the two departments? Or this: "Teachers are all but compelled to take that work not in the subjects they are teaching but in endless courses in education." Passing over the "endless" (which suggests prejudice)—of the teachers I have advised, some prefer to take an M.S. degree in the academic subject, but in that case they must take all their work in that subject. Many prefer a degree in education, which *requires* that one third be taken outside education, and *permits* one half.

Cooperation and Consecration Needed

Can you and your academic colleagues really help to improve the education of our young people? I think you can. And I suggest a few possible ways:

1. Appraise and modify your course offerings to be sure that the dead material is removed and the newer findings are included, that beginning courses in a department are not based on the false assumption that all who take them will concentrate in that department, and that some courses, at least, include content that teachers can use in their work.
2. Appraise and review methods of instruction, in part on the basis of student evaluation, and avoid the assumption that young people are being educated by being failed for their inability to regurgitate a term's work on a single examination at the end of a course.
3. Attend and speak at state and national meetings of teachers and school administrators, where your views can be freely discussed.
4. Arrange for lectures, symposia, and even workshops cosponsored by your department and the college or school or department of education.
5. Set up combined-degree graduate programs which provide opportunities for students to take work both in education and in an academic department.

All these plans are in operation in various institutions. You can perhaps think of other ways that include a cooperative effort to work out solutions for present-day educational problems, most of which will be found to be much more complex than they seem from a distance.

Such cooperation takes time, in conferences and in committee meetings, and, yes, it takes consecration. If you are really as serious as your article suggests, you may be willing to work toward some of the needed improvements. But if you come into a group or a committee meeting with all the answers dreamed up to hand out as if to unworthy menials who have banded together against you, you might as well stay home and read a good book. Those who have the responsibility for carrying on the work of the schools just will not be interested. People are like that. I can't help it. They just are.

If you and your academic colleagues are not willing to help in such ways as I have suggested, it should be realized that uninformed criticism is at best unbecoming and at its worst positively detrimental to the cause of good education. I believe that most education people would be glad to meet you more than halfway in any serious attempt to improve our educational program.

SEA-STUFF

I live in an electric sea
That flashes in and over me.
Electric is the solid ground—
Its particles like bubbles bound;
Electric the transparent air—
Rivers of fire flow everywhere,
Too vast, too luminous for sight.
Heaven and earth are only light,
The momentary shape of motion,
A dazzling, dancing, living ocean.
Heaven and earth are not enough—
I am myself this strange sea-stuff.

JAMES DILLET FREEMAN

Lee's Summit, Missouri

The Tutelo Harvest Rites: A Musical and Choreographic Analysis

GERTRUDE P. KURATH

The author is a pioneer in methods of ethnic choreography and musicology. An M.A. in the history of art and archaeology at Bryn Mawr (1928) intervened between periods of study in the dance, instrumental and theoretical music, and theatre techniques. Overlapping careers as a concert performer, teacher, and stage producer preceded her research in folk drama and American Indian ritual. Her field trips have extended from Mexican Otomí and Yaqui to the Woodland Sauk and Fox, Cherokee, and repeatedly to the Iroquois. Currently she is assisting in the development of a record collection and transcription service at the University of Michigan Depository of Regional Music.

DURING the past fifty years ethnographers have often displayed an awareness of music and the dance as cultural components, especially as factors in religious activity. Occasionally they have consulted a specialist, but as a rule they have contented themselves with a brief comment. A superficial commentary on these artistic components can be of little use to science; a thoroughgoing analysis may provide a valuable contribution. Scientific methods and research personnel have been lagging, however, notably in choreography, despite the potentialities of the material.

The paradigm chosen for exposition here is the Four Nights Dance, the Tutelo harvest rite, termed *geiniwašondáge* in the Onondaga tongue.* The Tutelo were a Siouan tribe, removed from the southeastern Piedmont to Ontario and now amalgamated with the Cayuga and Onondaga of Six Nations Reserve, Ontario. Their rituals survive in the Iroquois celebrations at the longhouses. The harvest rite challenges analysis because of its formal variety and its unsolved problems. The interdependence of music and choreography demand joint treatment, and their historical background demands consultation of all clues to the culture of the Tutelo. This process of analysis and synthesis, though prolonged, may be reduced to a summary of essentials.

The process of "laboratory" analysis is subject to control, but it could not exist without the observations and recordings that combine training with a good measure of old-fashioned luck. As it happened, I attended the Onondaga Green Corn festival on August 16, 1949, and was able to witness the Four Nights Dance because the necessary singers and dance leaders were present and the crowd was in a suitable mood. I was able to participate because it is a women's dance. Three years later the song recordings materialized because of the good will of the Michigan Academy of Science and of seventy-two-year-old Peter Buck, a Tutelo-Cayuga, and also because the dry weather permitted hitching the tape recorder to the car battery. During September and October, 1952, the choreographic notes were checked three times with Anne Greene, an amiable Cayuga-Onondaga matron and one of the leading dancers. She and Richard Buck, Peter's cousin, separately served as interpreters of the text paraphrase provided by Susan Buck Claus. This aged invalid, the only living person with any memory of the Tutelo language, spent many patient hours on the partial recognition of the song meanings.

During the examination of the collected materials, the transcription of songs, the study of the dance movements, we must bear in mind the first impression of mixed Iroquoian and exotic qualities. We ask, "What can the forms reveal about the tribal identity and the original culture?"

* Acknowledgment is due to the Michigan Academy of Science, hence the American Association for the Advancement of Science, for support of the field work; to William N. Fenton and Leslie A. White for their recommendation; to Joffre L. Coe for helpful comments on the manuscript; and to John W. Gillespie for the loan of a battery converter and of his field notes on Eastern Cherokee dances and on the Shawnee Bread Dance.

The Ritual

Four Nights Dance expresses thanks for a good crop of corn and other foods. As such it ordinarily forms the last part of Onondaga and Cayuga harvest ceremonies late in the fall, just before the concluding feast. It has shrunk from four nights' to an hour's duration. In the center of the spacious longhouse floor two benches are set up, and six male singers and several small boys take their places face to face. The song leader manipulates a small Iroquois water drum; the assistants shake horn rattles of recent Iroquoian make. Each song, which is started by the leader, is performed twice. At specific times women sing the repeat, and women are the dancers. Three costumed leaders start circling counterclockwise and, as usual among the Iroquois, in the course of time, other women and girls line up single file. On the observed occasion, the numbers swelled to thirty participants. Between songs the dancers saunter, but very briefly, for one song follows another immediately—no mean feat.

Even without pauses the ritual stylistically divides into eight parts. Its recording includes 40 songs, but it is not complete, for several songs were omitted in the second and last parts because of domestic incidents in Peter Buck's home. Otherwise this material is reliable and permits of safe deductions. The geometry of the dance steps is, on the other hand, approximate, because of individual variation and because of the taboo on photography.

One song from each section has been selected for reproduction. In every case the percussion accompaniment is written below the melody and the choreographic script above; in two instances the ground plan is incorporated. In the script the symbols are confined to the feet and knees so as not to confuse the reader. These symbols have been described in several publications—for example, in an article on the Iroquois Death Feast.[1] The method of scale weighting according to note frequency has also been explained before.[2] Some brand-new devices are introduced, however.

I. *Introductory Songs* by men, 1–6 (Fig. 1).
   1. Invocation to percussion tremolo.
   2–6. Songs to alternately accented duple beat.
   Character: short, syncopated themes in descending sequence—that is, repetition varied by level or contour.

II. *Tutelo Step* by men and women, 7–17 (Fig. 2).
   Music: 11 songs (should be 12), similar in character to Part I.
   Dance: Women's dance step—right foot diagonally right forward, left heel brushes ground next to right instep; reverse.

Fig. 1. The fourth introductory song is performed by the seated male singers and is not associated with any dancing.

   Torso nearly erect, turns slightly toward heel during brush.
   Single file, forward progression.

III. *Corn Mime* by men and women, 18–21 (Fig. 3).
   Music: 4 songs with same melody but varying words, to even slow beat.
   Character: Long sustained, balanced phrase repeated thrice in sequence.
   Dance: Tutelo step and progression as in Part II.
   Gesture different for each song.
   18. Husking corn: left hand in front of waist, closed, palm toward body; right hand moves obliquely down 12 inches during step and up during brush, to join left hand.
   19. Pounding corn: clasped hands close together in front of waist, move vertically down during step and up during brush.

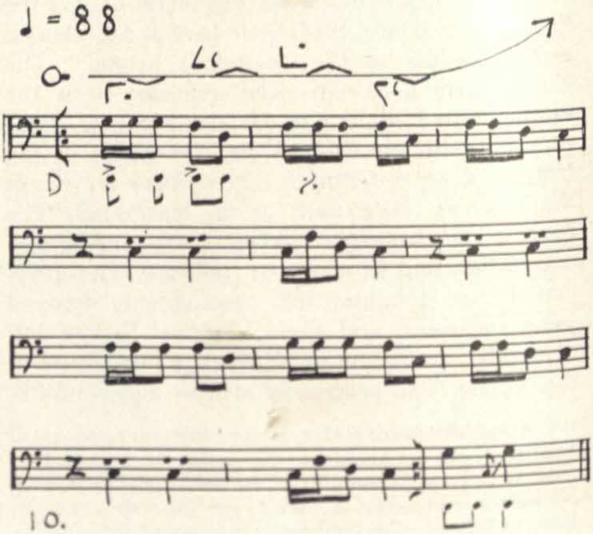


Fig. 2. Songs 10 and 11 are typical of the second part of the ritual, where men and women sing and women dance.

FIG. 3. The third part of the ritual consists of four songs, 18–21, all with the same melody, and repeated by the women after the men's first rendering. At the same time, the women mime actions of the corn harvest.

20. Winnowing corn: clasped hands close together in front of waist, twist alternately right and left in rhythm.
21. Making cornbread: left hand held with flat palm upward, right palm rubs left in flat clockwise circles.

During small, precise gestures the body lilts gently, with rocking foot motion.

IV. *Side Shuffle,* with songs by men only, 22–25 (Fig. 4).

Music: 4 archaic songs, to tremolo and accented duple beat.

Character: somewhat irregular, medium length phrases in dwindling, level sequence.

Dance: Women face toward singers in center, slide right foot right, then slide left foot to join right.

Torso absolutely erect, no gesture, arms hanging relaxed.

Single file progression to right, with central focus.

V. *Enskänye Shuffle,* songs by men, 25–29 (Fig. 5).

Music: 4 songs with same melody but varying words, to tremolo and duple beat, in slower tempo during song 29 C.

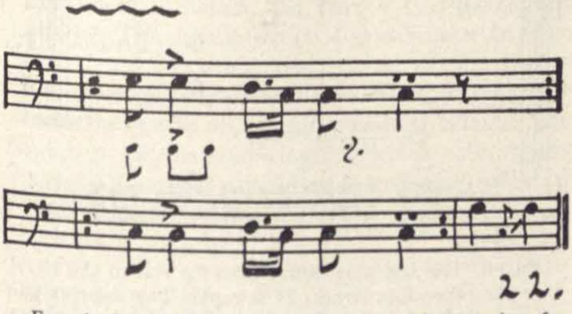


FIG. 4. Song 22 introduces a contrasting section for male singers and women circling with a side shuffle.

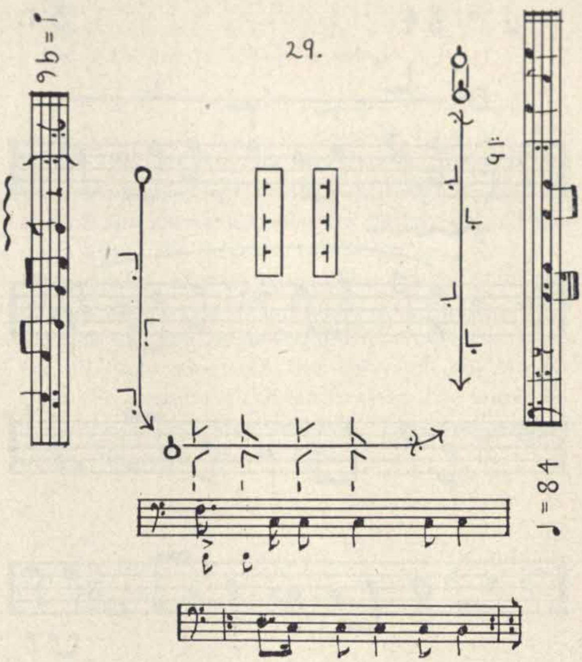


FIG. 5. Song 29 concludes the fifth part of the ritual. To male singing the women amble, then dance the so-called enskänye step, then run backwards.

Character: repetitious theme betwixt prelude and coda.

Dance: Enskänye, or women's shuffle dance step, facing center, twist both heels to right, sliding right foot back a few inches; then twist both toes to right, sliding right foot forward a few inches.

Torso erect, no gesture; knees rebound during each twist.

Progression as in Part IV.

Composition of song 29:

A—saunter straight ahead during tremolo.

B—sideward enskänye step during duple fast beat.

C—run backward with tiny steps during slower beat, each woman holding on to waist of dancer in front.

VI. *Strawberry Search,* men and women, 30–31 (Fig. 6).

Music: 1 transitional song and 1 long song, to percussion tremolo.

Character: very long, sustained phrase repeated four times in descending sequence.

Dance: Stride resembling Tutelo step—but three times as long—for duration of a half note, left foot step obliquely forward and left; for duration of quarter note, lightly place right foot next to left; repeat same to right.

During left stride torso and knee flexed oblique left, during right step straighten somewhat and face forward.

During left stride left hand swings out to left as though brushing away leaves from plants, during right stride draw back toward body.

Though focus toward center, progression straight ahead.

FIG. 6. Song 31 constitutes the sixth part, an enactment of a strawberry search to male and female singing.

VII. *Tutelo Step,* men and women, 32–36 (Fig. 7).
  Music: 4 complex songs to accented duple beat which lags behind (syncopates) melody after first few measures; 1 transitional song, terminal female whoop.
  Character: pattern of two short themes in sequence and alternation.
  Dance: Same as in Part III.

VIII. *Pairing and Crossover,* male singers, 37–40 (Fig. 8).
  Music: 4 songs similar to Part IV, to tremolo and duple beat.
  Character: repetitious, short staccato themes repeated in horizontal sequence, lively and vigorous.
  Dance: Forward walk and enskänye step in following pattern:
    A—every second woman face about, so as to form pairs face to face.
    B—in time with even duple beat, all do enskänye step, in quicker tempo.
    A—to accented beat, partners change places.
    B—to even beat, all enskänye.
  During next song change back to original position.
  Brief forward saunter between songs and during first few notes; otherwise circle static in location, with animation in footwork and interchange.
  This climaxes the cycle and ends with a whoop by men and women, "Yuhup!"

Preferably text and translation should be incorporated into the song, but in this case they

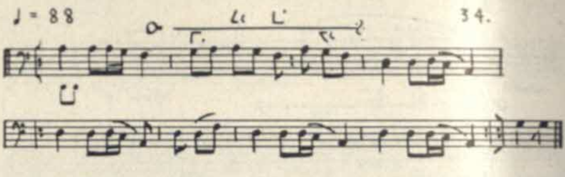


FIG. 7. Song 34 represents the seventh part, which resembles Part II in musical style and in the "Tutelo step" of the women.

would crowd the illustrations and are hence here quoted separately.

Fig. 1. *jiwagiho jiwagiho jiwagihonę | yonędi jiwagiho. yoho.*
  'hoeing at their gardens [they are]   yoho'
Fig. 2. *yowiyo henę yowiyo henę yowiyahe. he'e ho'o yowiyahe. yoho*
  'plenty corn [they are praying for a plentiful crop]'
Fig. 3. | : *wiyǫnka hinędo*: | *yoho.*
  'fine corn seed [kernel]'
Fig. 4. | : *biwa do*: | *yoho.*
  'nice seed' [?], or 'thanks!'
Fig. 5. *hayǫndo jihane. hohoyǫndo wohe'e. wayo yǫndo wohe'e.*
  '[corn] soup'   ?
  | :*hohenǫge*: | *yoho*
  'go backwards'
Fig. 6. *yoho hewiyo hoyahane. yoho.*
  '[have] plenty fruit'
Fig. 7. *weyowane hawegi heyowane. yoho.*
  ?
Fig. 8. *hewagile hewagile hewagileda. yoho.*
  'I [we] must go home'

The text obviously refers to the corn crop and the connected dance action, in spite of the non-Tutelo-speaking singer's modifications, Mrs. Claus'

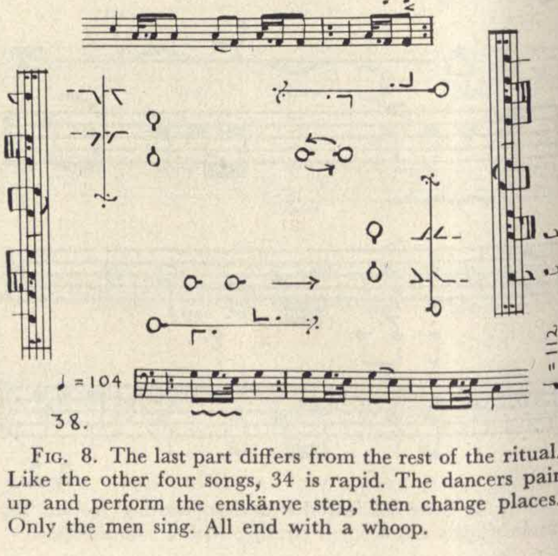


FIG. 8. The last part differs from the rest of the ritual. Like the other four songs, 34 is rapid. The dancers pair up and perform the enskänye step, then change places. Only the men sing. All end with a whoop.

frequent uncertainties and interpolations, and the diffusion through the Cayuga language. Some of the terms are new to us, others conform to scant vocabularies in previous publications.[3] As we cannot tarry on the linguistics, we refer to Horatio Hale,[4] Edward Sapir,[5] and Leo J. Frachtenberg.[6]

Analytic Devices

Stylistic variety may already have become apparent in the course of the descriptions and illustrations. This contrasts with the unusual homogeneity of the previously analyzed Tutelo Spirit Release Rite.[7] Eclecticism, therefore, is not a Tutelo characteristic and must have another reason. The precise nature of these styles can be ascertained by analysis of the musical tonality, structure, and rhythms, and of approximate choreographic measurements. The differences may imply the reason.

Tonality. Weighted scales give the clearest picture of the tonal material. They can be reduced to three types, a quartal scale based on a nucleus of fourths; a quartertial one, combining fourths and thirds; and a tertial one based on thirds, without excluding other intervals (Fig. 9). In most cases the tonal nucleus (marked by a bracket) hovers at the bottom of the scale, by the lowest or main tone (marked by a whole note with hold). Song 18 (Part III) has two nuclei focused on the center of the scale. The arrangement on the figure shows two progressions in the course of the rite from the quartal to tertial song groups. It does not show that Part I contains two quartertial songs and Part VII two quartal songs. The first scale varies in compass from an octave to a fifth, the second scale always contains a seventh or octave, and the third one is confined to a fifth. The performance of the melodies with their scales may also be analyzed.

Structure. Five of the song samples work with a single motif, *a,* and vary it by repetition on a lower level by changing the intervals. In Part III the motif has two phrases in balanced pattern. Part VII uses two themes, and Part V two themes and a coda. The manipulation formula can be symbolized by numerical designation of the highest and lowest tone in each phrase. Thus, 8:5:8 means "start on octave, dip to fifth, end on octave." The numbers in the following examples refer to the *tones* of the scale; *a* and *b* refer to themes, of which there is usually only a single one (namely, *a*), and perhaps a variant, *a′*.

```
Fig. 1—a  8:5:8   a 8:4   a 5:1
       a′ 8:5:8   a 8:4   a 5:1
       a 1:1   1:1   a 4:2:4   5:1
Fig. 4—a 3:5   a 3:1   3:1   a 1:2:1   1:2:1
Fig. 5—a 4:5:1   b 4:1   b 2:1   2:1   c 1:1   1:1
Fig. 6—a 8:6   a 7:8:3   a 4:6:1   a 1:4:1
```

This device shows the compass of each phrase, as well as its place in the scale. It shows how successive phrases descend the ladder, how two of the samples also begin on the highest tone, and all end on the lowest. An ensuing device will tell still more about the behavior of the phrases, their pattern of level, and compass. The symbol 8:5:8 indicates an interval of a fourth, for instance. Such symbols will now be shown in their respective levels for all eight songs. In the left-hand column we show the compass of the entire song. The numerals, *4,* etc., now represent *intervals.* The grouping will follow the scale type in Figure 9.

```
SONG
COMPASS     PHRASE COMPASS
  I. 8      :4
              5
               5:    3
                   11  5
 II. 5      4
             44  4
                1  1
III. 7                  4
                         76
 IV. 5                      3
                             32
  V. 5      5421
 VI. 8                  3
                         6
                          64
VII. 8                33
                               3
                         4| :4  44:|
VIII. 5                              3
                                      3
                                       333
```

Patterns

descent, shrinkage to monotone, 4th common	descent, central expansion, final shrinkage, no monotone, 6th and 7th largest	descent, shrinkage not to monotone, 3rd common

Each scale type exhibits special characteristics. Notably, the quartertial scale shows the broadest expansion, and the tertial scale the least contrast between phrases. Naturally the songs with limited compass are more level than those that encompass an octave. But every single one descends obliquely and sequentially.

After this abstraction and grouping one should return to the actual melodies for an appreciation of the ingenuity of thematic development, simpler in the case of the tertial songs, still regular in the quartal tunes, and more flexible though not extremely intricate in the hybrid scale type. At the

FIG. 9. "Weighted scales" show the tonal material of the eight parts of the ritual. They fall into three types, with varying structure and compass. The brackets show the tonal nucleus of the songs in Figures 1–8.

same time one can perceive the structural coherence and orderliness of each song and of the whole cycle.

Now for one more process of dissection—that is, the extraction of the smallest typical rhythmic unit in each melody and their grouping by the scale types, with the percussion beat included under each rhythm.

Rhythms. As in the case of thematic development, the rhythmic pattern varies with each song. However, the units in Figure 10, based on the song examples, can be considered typical. Although they speak for themselves, their relationship to scale grouping deserves a brief comment. The first and third scales contain lively, crisp rhythms, including syncopation in the first group and broken units in the third. The second scale contains placid, symmetrical units. The quick notes in 7. relate to the unit of 2. Parts VII and II are connected by the same dance step, we may recall. The liveliest rhythms are enhanced furthermore by the fastest tempi, especially toward the end of the cycle.

The duple drum beat predominates. After the first 17 songs a tremolo introduces each new melody. In 3. this is followed by a smooth slow beat; in 7. it perseveres as a sustained, wavering undercurrent. These are the mimetic songs, with long, sustained phrases.

On the whole, the rhythmic complexity is in inverse proportion to scale size and structural complexity. The most limited melodies make up for their repetitiousness by rhythmic interest within the phrase, and by more emphatic rendering.

Choreography. The ground plan consists in a steady counterclockwise circling until the final crossovers, but the step changes frequently. So we shall confine the analysis to the fundamental steps already described: the "Tutelo" step-brush and its stride variant, the side shuffle, the sidewards enskänye, and the interpolated walk, saunter, and backward run. The stride length in inches and the angle of flexion will be tabulated in the simplest terms, and the step types will be related to the scale types (Table 1).

Steps and Scales

Quartal	Quartertial	Tertial
I.		
II. Tutelo step		
	III. Tutelo step Mime	
		IV. Side shuffle
V. Enskänye		
	VI. Tutelo stride Mime	
	VII. Tutelo step	
		VIII. Enskänye

The feminine style is usually characterized by tiny, prim motions, as in the side shuffle, enskänye, and back run. Usually the knee flexes slightly with each impulse, although the posture remains erect. The Tutelo step is slightly larger, with mild torso inclination. The stride contrasts with the typical style by its length and the forward bend of the body. The Tutelo step and mime are associated with the second scale, the step alone also with the first scale. Enskänye is danced to songs of the first and third scales; the side shuffle is confined to the archaic group of tertial songs. Thus, as it happens, the tiniest steps accompany the most limited melodies; the largest movements, including the gestures, accompany the more elaborate, expanding themes.† Obviously these larger movements also require a slower tempo than the crisp shuffles like the enskänye.

Pattern of the Entire Cycle. Both musically and choreographically the rite falls into two large parts. The alternation of scales divides it into two halves I–IV and V–VIII. The tempo and step type divide it into I–V and VI–VIII. The fast enskänye in V brings a minor climax, then a reversal of direction in the backward run and return to the slow tempo.

† This would agree with Curt Sachs' observations on *Engbewegung* and *Weitbewegung* (confined and expansive motion), in *Eine Weltgeschichte des Tanzes*. Berlin: D. Reimer, 127–32 (1933).

TABLE 1
(See Fig. 11)

Step	Measurements						
	Length of Step (inches)			Angle of Flexion°			
	Forward	Side	Back	A Knee	B Torso	C	D
Tutelo stride	18	12		100	40	90	50
Tutelo step	6	6		140	20	140	20
Side shuffle		4		140	20	160	0
Enskänye		5		140	20	170	10
Back run			4	130	25	130	25

The last part works up to the real climax. Each half contains a mimetic section.

The center and end are emphasized in another way. Parts IV and VIII are melodically the most archaic, Part IV, also choreographically. The pattern of relative complexity could be expressed in an inverted pyramid, with the archaic Part IV at the bottom and, musically, also Part VIII.

I   VII   VIII (dance)

III   VI

II   V

IV   VIII (music)

Before turning to the interpretation of these numerical and stylistic patterns, we must call attention to the prevalence of the number 4 and, secondarily, of 6 and 12 in the song groupings. Although this division is not as rigid as in the Spirit Release Ceremony, it is nonetheless evident and significant.

On the Tutelo Trail

The stage is now set for an attack on the problem of identification. Do the analyzed qualities brand the rite as (1) Iroquoian, (2) Tutelo, or (3) something else?

1. The first assumption would not be unreasonable in view of two centuries of proximity, of recent transference to Six Nations longhouses, and of the "Iroquois feel" of many of the songs and steps. This can be answered quite precisely because of the thousands of Iroquois songs available in recordings studied by the writer. The huge repertoire contains a great variety of scales and structural patterns, along with a rich dance heritage. A thorough search tells the following story.

Music: The first and third scales are prominent in Iroquois music, the 54 21 scale especially in the women's Death Feast and other medicine rites and

FIG. 10. The rhythmic figures are grouped according to the scale types of the eight parts of the ritual. On the staff is written the fundamental melodic unit of the songs in Figures 1–8; underneath is the rhythm of the percussion instruments.

in some animal rites, the 5 3 1 scale in several esoteric rites, in a more developed form also in maize songs and the "Fish type."[7] But the second, 43 1 scale, occurs only in one instance, the Death Feast, songs 64–67.[8] This rite bears several resemblances to Four Nights Dance. The structural forms of descending sequence and dwindling repetition, the rhythms, the terminal call, the echoing of the songs by a female chorus, all could conceivably be Iroquois. Thus only one element, uniqueness of scale, isolates two parts of the rite, III and VI. This element is the most stable and fundamental of all and thus most significant.

Dance: The counterclockwise circle comprises nine tenths of Iroquois dance forms. The paired crossover concludes a number of rites and constitutes the pattern of the "Fish type."[9] The side shuffle predominates in medicine rites, as in the Death Feast. The enskänye step threads through all of Iroquois ceremonialism, including the "Fish

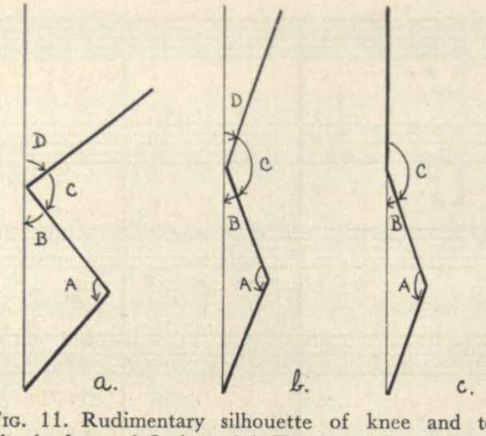


FIG. 11. Rudimentary silhouette of knee and torso angles in forward flexion: (*a*) Tutelo stride, (*b*) Tutelo step, (*c*) side shuffle. Enskänye step is slightly more erect and back run is little more flexed than (*b*). These represent only two of the many measurements involved in accurate choreographic analysis.

type," maize dances, and the end of Death Feast;[10] however, the "Tutelo step" is atypical. In only one Six Nations Dance is it duplicated, in a variant form—in the Delaware Skin Dance taken over from that tribe. The associated mime finds no equivalent. Enskänye dancers may, if they wish, use tiny, angular, stylized gestures,[11] but never in unison, never as an occupational imitation, and *never* with large strides or sweeping arms. The step in Parts II, III, and VII exceeds the enskänye dimensions only slightly, the stride in VI contrasts completely and surprises the spectator accustomed to Six Nations stylistic restrictions.

In sum total, Parts III, VI, and VII depart musically and choreographically from Iroquois traditions, Part II for certain only choreographically. Parts I and IV fit into the picture, especially the latter into the more archaic patterns. Part VIII advances three good arguments for influence from the popular Iroquoian "Fish-type" style.

2. The Tutelo could by this formal testimony claim Parts II, III, VI, and VII as their own. Before conceding this or possible Iroquois influence in the other sections, it is necessary to follow the rite backwards on the long trek from the southern homeland.

The late Tutelo chief John Buck stated that "Four Nights Songs are still sung in the same way as they used to be done."[12] Used to be done when? Printed records go back only fifty years. Herzog transcribed two of John Buck's recordings,[13] of which No. 8 is almost identical with our No. 34. Cringan transcribed an Iroquois version of 8 songs, selected from our 31–40, and essentially similar to these.[14] Simultaneously, at the turn of the century David Boyle described the Four Nights Dance as "really a series of dances, for the music and steps changed frequently," just as today, but it "was engaged in by men and women"—not as today, either because of faulty observation or because the personnel has changed.[15]

Four Nights Dance has been incorporated into Iroquois harvest rites since 1848. Between that date and the Revolutionary War it was celebrated in a separate longhouse on adjacent Tutelo Heights, evidently in combination with several Iroquois dances.[12] Tutelo membership in the Cayuga Wolf clan dates back to their temporary Pennsylvania residence in 1753. As we retrace their migrations back to Virginia, their trail and their very identity become ever vaguer. Mooney established them and their twin tribe, the Saponi, on the Roanoke River in 1671,[16] in a fine and prosperous location. In their Piedmont homes the Saponi had no special longhouse or "state house," according to John Lawson, who visited them in 1701.[17] This observation agrees with the archaeological discoveries by Joffre L. Coe, who in a verbal communication and in a paper described their villages of circular homes without a square or temple. This again would agree with John Buck's account to Speck regarding performances in homes, a different one during each of four nights, with all-night dancing and a feast.[12] It contrasts with the elaborate council houses and ceremonial grounds of neighbors, such as the Cherokee and Creek, and of the Sioux west of the Mississippi.[17] Despite continuous contacts with these and even more with the southerly Tuscarora and the Shawnee to the north, the Tutelo maintained a conservative and independent culture pattern.[18]

3. Nevertheless, it pays to examine and compare the song and dance styles of neighbors and linguistic relatives, wherever fragments are available. The outcome may be condensed into a few words, for details can be found in previous publications by the writer and in references appended thereto.[19] All these tribes performed first fruit and harvest ceremonies, lasting four days on square grounds (Creek and recent Cherokee), to seven days on heptagonal grounds (seventeenth-century Cherokee).[20] Women had a special dance in all of them, on the fourth day. The "long dance" of the famous Creek busk appears to have been quite different from that of the Tutelo. However, Bartram described an eighteenth-century Cherokee women's step resembling the enskänye.[21] A similar step has been reported for the modern Shawnee Bread Dance. Both the Cherokee and Shawnee women

also use a double stomp step exactly like the Delaware Skin Dance and very similar to the Tutelo step. Cherokee women, and men too, make extensive use of gestures such as corn pouring during the Corn Dance.[22] The Cherokee also pair and cross over in several dances. Other Shawnee resemblances consist in a division of the Bread Dance into eight parts (alternately for women alone and for both sexes) and in the significance of the numbers 4 and 12.

Musically, the Shawnee appear closest. A Pumpkin Dance transcribed by Bruno Nettl from the Voegelin collection uses a 54 21 scale in mildly descending sequence.[23] The Cherokee also sometimes use sequence, but in a 7 5 3 1 scale. Creek songs use a period formation instead of sequence; that is, they contain statement and response. Such antiphony, which also prevails among the Cherokee and Iroquois, is absent in Tutelo music.

More musical similarities occur in Omaha songs across the Mississippi. A maize ritual song resembles the theme of Tutelo song 18.[24] Numerous songs show the 54 21 descending sequence, and particularly resemble Tutelo song 4. A few Omaha and Pawnee songs also use a 43 1 scale, but not in maize rites.[25]

In contrast with the rarity of this hybrid scale and of the "Tutelo step," the side shuffle and associated simple 5 3 1 tonality are virtually ubiquitous in older ritual songs of the Eastern woodlands. The counterclockwise circling also characterizes the rites of all tribes of this area. Here Tutelo and Iroquois shared in a widespread pattern, whereas Tutelo step and gesture appear Southeastern. But the stride and the song style of Parts III and VI so far have no equivalent.

Function as First Fruits Rite

With all their conservative tendencies, the Tutelo must be regarded as participants in a larger Southeastern cultural complex. Maize ritualism had reached impressive proportions, especially to the south. Yet even among the Creek it incorporated a hunt and animal dances.[26] Among the Tutelo, as among the Shawnee, agriculture never surpassed the hunt as a means of subsistence,[27] and first fruit rites would pay homage to beasts and wild crops as well as to maize. Actually, the name Four Nights Dance does not refer strictly to corn, and only Part III represents a harvest of corn, Part VI a harvest of wild fruits. The (rather doubtful) text translations more consistently involve crops. Regarded in the light of a first fruits rite, the stylistic peculiarities make sense by means of the following hypothesis:

1. The central archaic Part IV partook of a widespread, ancient substratum, possibly as animal first fruits rite. Part VIII might fit into this concept, despite the complex choreography, especially when we remember that among the Iroquois this ground plan is confined mostly to animal dances.

2. The more developed Parts I, II, V, and VII connect both musically and choreographically with tribes to the immediate northwest, the semiagricultural Siouan Omaha and, considerably more, the Algonquian Shawnee. Analogies with Iroquois songs select women's medicine and ghost rites. Inter-Tutelo analogies are confined to mortuary and regeneration rites. Cherokee resemblances are confined to dance steps. Though this fits into conclusions of archaeologists and historians of prehistoric and historic proximities,[28] yet it is best to refrain from premature suggestions concerning the heritage of these song groups.

3. Parts III and VI definitely serve as harvest celebrations. Their greater complexity points to a fairly recent period; their tonal uniqueness indicates local origin right there among the Tutelo, though not perforce simultaneously.

In contrast with the homogeneous Tutelo mortuary rites, Four Nights Dance thus might account for its accumulation of patterns by growth at different periods and under various influences. The sum total of forms gives several clues as to Tutelo culture: the importance of women, their share in food gathering and preparation of corn and unleavened bread, artistically a vigorous and precise, well-organized quality, a predilection for functional mime and for functional, repetitious, and unadorned designs, corresponding to the recovered artifacts. Traditions and facts indicate unostentatious, domestic ritualism, with dances accommodating their circular ground plans to the circular confines of the habitations.

In the southerly Piedmont climate the first green corn would have been celebrated not much later than the Creek busk, hence in July. The migration to northerly Ontario would synchronize this date with the time of strawberry harvest and would gradually delay the festivity to coincide with the harvest of that climate. This is, indeed, what has taken place.

Beyond these suggestions, further study of adaptation in dates, forms, and style will be left for another time. Conjectures require help from other cultural sciences. For the most part these must deal with the past. For the modern vestiges of the Tutelo tribe have lost not only their ancient homeland but also every trace of their material culture and social structure. They have clung tenaciously only to artistic elements of three splendid rites, and that because their one-time enemies, the Iroquois, have perceived the superior qualities of these works,

have admitted them to the longhouse, and have conscientiously reproduced both the familiar and the exotic forms. Despite inevitable changes and possibly additions, they have left the essentials intact. In this case the combined sciences of musicology and choreography must be called in from the periphery to contribute another chapter to ancient life in the American Southeast.

References

1. Kurath, G. P. *Iroquois Symposium*, Pt. 7. *Bur. Am. Ethnol. Bull. 149,* 165 (1951).
2. *Ibid.*, Pt. 6, 135.
3. *Ibid.*, 132.
4. Hale, H. *Proc. Am. Phil. Soc.*, **20**, 114 (1883).
5. Sapir, E. *Am. Anthropol.*, **15**, 296 (1913).
6. Frachtenberg, L. J. *Ibid.*, 479.
7. Kurath, G. P. *Midwest Folklore*, **3**, (2), 1953).
8. ———. *Bur. Am. Ethnol. Bull. 149,* 157 (1951).
9. *Ibid.*, 115, 122.
10. Kurath, G. P. *Proc. 29th Intern. Congr. Amer.*, Vol. 3. Chicago: Univ. Chicago Press, 124 (1952).
11. *Ibid.; Bur. Am. Ethnol. Bull. 149,* 132, 156 (1951).
12. Speck, F. G., and Herzog, G. *The Tutelo Spirit Adoption Ceremony.* Harrisburg, Pa.: Penna. Hist. Comm., 119 (1942).
13. *Ibid.*, 94.
14. Cringan, A. T. *Archaeol. Rept. Min. Educ. Ontario,* 179, (1902).
15. Boyle, D. *Ibid.*, 119 (1898).
16. Mooney, J. *Siouan Tribes of the East. Bur. Am. Ethnol. Bull. 22,* 37 (1894).
17. Swanton, J. R. *Ibid.*, 137, 391 (1946).
18. Coe, J. L. In J. B. Griffin (Ed.), *Archaeology of Eastern United States.* Chicago: Univ. Chicago Press (1952).
19. Fenton, W. N. *The Iroquois Eagle Dance. Bur. Am. Ethnol. Bull. 156* (1952); Kurath, G. P. *Am. Anthropol.*, **55**, (1), (1953); *Midwest Folklore,* **3**, (2), (1953).
20. Witthoft, J. *Green Corn Ceremonialism in the Eastern Woodlands.* Ann Arbor: Univ. Mich. Press, 42 (1949).
21. Bartram, W. *Travels.* Philadelphia: (Macy-Masius ed. [1928]), 299 (1791).
22. Speck, F. G. *Cherokee Dance and Drama.* Berkeley and Los Angeles: Univ. Calif. Press, 77 (1951).
23. Nettl, B. *J. Am. Music Soc.*, **5**, (2), (1952).
24. Fletcher, A. C., and Fillmore, J. C. *Ann. Rept. Bur. Am. Ethnol.*, **27**, 262 (1911).
25. ———. *Archaeol. and Ethnol. Papers Peabody Museum,* **1**, (5), 7, (1893).
26. Witthoft, J. *Op. cit.*, 84.
27. Voegelin, E. *Papers Mich. Acad. Sci.*, 26, 520 (1940).
28. Fenton, W. N. *Historic Northeastern Position of the Iroquois. Smithsonian Misc. Collections,* 100, 194, 232, 241 (1940).

THE MIGRATION

Day after day the birds are coming back
Under the turbulence of windy rain.
Through mountains where the sudden storms attack
The small wings northward find their way again.
The chilling hail swirled from unfriendly space
Is a hard thrust against them in the night;
But on their journey nothing will erase
The urge that gives them their instinctive flight.

There is a transient action in the sun:
A morning suddenly has birds and song.
They haunt the budding countryside and run
Through trees expanding to make shadows long.
As chains of cold loosen about the land,
They reappear—and then we understand.

Daniel Smythe

Delanson, New York

The Tuatara: Why Is It a Lone Survivor?

CHARLES M. BOGERT

The author may well attribute his interest in herpetology to his early years in the American Southwest: He was born in Mesa, Colorado, and was educated at the University of California, Los Angeles, where he was a teaching assistant for two years. He has been at The American Museum of Natural History since 1936 and curator of the Department of Amphibians and Reptiles since 1943.

WHAT happened to the rest of the beakheads? It seems unbelievable that *any* beakhead managed to survive. Theoretically they should have become extinct about 135 million years before the dodo was even discovered. Strangely enough, the one surviving beakhead was made known to science almost exactly one hundred and fifty years after the dodo disappeared.

In 1831 when J. E. Gray[1] described the original specimen of the only existing beakhead, he scarcely realized that the reptile skull he had in his hand was anything very spectacular. The title of his brief paper indicates that he thought he had a lizard skull, but he says nothing concerning its source. He observed that there was an odd arrangement of the lower jaw in its attachment to the skull, which he said would "doubtless form the type of new genus, which [in allusion to the wedge-shaped teeth] I propose to call *Sphaenodon*."

Eleven years later the good professor had virtually forgotten about the odd skull. When he examined some reptiles that a Dr. Dieffenbach brought back from New Zealand, he described two of them as new to science. Gray[2] placed one of these in the family that includes the common Old World lizards of the genus *Agama*. Not realizing that his specimen possessed a skull like the one he had examined over a decade before, he coined an entirely new name for the genus and supplied a name for the species, calling the creature *Hatteria punctata*. In a few terse phrases he described the external appearance of the reptile, adding a paragraph that consisted of three words, "Inhabits New Zealand."

To this brief description he appended some notes supplied by Dr. Dieffenbach, who had observed that the species "lives in holes, especially on the slopes of the sand hills of the shore. The older missionaries say it was formerly common, and the natives lived upon it, but for the last fifty years it has been scarcely ever seen. This specimen was found on a small rocky island, two miles from the coast, in the Bay of Plenty. . . . It is extremely sluggish in captivity, and could be handled without any attempt at resistance or biting." Still quoting Dr. Dieffenbach, Gray added, "The natives called it 'Tuatara.'"

These notes and descriptions produced no furor. In fact, they remained virtually buried for the next twenty-five years, when in 1867 Albert Günther,[3] of the British Museum, carefully examined some tuataras, both inside and out, and came up with the first really startling conclusion concerning this New Zealand reptile. Günther said it was not a lizard at all. Moreover, he pointed out that the term *Rhynchocephalus* (from the Greek *rynchos* "snout," and *kephalē* "head," roughly translated as "beakhead") had been applied to the same reptile by Sir Richard Owen[4]—the same man who coined the name "Dinosauria," incidentally. Furthermore, said Günther, these reptiles were so distinctive that they belonged in a separate order equal in rank to the crocodilians, the turtles, or to the group that contains both the snakes and lizards. Using Owen's name as a basis, he applied the name Rhynchocephalia to the new order.

Offhand, this verdict does not sound especially provocative. But it was enough to stimulate a stream of studies that continues down to the present day. Perhaps a hundred technical papers have been written about New Zealand's tuatara. Its bones, skin, tail, brain, teeth, muscles, and other organs have been examined, described, and compared with those of other reptiles. Its breeding habits and its development have been studied meticulously. Its rate of oxygen consumption and its heat production have been measured. Most biological texts mention the tuatara, and numerous

Two views of a tuatara (*Sphaenodon punctatum*), the only surviving beakhead, photographed on Little Barrier Island, Hauraki Gulf, New Zealand, in February 1948 by Robert Cushman Murphy. The eye is so dark that the elliptical pupil is not apparent except upon close examination.

popular articles have described it, often with misplaced emphasis on its "third eye."

Actually this vestige of an eye in the forehead of the tuatara is not so spectacular as many accounts would indicate. Externally the "eye" is rather easily seen in the hatchling as a small transluscent scale, but in the adult is it barely discernible. In fact, a photograph widely published[5] over a decade ago included a pencil pointing toward what was said to be the "third eye." In reality it indicated the location of what was apparently a scar slightly off to one side and much farther forward on the head.

It is quite true, however, that this "parietal eye," to use the technical term, does indeed contain some of the structures of an ordinary functional eye; in particular, a lens and a retina are present. Also the retina, the deeper layer of the eye that contains sensory cells, may be connected to the forebrain. At least there seems to be a connection in the hatchling,[6] although it may degenerate in the adult.[7] But there is no iris or similar mechanism to regulate the amount of light that reaches the sensitive layer. Since the parietal eye in fully grown tuataras is covered with skin, it is doubtful whether any appreciable amount of light reaches the retina.

In some true lizards there is a large transparent scale over the eye that corresponds to the cornea or the outer covering of the eyeball of normal functional eyes. Thus the parietal eye is far more conspicuous and even better developed in such American reptiles as the horned lizards, the fringe-footed sand lizards, or the ordinary anole (the false chameleon commonly sold at circuses). For that matter, all the structures found in the parietal eye of the tuatara have also been found[8] in the Australian stump-tailed lizard, even though other members of the family to which it belongs may lack even the hole in the skull that marks the location of the parietal eye in many lizards.

The really interesting aspect of the third eye lies in the fact that it is a remnant of a pair of eyes, one of which all but disappeared in ancient times. One eye is suppressed to develop as the pineal organ, believed by some to be one of the ductless glands. In lizards it is the one on the right side that is retained as the vestige of an eye;[6] in the tuatara it is the one on the left. It has been suggested that the purpose of this elementary eye was to warn the animal of the approach of an enemy from above.[7] Were there any evidence to indicate that an eye on the top of the head came into existence or showed signs of improvement as the flying reptiles or the birds evolved, this idea might be taken more seriously.

Repeated experiments have failed to prove that the parietal eye is of any real use either to the lizard[9] or to the tuatara. It has nothing to do with vision. Even though there are indications that the parietal eye of the anole is sensitive to light, other structures in the skin are of greater importance in this respect.[10] If heat is directed to the parietal eye it seems to be no more sensitive than the skin. The most that can be said is that, in some lizards that bask in the early morning, a transparent scale over the parietal eye might permit the sun's rays to warm the brain somewhat more rapidly than would be the case were the central nervous system completely covered with bone and skin.[9]

However, the discovery that the vestigial eye in the lizards and in the tuatara arises from separate parts of the original pair provides further support for Günther's belief that the New Zealand reptile is not a lizard. Günther pointed out several other differences that set the tuatara apart from the lizards, noting that in some respects it more closely resembles extinct reptiles, crocodilians, or birds.

Later studies have continued to bear out Günther's conclusions. One of the more interesting facts that have come to light concerns the nature of the "egg breaker" of the hatchling tuatara. After twelve or thirteen months of development within the egg, the fully formed young would be imprisoned within the leathery shell that has held it in protective custody were it not for a sharp spine on the tip of its snout. As the time for hatching approaches, moisture is absorbed by the egg until its shell becomes as tight as a rubber ballon. Thereupon the spine is brought into play. As soon as it punctures the shell, the entire end of the egg splits wide open and the young tuatara emerges, ready to dig its way to the surface.[11]

Close examination of the egg breaker, which falls off within a week after hatching, shows that the spine is a horny outgrowth from the skin—precisely the same sort of structure that occurs in turtles, crocodilians, and birds. Although this horny spine, more properly a caruncle, is commonly called an "egg tooth," it is only in the lizards and the snakes that actual egg teeth are found,[12] for the egg slitter of lizards and snakes is a real tooth made of dentine and attached to the bone. In some lizards two egg teeth are present, but whether single or paired, such teeth have razor-sharp edges that serve not merely to puncture the shell, but actually to slit it.

The presence of a caruncle in the tuatara suggests that it is less lizardlike than one might expect from its outward appearance. But its habits are not appreciably different from those of other existing reptiles. It does have special breakage planes in its tail, which can be regenerated when lost.[13] In this respect it is like most lizards and unlike turtles, crocodiles, or snakes, all of which remain stumped-tailed if a predator nips off the rear end.

Like the crocodilians and some lizards that are active at night, the tuatara is equipped with vertically elliptical pupils and a voice, but its call is in no way similar to the roar of an alligator; nor does it resemble any of the various sounds—squawks, chirps, or whistles—produced by the nocturnal lizards called geckos. On the contrary, the call of the tuatara is a croaking noise, more nearly comparable to that of some frogs.

The time required for the incubation of the tuatara's eggs is abnormally long for a modern reptile, but even in this respect it is approached by some turtles. Furthermore, the eggs of the tuatara, scarcely an inch long, are extraordinarily small for a reptile that commonly attains a length of two feet. The egg of a Gila monster of similar dimensions would be over twice as long and several times as bulky. It is quite evident that the eggs are fertilized internally, as they are in all reptiles, but another important difference that distinguishes it from all modern reptiles turns up here—the male tuatara lacks any apparent means of inseminating the female.

Despite peculiarities that show rather conclusively that New Zealand's famed reptile is not a lizard, there is nothing outwardly spectacular about the tuatara. Superficially it resembles such lizards as the larger iguanas of the American tropics or several Old World lizards, in having a row of horny spines on a ridge down the middle of the back. Even though exceptional specimens are reputed to reach a length of thirty inches and to weigh over two pounds, the tuatara would not be regarded as large among such contemporary reptiles as marine turtles that weigh over three quarters of a ton, twenty-foot crocodiles that may weigh even more, or snakes that may reach a length of thirty feet.

Today the tuatara is restricted to a tiny fraction of the earth's surface, a score of small islands off the coast of New Zealand in Cook Strait and the Bay of Plenty.[14] The Maori were doubtless the first people to see it, and they took some interest in it, noting that it was edible. Today it is of no economic importance. Why, then, should this reptile attract so much attention? The answer lies in the eventual realization that the tuatara is a relict, a living fossil—another way of saying that it is the lone survivor of a group of animals that had its heyday in the distant past. For the animal to which Owen applied the fancy Latinized Greek name meaning beak-head should have been a fossil—a creature that had died millions of years before Owen examined its bones. Why, one may well ask, does a relict warrant such extensive study? One answer lies in the fact that by learning all we can about such relicts we can hope to find some way of accounting for the extinctions of the larger reptiles. Entire groups, including all the real giants, faded from the scene during Cretaceous time or even earlier.

When the dinosaurs still roamed the earth 150

million years ago, the ancestors of the tuatara were their unimpressive neighbors. The dinosaurs rapidly—at least to the geologist—expanded into a multiplicity of forms and then disappeared from the face of the earth. The relatives of the tuatara evidently underwent little change, at least in their bony structure, despite the fact that they managed to out-survive the dinosaurs. But from what evidence we can piece together it was a precarious survival. Had the tuatara not reached a distant outpost, it might well have preceded the dodo into oblivion. But did this odd reptile manage to hold on in New Zealand because no important enemy existed until man eventually appeared, bringing along dogs, cats, and the inevitable rats? Or was it New Zealand's climate that saved it?

Before attempting to answer these questions we need to know something of the tuatara's history, its relatives, and their reputations as world travelers. The tuatara, as one of the rhynchocephalians, has a fossil record that is evidently incomplete; but it suggests that the beakheads never loomed very large in the evolutionary stream. Despite the present-day existence of the tuatara, not one bone identifiable as that of a beakhead has been discovered in the rocks laid down since the early Cretaceous period,[15] some 135 million years ago. The few skeletal fragments left behind in still older rocks suggest that the beakheads branched off the same stem that later gave rise to the dinosaurs, as well as to the crocodilians, lizards, and snakes. But while other branches of this stem were developing into birds, and the mammals were descending from an even earlier offshoot of the main reptile stock, the beakheads seemingly never gave rise to anything except more beakheads.

In all probability the ancestral lizard was, like the dinosaurs, a contemporary of some of the early rhynchocephalians. The turtles were present, and it is likely that they already had acquired many of their distinctive characteristics, including the armor that has seemingly led to their persistent but otherwise limited success. However, the beakheads were past their peak and approaching their decline before the turtles, crocodilians, lizards, or birds began to flourish.

That the beakheads attained a small measure of success is attested by the fact that some 200 million years ago there were several kinds and that some of them spread over much of the world. Their remains have been found in deposits laid down in ancient times in Africa, Europe, the Americas, and Asia.[16] Possibly the ancestral tuatara reached New Zealand by way of Australia, despite the fact that no beakheads, either fossil or recent, are known from that continent. Or perhaps Australia was bypassed, and the beakheads, by dint of a little swimming, got to New Zealand over a circuitous route represented by a chain of islands, now widely separated, that extends from New Guinea through New Caledonia, and swings southward to include Norfolk Island. This tiny island lies between New Caledonia and New Zealand, with about five hundred miles of ocean separating the two. New Zealand lies even farther from Australia or Tasmania, with over a thousand miles of ocean in between.

It is not difficult, of course, to account for the presence in New Zealand of bats that fly and of marine mammals so obviously able to swim; such flightless birds as the kiwi and the moa descended from winged ancestors.[17] But it is doubtful whether we shall ever learn precisely how the tuatara reached the islands. Possibly there were no warm-blooded animals in existence when it accomplished the feat, for the ancestors of the birds and mammals may not yet have had the mechanisms for internal heating. In any event, it is generally believed that land-dwelling mammals did not reach New Zealand until man belatedly arrived.

Snakes were abundant in Australia, and a few got to New Caledonia, six hundred miles or so off the east coast. Nevertheless, they never succeeded in reaching New Zealand. Several geckos and skinks did, but these lizards secrete themselves in man's boats and other belongings and turn up in virtually all the tropical or temperate oceanic islands inhabited by man. Those surviving in New Zealand, however, must be able to tolerate a relatively cool climate.

New Zealand, in almost the same latitude as Patagonia, lies as far from the equator as New York, but its climate is relatively cooler, with its warm season more like that of Newfoundland in summer. Owing to the tempering effect of the surrounding waters, its winters, on the other hand, are similar to those of the American Gulf Coast. Days when a reptile might bask in the sunshine are limited in number. Cloudy, overcast days are more numerous, for the rainfall of New Zealand equals that of the wettest parts of temperate North America. Portions of the islands at lower elevations have twice the rainfall of New York, and in the highlands the rainfall is considerably heavier, amounting to more than 200 inches.

It may be of considerable significance that the tuatara survives in a region so cool that it would be shunned by most modern reptiles. It is further noteworthy that lizards become abundant in the fossil

Adult tuataras in the group at The American Museum of Natural History in New York. Until 1952, when zoological gardens in San Diego, Chicago, and New York each obtained a live specimen, the tuatara was known to most Americans only as a living fossil depicted in textbooks. Although superficially lizardlike, several anatomical and physiological peculiarities mark it as the sole survivor of the order Rhynchocephalia, most members of which became extinct 135 million years ago.

record at approximately the same time that the ancestors of the tuatara disappear. On the whole, lizards seem to have replaced the tuatara and its relatives. Or the increase in the numbers and kinds of the heat-loving lizards may have been brought about by the same factors that resulted in the near disappearance of the beakheads.

Several lizards occupy the islets now inhabited by the tuatara, and apparently they do not interfere with it—indeed, they may be preyed upon by the tuatara. There were tuataras on the main islands when the first Europeans arrived, as Dr. Dieffenbach's account notes. Soon afterward, with the introduction of hogs, cats, and rats, the tuatara probably became exterminated on the larger islands. As recently as 1940 occasional tuataras are said to have been seen in the more inaccessible parts of the main islands,[5] but these reports remain unverified. It is altogether probable, therefore, that the tuatara's presence on the islets at first uninhabited by cats, rats, and pigs saved it from extinction.

On Stephen Island the later introduction of the domestic cat might well have finished off the beakhead population had steps not been taken to destroy the cats. In 1899 a report stated that several dead tuataras partly eaten by cats had been found. Fortunately the New Zealand government employed various means of eliminating the cats, and the methods seem to have been effective—at least the tuatara population on Stephen Island appears once more to be thriving.[18]

Quite aside from the late entry of cats into the picture, circumstances suggest that in other parts of the world the evolution of mammals might have played a part in the near extinction of the beakheads. We may also infer that the ancestral tuatara reached New Zealand before either the mammals or the lizards began their major expansion. If we consider the almost disastrous effects of the advent of mammals in New Zealand in relatively recent times, there can be little doubt that the tuatara would not have survived until the present had it not reached this remote asylum well ahead of the mammals.

Other questions remain unanswered, however. Were the ancestral beakheads inhabitants of regions as cool as New Zealand? Or did the tuatara gradually acquire the ability to live under such conditions?

The tuatara is sometimes cited as a remarkable case of evolutionary stagnation.[15] The skeleton of a reptile found in the Jurassic deposits of Europe is so nearly identical with that of the living tuatara that very little change in the bony structure must have taken place during a period of 150 million years. This suggests that the ancestors of the tuatara were not especially plastic—their stock did not produce any modifications, at least in the bones, that might have been necessary for survival under specialized conditions. It is not necessarily a reliable inference, but reasons might be advanced for the belief that the ancestral tuatara lived under climatic conditions similar to those of its surviving descendant.

It seems probable that in the ancient past climates throughout the world were more nearly uniform than they are today. There is evidence that the zoning of climates began to become more pronounced about 60 million years ago. It cannot be established with certainty whether there was a trend toward increasingly warm climates following the demise of the dinosaurs, but this has been suggested. In any event, no one who is familiar

with the evidence doubts that there have been numerous climatic changes. Geologists find good evidence of widespread glaciation during the Permian Period over 200 million years ago, and we are, of course, now emerging from the great glacial epoch in which an ice sheet covered much of Canada and portions of the United States.

But was it generally colder during the time of the dinosaurs than it is now? It is extremely difficult to find any real proof that it was, but Raymond B. Cowles,[19] of the University of California at Los Angeles, has suggested that increasingly hotter climates may have been a factor of major importance in the disappearance of many reptiles during the great crisis near the end of the Mesozoic era 60 million years ago.

Cowles further points out that the descendants of reptiles, whose ancestors were living alongside the dinosaurs, can be sorted into three classes. First, there are the smaller species, particularly the lizards that escape the heat by burrowing, seeking crevices, or by emerging only at night. Second, there are the crocodilians and the marine turtles, the largest reptiles that still survive, but perhaps only because they take advantage of relatively large bodies of water, which never attain the high temperatures of the land. Finally, he points out that it was the warm-blooded mammals and birds that really began to flourish as the large reptiles began their rapid decline.[20, 21]

We may never know when the birds and mammals reached their present warm-blooded state. We do not have the vaguest notion when fur came into existence, but we do know that feathers were present on birds that were still reptilelike during the Jurassic. Some sort of insulating coat was necessary, it is assumed, before an animal would find it advantageous to utilize energy for internal heating. Did warm-bloodedness precede the feathers or the fur?

Professor Cowles doubts that it did. A man forced to spend his summers in Death Valley might well build himself a well-insulated house to protect himself from the heat. Finding that he had to spend a winter there, he would doubtless be pleased to realize that insulation had its virtues during the cold period when he had to rustle wood for his fireplace. Cowles[22] suggests, therefore, that fur and feathers came into existence because such body coverings afforded protection from increasingly greater amounts of heat that reached the earth from the sun. At first such insulation merely permitted these animals to engage in more extended forays in search of food. When the sun approached the zenith the animals with overcoats could remain abroad when the others were forced to retire—if they could burrow or find shade. Later, having become well endowed with fur or feathers, the birds and the animals were better equipped to acquire and to perfect the internal heating mechanisms that led to warm-bloodedness.

Thus, if Cowles' speculations are correct, the smaller reptiles, the shore-dwelling crocodilians, and the insulated birds and mammals might have been well prepared to tolerate increasingly hotter climates when the dinosaurs could not. For bulky land dwellers would have found it difficult to remain abroad long enough to nourish their huge bodies and still avoid overheating. What was worse, it would have been impossible for them to burrow, and difficult for them to find suitable shelter.

Since we cannot experiment with dinosaurs, we are forced to base any assumptions concerning their heat tolerance on what we can learn from their nearest surviving relatives. The crocodilians are descended from a stock that also gave rise to the dinosaurs, and we find that alligators die if their body temperature rises much above 100° F.[23] In contrast, some lizards are habitually abroad with temperatures as high as 107°,[24] and for short periods nearly all snakes and lizards can withstand body temperatures of 104°, or even higher.

Thus, without having recourse to oceans, lakes, or rivers, the crocodilians would stand little chance of surviving in regions where high temperatures prevail during any part of the year. Indeed, they are absent from all deserts except those traversed by such large rivers as the Nile. Furthermore, even though crocodilians bask, they do most of their feeding at night, after temperatures have dropped.

If the dinosaurs were no better able to tolerate high temperatures than the crocodiles and alligators, they may have encountered severe problems with increasingly hotter climates. We do not know whether dinosaurs were adapted to forage at night, but there are good reasons for believing that many of them were land dwellers, like the beakheads. Consequently, there are advantages in learning something of the heat requirements of the beakhead surviving in New Zealand, more especially since its relatives in all other parts of the world came to the end of their rope at very nearly the same time that the dinosaurs did. It was with this idea in mind that I asked Robert Cushman Murphy to take along some special thermometers when he generously offered his services as he was about to set out in 1948 for the Pacific Science Congress in New Zealand. There Dr. Murphy met Karl P. Schmidt, chief curator of zoology of the Chicago Natural History Museum, who was also attending

the congress. Having devoted most of his career to the study of reptiles, it need scarcely be added that Dr. Schmidt planned to see tuataras in their natural habitat.

To do so he arranged to go to Stephen Island, long famed among the islets in Cook Strait as a sanctuary for the living fossil. When Dr. Schmidt set out in February he was accompanied by William H. Dawbin, a competent zoologist from New Zealand's Victoria University College. Learning that we needed a series of temperatures, Mr. Dawbin returned to the same island in April, and again in November, when he stopped briefly at Trios Island en route. On these two trips Mr. Dawbin managed to obtain temperatures of 76 tuataras.

This entailed considerable work at night, since we wanted to know what range of temperatures the tuatara tolerated while it was abroad and active. During the daylight hours it rarely ventures far from its burrow. It may bask at the entrance on sunny days, but if the day is overcast it is unlikely to be seen.[25] It comes forth to feed principally at night, and probably its mating activities, which have never been observed, are also carried on in darkness. Mr. Dawbin was forced to search for tuataras between sunset and midnight, often when it was too cold for comfort, with air temperatures rarely above 55° F. Even so, the tuataras were evidently foraging actively, and in November they may have been depositing their eggs. Despite the time required to record temperatures, not only of each reptile but of the air and the ground where each individual was taken, Mr. Dawbin caught as many as 24 tuataras in a single evening.

Knowing that the tuatara inhabited a moderately cool region, we expected it to tolerate low temperatures. Nevertheless, we were not wholly prepared for the results obtained by Dawbin. The highest body temperature he recorded for an active tuatara was 56° F., and some were engaged in their normal pursuits when the body was only 11° above freezing. The average temperature of active tuataras was just under 52° F. These are not temperatures of modern reptiles! Of thousands of reptiles captured in the deserts, plains, and mountains of the United States, or in the cool cloud forests on the summits of old volcanoes in the American tropics, none has ever been found abroad and moving with a body temperature lower than 58° F. The one snake with this temperature was quite possibly caught off base the night we found it in the foothills of California, for air temperatures had suddenly descended as a storm moved in from the north.

Such amphibians as toads maintain the body at a mean level of 76° F., but temperatures like those of the tuatara are encountered among land-dwelling backboned animals only in salamanders living in spruce forests on mountaintops. Reptiles are a notch above the amphibians in the evolutionary scale, but perhaps the tuatara actually retains characteristics of its primitive moist-skinned ancestors.

Experiments carried out in New Zealand by R. D. D. Milligan[26] indicate that the tuatara has an extremely low metabolic rate, or "rate of living," lower than that of turtles or lizards. As a matter of fact, the chemical changes in the cells that provide the energy necessary for the vital processes within the body of the tuatara are evidently carried on at a considerably lower rate than in frogs at similar temperatures. One tuatara at a temperature of 48° F., manifestly warm enough to be active, showed no sign of breathing during an entire hour that Dr. Milligan watched it.

We need scarcely wonder that the tuatara can subsist on two snails or a few crickets per day, or that it spends most of its time motionless. It is this behavior (or the lack of it) that causes it to be described as sluggish. Nevertheless, it can and does move fairly rapidly for short distances.[25] We may well doubt that it is capable of any sustained activity, even less than most reptiles, and none of them compares with the mammals or birds on this score. The tuatara's low rate of living is doubtless coupled with its limited heat requirements, and it may have reached New Zealand at a time when reptiles not only tolerated but required low temperatures.

Turtles have their protective armor, and many reptiles rely upon their secretive habits, hiding to avoid the heat as well as their enemies. However, lizards that are active and living in open country during the day avail themselves of heat provided by the sun to raise the body temperature. Reptiles that regulate their body heat by basking are as warm-blooded as their avian or mammalian enemies—and move virtually as fast. True, they are forced to seek shelter when the sun fails to shine or when air temperatures drop, but since they are not using energy to keep the body warm, they require only a fraction of the food that a bird or a mammal must consume merely to remain alive.

As for the beakheads, if they were at all like the tuatara, they foraged at night. Adapted for this sort of existence, and perhaps with eyes modified for vision at night, they could not avail themselves of direct sunlight to raise the body temperature and increase their speed. When the mammals began to appear on the scene the beakheads were no match for them. So we can attribute the tua-

tara's survival in remote New Zealand to a combination of factors. The cool climate suited its low rate of living, and until historic times no mammals arrived to molest it.

What happened to the rest of the beakheads? We shall never know for certain. Nevertheless, Professor Cowles may be quite correct in his belief that increasingly higher temperatures contributed to their extinction as well as to that of the dinosaurs. In view of the fossil record it seems probable that the beakheads in other parts of the world preceded the dinosaurs into oblivion. It may have been not so much because they could not withstand the heat, but because they failed to avail themselves of its advantages. Had the beakheads been able to follow the course taken by the lizards, they might well have adopted a means of regulating their body temperature. With warmer bodies they would have been able to move fast enough to escape the mammals that began to appear. It was largely a matter of the tuatara's good fortune that the mammals arrived too late to catch the boat for New Zealand.

Perhaps the tuatara could be compared to the village centenarian. He may have no particular virtues, either physical or mental, but he has outlived all his former contemporaries—more than can be said of the poor old dodo, despite its acquisition of internal heating.

References

1. GRAY, J. E. *Zool. Misc.*, 13 (1831).
2. ———. *Ibid.*, 72 (1842).
3. GÜNTHER, A. *Phil. Trans. Roy. Soc. London*, **157**, 595 (1867).
4. OWEN, R. *Trans. Geol Soc. London*, **7**, 64 (1845).
5. CONRAD, G. M. *Natural Hist. Mag.*, **49**, 224 (1940).
6. DENDY, A. *Trans. Roy. Soc. London*, **B** 201, 227 (1911).
7. GASSON, W. *Optician*, **37**, 261 (1947).
8. SIMPSON, D. A. *Trans. Roy. Soc. S. Australia*, **73**, 1 (1949).
9. STEBBINS, R. C. *Ecol. Monographs*, **14**, 331 (1944).
10. CLAUSEN, H. J., and MOFSHIN, B. *J. Cellular Comp. Physiol.*, **14**, 29 (1939).
11. HOWES, G. B., and SWINNERTON, H. H. *Trans. Zool. Soc. London*, **16**, 1 (1903).
12. BEER, G. R. DE. *Proc. Linnean Soc. London, 161st Session* (1949).
13. ALI, S. M. *Proc. Indian Acad. Sci.*, **13**, 171 (1941).
14. FALLA, R. A. *Bull. Auckland Zool. Soc.*, (2), 3 (1935).
15. SIMPSON, G. G. *The Meaning of Evolution*. New Haven: Yale Univ. Press (1949).
16. ROMER, A. S. *Vertebrate Paleontology*, 2nd ed. Chicago: Univ. Chicago Press (1945).
17. MURPHY, R. C. *Proc. Am. Phil. Soc.*, **95**, 569 (1951).
18. SCHMIDT, K. P. *Fieldiana–Zool.* **34**, 1 (1952).
19. COWLES, R. B. *Science*, **90**, 465 (1939).
20. ———. *Am. Naturalist*, **74**, 542 (1940).
21. ———. *Ibid.*, **79**, 561 (1945).
22. ———. *Science*, **103** (1946).
23. COLBERT, E. H., COWLES, R. B., and BOGERT, C. M. *Bull. Am. Museum Nat. Hist.*, **86**, 327 (1946).
24. COWLES, R. B., and BOGERT, C. M. *Ibid.*, 83, 261 (1944).
25. DAWBIN, W. H. *Tuatara*, **2**, 91 (1949).
26. MILLIGAN, R. R. D. *Australasian Assoc. Adv. Sci.*, **16**, 404 (1923).

SCIENCE ON THE MARCH

POTATO INSECT PESTS IN THE BOLIVIAN ALTIPLANO*

ON THE Bolivian Altiplano, a high plateau 12,500 feet above the sea, extending for hundreds of miles between the eastern and western mountain ranges of the country, the potato continues to be grown in probably much the same manner as in ancient times. The Altiplano is one of its ancestral homes.

Fortunately some of the more serious insect pests that attack potatoes in Bolivia do not appear to have accompanied the crop when it was introduced into other parts of the world. To what extent these stay-at-home pests could become established elsewhere is a moot question. The chances are that they would be just as troublesome as in their native habitat. It is well to know their power and to strengthen, insofar as possible, all safeguards against their spread.

The insects that, according to the author's observation, caused most serious damage to potatoes on the Altiplano during the past season are listed in Table 1.†

Premnotrypes sp., commonly known as *gusano blanco*, is by far the most damaging to potatoes in this region. Its larvae, or grubs, invade 30 per cent or more of the tubers and make them unfit for any purpose except possibly as feed for livestock. A survey of potato fields in the Lake Titicaca vicinity during harvesttime in 1952 showed from 18 to 59 per cent of the tubers to be infested with the pest; average infestation was 34 per cent. Growers said their potatoes were damaged to about this extent every year. The pest is widespread in the region.

Usually there are 5–10 larvae in each infested tuber, but the number may be even higher. The writer noted 29 in a single potato, and one grower who had a heavily infested field had counted as many as 47 in one tuber. When the larvae have consumed the contents of one tuber, they invade others nearby.

This insect appears to have an annual generation or life cycle. After the potatoes have been harvested, heaped on the ground, and covered with dry grass for winter storage, the mature larvae either remain in the tubers or bore into the soil beneath, where they gradually complete their transformation in time to emerge as adults and lay their eggs on the next crop of potatoes.

Growers endeavor to cull the damaged tubers before marketing the crop, although doing so involves a great deal of hand labor. Even so, many damaged tubers escape notice, and the consumer must remove the larvae before cooking the potatoes.

At maturity the larva is white and stout-bodied, with a brown head, and is about 8 mm long. The adult is a dark-brown, stout-bodied beetle, about 6 mm long.

TABLE 1

Order	Family	Species	Common Name
Coleoptera	Curculionidae	*Premnotrypes* nr. *latithorax* (Pierce)	*Gusano blanco*
Lepidoptera	Gelechiidae	*Gnorimoschema operculella* (Zeller)	Potato tuber worm
Lepidoptera	Phalaenidae	*Copitarsia consueta* (Wlk.)	*Ticona*
Diptera	Anthomyiidae	*Hylemya cilicrura* (Rond.)	Seed-corn maggot
Thysanoptera	Thripidae	*Frankliniella tuberosi* Mlt.	Thrips

Indian farmers place layers of a native mintlike plant called *khoa* in the winter storage heaps of potatoes, on the assumption that it drives the larvae out of the tubers. Arnaldo Sanjines A. de Leon and Julio Rea, of the Servicio Agrícola Interamericano Experiment Station near Achacachi, and Gordon Barbour, a prominent grower near Huatajata, express the belief that the practice merely serves to console the farmer and has little

* A contribution from the Servicio Agrícola Interamericano, a technical agricultural service organization for Bolivia, operated jointly by the government of Bolivia, the Office of Foreign Agricultural Relations, USDA, and the Institute of Inter-American Affairs. U. S. participation in this work was carried out as part of the Point IV Program in Bolivia, administered by the Technical Cooperation Administration, U. S. Department of State.

† Appreciation is expressed to the Division of Insect Identification of the U. S. Bureau of Entomology and Plant Quarantine, Washington, D. C., for insect identifications, and to Bolivian technicians Julio Rea and Walter Rodriguez for assistance in this study.

Two potato tubers cut open to show injury caused by *gusano blanco* (*Premnotrypes* sp.). (Photo by Frank J. Shideler.)

practical value. Many of the maturing larvae normally leave the tubers at harvesttime or during storage, and the device probably does very little to speed their departure.

Copitarsia consueta (Wlk.), referred to by the local growers as *ticona,* is another serious pest in some years. At times it causes damage not only to potato tubers but also to quinua (*Chenopodium quinoa*), another important food crop of the Altiplano.

The larva of ticona sometimes resembles the common garden type of cutworm, both in size and in appearance; the moth is mottled-brown and robust, and has a wing expanse of about 45 mm. The larva burrows deep into the ground to gouge the tubers, thus making the potato unfit for table use. Usually only one larva is found in a single tuber.

Among other potato pests are the adults of the seed-corn maggot *Hylemya cilicrura* (Rond.). These were collected in fairly large numbers with an insect net during the 1952 growing season, but at the close of the season only the maggots were to be found. The maggots were present in large numbers in potato "fruits" that were still clinging to the frozen vines or had dropped to the ground. Apparently the pest survives the winter in this manner. In cool, backward seasons these maggots cause damage to newly planted seed potatoes.

Thrips, *Frankliniella tuberosi* Mlt., do their damage by feeding on the foliage of the potato plant. During the early part of the past growing

Much of the cultivation carried on by the Indian farmers of the Bolivian Altiplano is done with primitive hand tools. (Photo by Robert O. Blodgett.)

The author inspects potatoes heaped on the ground with protecting cover of dry grass for winter storage. (Photo by Frank Shideler.)

season the orange-colored nymphs were most abundant, but as the season advanced the dark-colored adults predominated. These tiny insects feed by piercing and rasping the leaf tissue, and the injured foliage takes on a rusty or browned appearance. The thrips were abundant also on the potato fruits after the first killing frosts, and it is probable that they spend their dormant period there. One of the large growers in the Titicaca region, who has controlled these thrips by a well-timed spray of DDT—at the rate of 3 lbs. DDT/hectare—said that when the insects are uncontrolled they can cause a 20 per cent reduction in tuber yields.

The foregoing pests constitute the main insect problems of potato growers in the Bolivian Altiplano. Of these, the seed-corn maggot (of European origin) and the potato tuber worm are widespread in the world. Only the maintenance of adequately enforced quarantines, based on sound biological principles, can halt the dispersal of the others. With our modern rapid means of transportation, the danger of spreading these and other agricultural pests is becoming ever greater. Already there is a long list of destructive insects that are firmly entrenched in the Americas—insects that were unknown there less than 100 years ago.

J. Alex Munro‡

Servicio Agrícola Interamericano
La Paz, Bolivia

‡ Entomologist, Office of Foreign Agricultural Relations; on leave of absence from the North Dakota Agricultural College, Fargo.

ECONOMY OF DOUGLAS FIR IN THE PACIFIC NORTHWEST*

ECOLOGICALLY speaking, forest economics is concerned with the relationships between forests and human beings. The economics of Douglas fir, then, consists of two phases: the influence of *Homo sapiens* L. on Douglas fir (*Pseudotsuga taxifolia* [Poir.] Britton), and the influence of the species Douglas fir on man.

As to the first phase, a great deal has been said about how man has been the worst enemy of the Douglas fir forest, through fires, destructive logging, and clearing of steep or poor land for agriculture. Fortunately, human beings are beginning to learn to work with nature in the management of Douglas fir forests. No more need be said now on this phase of the economics of Douglas fir.

Concerning the second phase, it should first be pointed out that Douglas fir has significance or value to man only through the satisfactions, products, and services that it yields or is capable of yielding.

Douglas fir, as a species, as a forest, or as a material, means different things to different people. It is variously considered as an obstacle to agriculture (a stubborn type of woody vegetation occupying land needed for growing field crops or for pasturing livestock), a raw material for industry, a useful (and at times exasperating) building material, a base for employment, a tax base, a resource to be depleted, developed, or conserved, a forest furnishing a habitat for fish and game, or an environment for camping, hiking, picnicking, and communing with nature. To many, the Douglas fir forest is, in the fullest sense, a genuine source of

* For editorial consistency in a general scientific journal, the editors must follow general usage—specifically Webster—rather than the specialized terminology of the several scientific fields. They are aware of the fact that foresters have officially introduced a hyphen in Douglas fir to distinguish it from the true fir (*Abies*).

Poles and pulpwood are removed in stand-improvement thinnings from a managed second-growth Douglas fir forest, Crown Zellerbach's Columbia Tree Farm, Oregon. (Photo by Harold M. Brown.)

inspiration and of recreation. Those who are concerned with flood prevention, regulation of stream flow, the water supply, or soil and water conservation will be interested in Douglas fir as an important component of the ground cover, to some extent, in every state west of the Great Plains.

Detailed consideration will be limited here to the timber resource aspects of Douglas fir and to some problems of the business of growing, harvesting, and utilizing crops of Douglas fir trees. Although the Douglas fir region includes much of British Columbia and a portion of the Coast Range of northern California, the area we shall refer to as "the Douglas fir region" will be only that portion of the states of Washington and Oregon lying west of the summit of the Cascade Mountains.

Importance of Douglas Fir to the Economy of the Pacific Northwest. Eighty-three per cent of the land lying between the Pacific Ocean and the summit of the Cascades is forested or is chiefly suited to forestry and not otherwise used.[1] Were it not for the large agricultural areas in the Willamette Valley and the Puget Trough, the forest land would be 89 per cent of the total. On the basis of area alone, therefore, forestry is at least fully the equal of agriculture in economic importance to the Douglas fir region.

After excluding all forest land not capable of producing timber of commercial quality and all land withdrawn for special uses, such as national parks and wilderness areas, there remain 26 million acres of commercial forest land which comprise 77 per cent of the total land area of western Washington and 70 per cent of the total land area of western Oregon.[1] Sixty-one per cent of the sawtimber volume of the region is of the one species, Douglas fir.[1]

In 1950, forest products provided 57 per cent of all the freight carloadings in Washington and Oregon.[2] Each year the forest industries of the Douglas fir region make up about 60 per cent of the industrial payroll of the area.

Dependence of the United States on Forests of the Douglas Fir Region. Since World War I, the

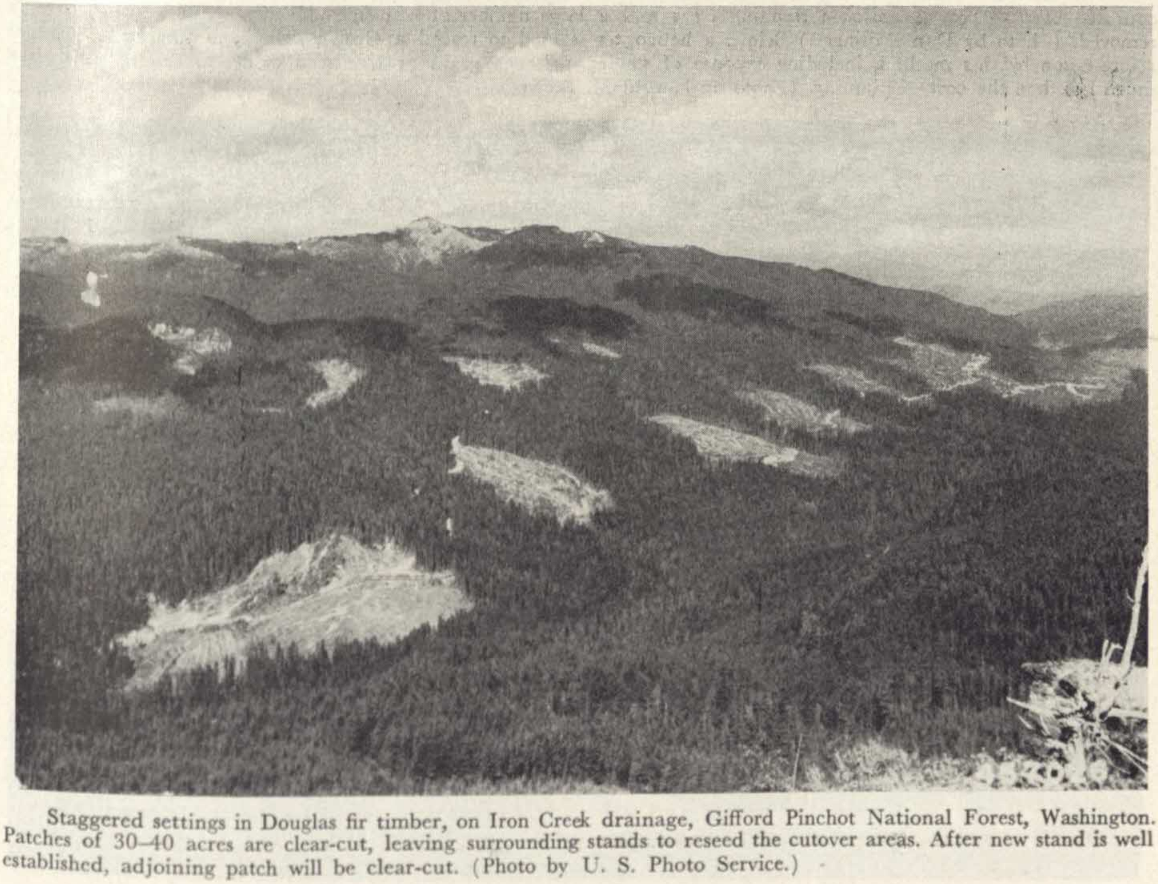
Pacific Northwest has been the leading producer of lumber in the United States. Washington was the leading state until 1938, when Oregon assumed the leading position. In the past few years California has edged out Washington for the second position in national lumber production, a change which was due in part to a greatly accelerated rate of cutting in California's Douglas fir region since 1947. Eighty per cent of the annual cut from Oregon and Washington is from the Douglas fir region.[1]

In the year 1950, 10.7 billion board feet of lumber, or 31 per cent of the nation's total production, came from the Douglas fir region; of this amount, 10.6 billion board feet were softwood lumber, constituting 39 per cent of the nation's total softwood production.[1]

In 1950, of the lumber produced in the Douglas fir region of Washington and Oregon, only 19.5 per cent was marketed in these two states; 3.0 per cent was exported, and 77.5 per cent went to other states. Some of the details on the shipments to other states are significant: 9.4 per cent went to California by rail and truck, 23.3 per cent went to various states by water (mostly through the Panama Canal), and 44.8 per cent went by rail to other states, principally in the Midwest and Northeast.[3] In the same year the Douglas fir region of Washington and Oregon produced at least 70 per cent of the nation's veneer and plywood, and at least 16 per cent of the wood pulp.[4]

The Douglas fir region of Washington and Oregon, in 1944, had only 5.6 per cent of the commercial forest area of the United States, but 32 per cent of the nation's volume of standing timber, and 14 per cent of the country's potential growth capacity.[1] (The entire eastern half of the United States has 77 per cent of the commercial forest area, but only 35 per cent of the volume. The Southeast alone has 40 per cent of the area, 21 per cent of the volume, and half of the U. S. growth capacity; so it is an important competitor of the Douglas fir region.)

Forest Conditions in the Douglas Fir Region. In western Washington and Oregon the land now forested consists mainly of land that is too rough, too steep, or otherwise not well suited to agriculture or other higher use. Except in the flatter por-



Staggered settings in Douglas fir timber, on Iron Creek drainage, Gifford Pinchot National Forest, Washington. Patches of 30–40 acres are clear-cut, leaving surrounding stands to reseed the cutover areas. After new stand is well established, adjoining patch will be clear-cut. (Photo by U. S. Photo Service.)

After a severe windstorm, this entire area on Crown Zellerbach Corporation's Clackamas Tree Farm in Oregon consisted of scattered and broken standing trees and a large number of windfalls. All merchantable wood is being removed. (Photo by Don Baisinger.) Below, a helicopter is used to reseed a Crown Zellerbach tree farm. Cost of reforestation by this method, including expense of sowing poisoned grain or tree seed to destroy rodents, is often much less than the cost of planting. (Photo by Harold M. Brown.)

tions of the Willamette Valley there is very little land that is likely to shift from forest to agricultural use. On the moderately steep slopes of hills and in shoestring valleys there are many tracts where there is some question as to whether the best use would be grazing or timber growing. Such areas need further study.

One of the least satisfactory things about forest conditions in the Douglas fir region is the ownership pattern. According to P. L. Buttrick,[5]

> The prize example of a complicated and uneconomic forest ownership pattern is found in western Oregon, where a great number of small holdings and a small number of very large ones are mixed indiscriminately with both large and small public holdings under four distinct administrations, and all boundaries are based on a rectangular survey having no relation to topography.

In Washington the patchwork pattern is simplified only by the absence of revested railroad grant lands and the presence of a little more consolidation. Needless to say, such a pattern as that described by Buttrick results in numerous problems of access, timber trespass, and administration.

In western Oregon most of the federally-owned forest lands are administered by the Forest Service of the Department of Agriculture; but part of them, often intermingled with or actually overlapping Forest Service lands, are under the jurisdiction of the Bureau of Land Management of the Department of the Interior. Neither agency receives appropriations for local units that are directly proportional to the amount of timber sales activity.

The ownership of commercial forest land in the Douglas fir region, as of January 1, 1945, is shown in Table 1.

TABLE 1

PERCENTAGE OF OWNERSHIP OF COMMERCIAL FOREST LAND IN THE DOUGLAS FIR REGION

Federal and Indian			
National Forest	29.5		
Indian	0.9		
O & C, revested, etc.	7.8		
Unreserved public domain	0.9	39.1	
State, county, and city		10.1	
Total public			49.2
Private			
Large (over 50,000 acres)	14.2		
Medium (5000–50,000 acres)	6.7		
Small (0–5000 acres)	29.9		
Total private			50.8

Approximately one fourth of the land in small ownerships is in farms; much of the remainder is absentee-owned.

The present forest management of lands owned by the public and by large private companies is mostly good. The management on the small private holdings is mostly poor and destructive; yet much of the potentially most productive land is in these small holdings. Here is the heart of the forest problem.

The cover condition of the Douglas fir region and the total volume of standing timber are shown in Table 2.

TABLE 2

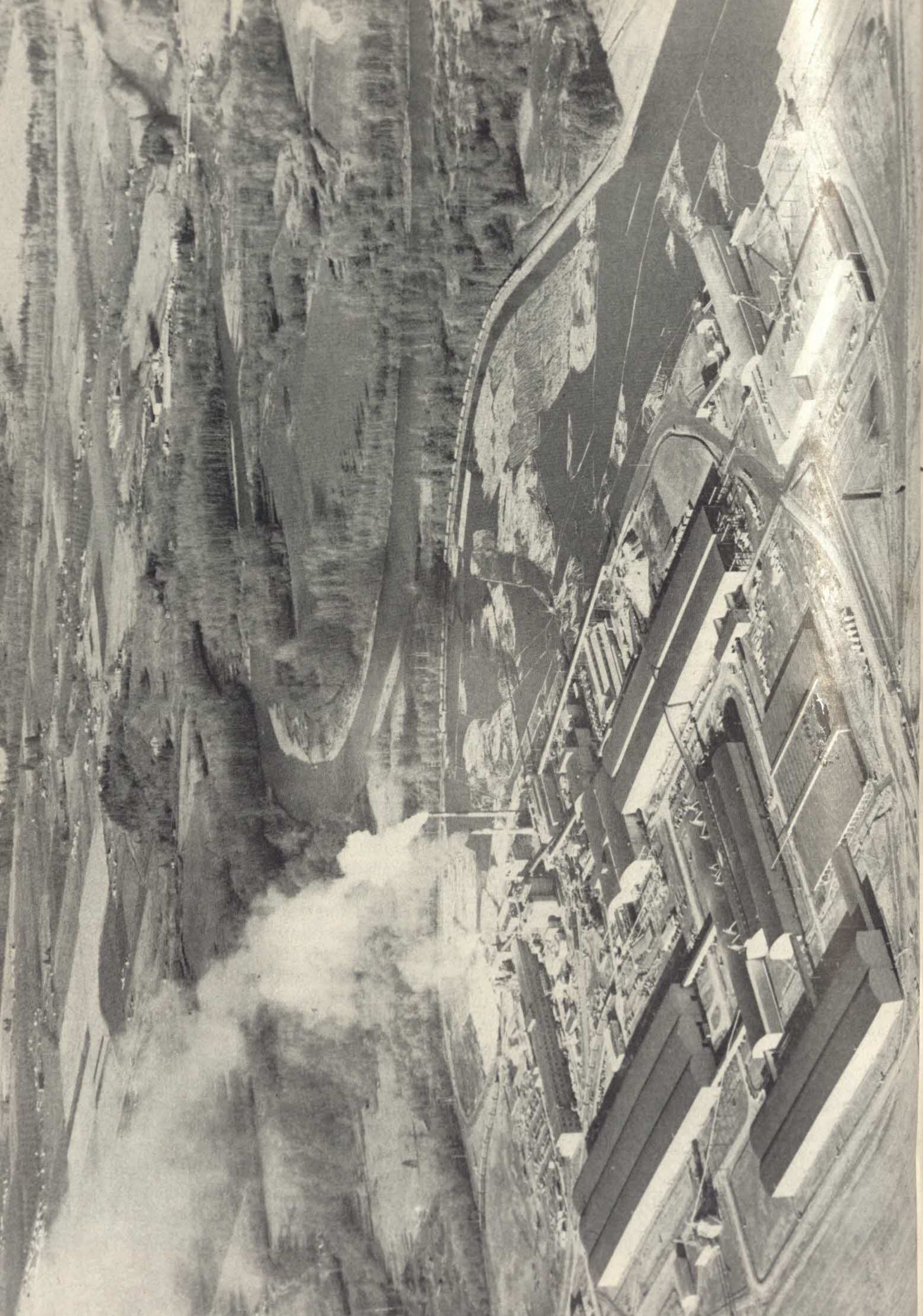
COMMERCIAL FOREST AREA (AND VOLUME) BY COVER CONDITION AND OWNERSHIP, DOUGLAS FIR REGION

(Areas in millions of acres; volumes in billions of board feet, log scale)

	Public		Private		Totals
Condition	Washington	Oregon	Washington	Oregon	Douglas fir Region
Virgin	2.2	4.1	1.3	1.7	9.3
Second growth	0.5	1.4	0.7	1.1	3.7
Poles	0.5	1.3	1.4	1.3	4.5
Seedlings	0.7	0.6	1.2	0.5	3.0
Denuded	0.6	1.0	2.1	1.8	5.5
Totals	4.5	8.4	6.7	6.4	26.0
(All conditions	97.3	164.4	80.5	96.8	449.0 billion board feet

In the Douglas fir region there is a significant difference between Washington and Oregon in species composition. In Washington, although 70 per cent of the volume cut each year is Douglas fir, this species makes up only 37 per cent of the standing timber; the volume of Western hemlock almost equals that of Douglas fir, and the remaining volume includes significant proportions of true firs, Western red cedar, and Sitka spruce. In Oregon, 85 per cent of the volume cut each year is Douglas fir, which constitutes 78 per cent of the standing timber, with only minor amounts of hemlock and other species. Because of the large volume of hemlock and true firs in Washington, the pulp and paper industry is much more important in Washington than in Oregon.

The only process by which Douglas fir is pulped is the sulfate (or Kraft) process, and so far there are only a very small number of sulfate mills in the region. These include one at Springfield, Oregon, three on the Columbia River, and two on Puget Sound. A sulfate mill, fiberboard plant, or chipping plant (which ships to a pulp mill or board plant) greatly increases the possibility of close



utilization of mill waste, and makes profitable the use of logs of smaller sizes and poorer quality than would otherwise be possible. Several developments along this line have given a considerable impetus to forest utilization, and thus to forest management, during the past few years.

The so-called forestry balance sheet is a comparison between growth and drain. "Drain" includes wood used, wasted, and lost to insects, disease, wind, or fire. In the Douglas fir region the 1944 growth was only 3.7 billion board feet as compared to a drain of 12.0 billion board feet. The growth goal set for the region by the U. S. Forest Service estimate is 10.0 billion board feet. This goal can be achieved in fifty years if reasonable progress is made toward improving forest management by most forest owners. The potential growth in the region under moderately intensive management would be 12.6 billion board feet, or more than the current drain. Until the decadent old-growth stands of the Douglas fir region are cut, they will annually decrease in volume. It is only when they are replaced by vigorous young stands that the region's growth can balance decay and other drain. The federal access-road program (a self-reimbursing subsidy) is intended to open up the now economically inaccessible areas and speed the utilization and salvage of the old-growth and decadent timber, and to reduce the pressure for cutting the immature stands.

The forest economy of the Douglas fir region is now in a rapid transition from "mining" timber to "tree-farming." The concept of the forest resource as static or as a store of forest wealth is giving way to the concept of the forest resource as dynamic, as a wealth of growth-capacity.

The Business of Douglas Fir Forestry. In forestry many possible practices are desirable and feasible from a biological or engineering point of view, but impractical or undesirable when an economic or financial test is applied. An example is in the silvicultural operation of pruning. An open-grown stand of rather limby 25-year-old Douglas fir timber is to be harvested at age 65. If these trees are ever to produce clear lumber, immediate pruning will be necessary. If selected crop trees are pruned for 18 feet of their length at a cost of $30 per acre, the pruning will result in an estimated increase in value, at the time of harvest, of $200 per acre (in this hypothetical case). Investing $30 and getting $200 in return would seem like a splendid investment, but the time and interest factors have been disregarded. If the money to pay for the pruning must be borrowed at 6 per cent interest, with both principal and interest to be repaid at the time of harvest, then the amount to be paid will be $30 accumulated for 40 years at 6 per cent compound interest, which will amount to $309. So the pruning is physically feasible and aesthetically desirable, but economically impossible. Or if, instead, the owner could use his own money either to finance the pruning or to put in an alternative investment to earn 5 per cent interest, his $30 per acre investment in pruning would have to return $211 in order to pay as well as the alternative investment.

This illustrates the necessity in private forestry of (1) keeping costs less than gross earnings and (2) making only those investments which give promise of returns commensurate with the amount invested. The investor or businessman putting his money into a forestry enterprise should be concerned with the amount of money he must invest or leave invested, and the rate of return on that investment. Choice between alternative plans of management must usually be based on comparing the estimated long-run financial results of each, allowing for differences in time by applying time discounts, and choosing the alternative that promises to yield the highest rate of return per dollar invested, or at least a satisfactory rate of return, together with other benefits.

Another example of the difference between financial and physical considerations is in the choice of a rotation (number of years to grow a crop of trees under sustained-yield management). At present, in the Douglas fir region most rotations, particularly on public forests, are chosen on the basis of a number of practical considerations, including condition of the timber and time required for opening up the area. Many foresters give some attention to the age to which trees should be grown to produce the greatest possible average annual volume growth; this might occur with rotations varying from 80 to 110 years, depending on site quality and basis of measurement. If a financial version of this method were used, an attempt might be made to choose the rotation that would produce the greatest growth in value per acre per year (or the greatest net annual earnings per acre); this method would result in rotations such as 150–170 years. Yet private forest owners think in terms of 60–90 years, most of them not seriously considering longer rotations. This dif-

---

The Weyerhaeuser Timber Company plant, Springfield, Oregon. A sawmill, a pulp and paper mill, a plywood plant, and a Pres-to-Log plant, fully integrated, make complete utilization of Douglas fir and wood of other conifers possible. Even material from defective logs is used. (Photo by Valley Flying Service, Inc.)

ference results from a desire for a good rate of return on investment, so as to maximize the total net earnings per year. With a rotation of 150–170 years, the amount of money invested in growing stock (timber) and land would be so great that only a very low rate of return on the investment could be realized. Only a forest owner who can disregard investment can afford such a rotation in a Douglas fir forest.

Many forest investments, such as immature timber or stands still economically inaccessible, are deferred-yield propositions, which are singularly unattractive to most investors, partly because they are long-term investments—although with some possibility of getting the money out by resale—but mainly because of the large, practically uninsurable risks involved. These risks include (1) physical loss or destruction by fire, insects, disease, wind, etc., and (2) market risk and risk of higher costs (higher wages or higher taxes). Any investor who assumes these risks must allow for them in the price he pays or the return he demands.

At the present time it is practically impossible to buy insurance on standing timber in the Douglas fir region at a reasonable rate. An encouraging indication is that several private insurance companies are now said to be seriously considering underwriting this type of risk.

Another difficulty in financing deferred-yield forestry enterprises is the fact that banks that are members of the Federal Reserve System are not allowed to lend money secured by forest land and standing timber because of the ruling that forest land is unimproved real estate and hence not allowable for security on loans. Practically speaking, the only long-term forest credit available is from purely personal sources. Banks will lend money on short-term loans secured by felled timber in cold decks, or secured by accounts receivable. But under no circumstances will a bank even consider a forest loan for a period longer than 10 years, which is a short term by forestry standards.

Federal legislation has been proposed by forest economists and agricultural economists to create a Forest Credit Division in the Farm Credit Administration, to make long-term loans on a business basis to forest owners, with low rates of interest and conditions of repayment tailored to fit the needs of the borrower; but such legislation has not been enacted into law.

Forest taxation in the Douglas fir region was, a few years ago, only a minor problem to forest owners; but now that federal income taxes on corporations and individuals have become almost confiscatory, and general property taxes on forest land are increasing in this region, taxation has become a major problem. Because of the economic necessity of paying only the smallest possible legal federal income tax, many companies find that they must choose their management practices on the basis of the effect on the amount of income tax payment, rather than on the basis of long-term profits, where taxes are considered on the same basis as other costs. The taxation of immature timber as real (or personal) property has a strong tendency to encourage liquidation of the timber whenever the tax levy become sizable. Many forest owners believe, therefore, that the entire basis of forest taxation might be reconsidered from the standpoint of its effect on forest conservation.

Contrary to the situation in other industries, most operators of sawmills and of logging operations do not know exactly how much money they make or lose on trees or logs of different sizes, quality, or distances from the mill; hence they have only an approximate idea of their economic margins and of the factors causing variations in their costs. This is because of the extreme variability among the different logs and trees encountered and in the logging and hauling conditions. In order to obtain an accurate knowledge of costs and margins it is necessary to make a time-and-cost study for each major variation in mill and woods conditions.

Like most businesses, forestry enterprises are strongly affected by variations in price and demand related to business cycles. This is less true of pulp companies than of lumber companies. During a general business depression lumber prices tend to be so low that very careful planning and management, even during periods of good prices, are necessary for a lumber company to ride safely through a major depression. Such planning will include (1) keeping the mill capacity small to avoid heavy carrying charges during shutdowns or periods of low activity; (2) cutting the decadent, low-quality, remote, and high-cost stands during periods of good prices; and (3) leaving some accessible stands of good-quality, low-cost timber for cutting during periods of low prices.

In looking over a clear-cut area in the Douglas fir region, one is frequently dismayed at the large amount of usable wood left on the ground; and some might even indulge in unkind thoughts about those who did the logging. These thoughts may be justified if the material left on the ground includes logs or trees of all sizes and qualities, as evidence of careless logging. But if there is evidence of the logger's having attempted to adhere to some standard

as to minimum diameter and minimum quality, perhaps there is something to be said in his behalf.

Only the most rabid wood conservationist would argue that a logger, forced by topography to use high-lead or other cable method of yarding, should take every last stick felled or knocked down, even those which would not pay their way to and through the mill. Any such logging of submarginal logs could be required only as a definite expenditure for reducing insect or fire hazard or for other silvicultural purpose.

Even if it can be shown that the logger could have earned 10 or 20 cents per thousand board feet by taking some of the logs left on the ground, the logger may have a good argument. What was he trying to conserve? Wood? Labor? Land? Capital? If he was trying to conserve labor and capital by not wasting time and money on trees or logs that could net only a few cents, when the same amount of capital and labor could have been applied so as to produce net earnings of 50 cents or $1.00 or more per thousand, he was conserving resources just as important as wood.

We tend to forget that a type of conservation that is making most complete or efficient use of one resource may be using other resources quite wastefully. It is virtually impossible to obtain the most efficient use of each of several resources simultaneously. Proper conservation policy would be to use efficiently those resources that are most scarce or most expensive. The other resources should then be used with care, but are bound to be used at less than their own maximum efficiency.

What about the economic future of Douglas fir and the Douglas fir region? Predictions are dangerous and should be qualified. Three things, however, can be said with assurance: (1) Present trends toward closer utilization of Douglas fir are encouraging and are likely to continue. Even Douglas fir bark will become a valuable chemical raw material, yielding wax, tannin, and other extractives. (2) The progress made in the management of Douglas fir forests during the past decade has been amazing; further improvement may be expected. (3) The population of the Pacific states is likely to continue to increase for several decades, thus increasing the local demand for forest products.

Two fields in forestry in the Douglas fir region need further study and development: (1) the financial and management problems of the small private forest; and (2) the economics of public forestry and forest administration, including the further application of the analysis of costs and benefits and of time discounts to contemplated public projects, and the removal by Congress of legal and financial obstacles to the efficient business administration of local units of federal forestry agencies.

ROBERT F. KENISTON
School of Forestry, Oregon State College

References

1. U. S. Forest Service, 1944 reappraisal data.
2. INTERSTATE COMMERCE COMMISSION. *Preliminary Report for 1950.*
3. *1949–1950 Statistical Year Book.* Portland, Ore.: West Coast Lumbermen's Association (1951).
4. GOLDHAMMER, B. Paper presented before the American Veterans' Committee, Portland, Ore. (Nov. 2, 1949).
5. BUTTRICK, P. L. *Forest Economics and Finance.* New York: Wiley (1943).

BOOK REVIEWS

ELEMENTIST GOING UP

The Sensory Order: An Inquiry into the Foundations of Theoretical Psychology. F. A. Hayek. With an introduction by Heinrich Klüver. Chicago: University of Chicago Press, 1952. xxiii + 209 pp. $5.00.

IF YOU are going to write a theoretical psychology, you can start at the top with psychological events as they occur in vivo and work down to some elements (like sensation, nerve impulses, reflexes) at the bottom, or you can start at the bottom and work up. In either case you count as an elementist. If you are convinced that analysis of that sort distorts nature, you can go over to field theory or some sort of wholism, and still you can go *von oben nach unten* or the other way. Hayek is an elementist going up, so three fourths of the other theorists will dissent from his propositions unless the cogency of his argument makes converts of them. The human mind is, however, scarcely so rational as to make that miracle probable. If Hayek wrote to gain disciples, he had better have written more persuasively and less precisely, for one can get bored by the deadly logical progression of this book long before he can find a cogent phrase to which he dares register vigorous dissent.

Hayek is talking science—that is to say, he is dealing solely with generalized objects, not the stone that Dr. Johnson kicked but the constructs that make up knowledge. So he has (1) the world of stimuli, where you meet events like radiation with a given wavelength, not such stimulus objects as you buy from a catalog and put in an apparatus case. And then he has (2) the neural world of nerve impulses, and they too, mind you, are constructs, generalized events inferred from observation of electrical potentials. (Hayek dodges the problem of operational definition by taking a firm stand *ab initio* among the hypothesized constructs.) The third world (3) is psychological or mental, where the elements are sensations. Literally, sensations are constructs, too, not at all bits of actual or private experience. Nor has Hayek any use for phenomenology or positivism in science, although the sort of positivism he is eschewing is more the experiential Ernst-Mach kind than what the Vienna Circle made Mach's views over into.

The psychological events are constituted by a large class of hierarchical neural relationships, only a small portion of which are conscious. The rest of them are unconscious, preconscious, or potentially conscious. That is all right, for it is exactly what ought to follow from the conception of mental events as constructs and the author's further belief that the stimuli, the neural impulses, and the sensations are not in different worlds but fundamentally all of the same kind of "stuff." Is Hayek a physicalist? He'd not like to be called one, for then he might be thought a behaviorist (much too narrow for him are those exponents of an empty organism) or a positivist.

As you work from below up, you must resort to some principles of association to synthesize the elements into larger and larger wholes. Hayek's word for synthesis is *classification*. And his principle of classification (association) is frequency of temporal contiguity, that patient wheel horse of the psychological band wagon since Hume and, in a way, since Aristotle. Right there lies my first major dissent. Hayek ought not to rest his whole structure on this disputed principle. It is true, of course, that frequent occurrence of association tends to be followed by recurrence of the associated terms, but Hayek is not talking at this naïve level. He and we need to know what is the physical nature of connection. For the moment, Hayek sounds like an operationist. All he has said is that frequent concurrences tend to recur; yet we know that they do not always recur, and also that frequency is not a necessary condition for recurrence. Moreover, as Hayek himself says elsewhere, these events need to be fitted into space as well as into time; so when the connections are made, whether it is by frequent concurrence or otherwise, you still need to think of them as somewhere. (Hayek is not thinking of any such controversial connectors as synapses; he is dealing with an ideal nervous system, one like Avenarius' System C, an als-ob nervous system.) You see, the number of combinations of a thousand things taken any number at a time is 2^{1000}, which is ever so much greater than the total number of electrons in the universe (10^{300} *vs.* 10^{70}), and, although Hayek would, of course, not expect every combination to form a class in the sense that it had a unitary effect, still the problem of getting a common effect for so many groups of fibers, whose only community is temporal and not spatial contiguity, is great. Where would all the connections be?

The concept of the formation of classes (larger and larger wholes) is Hayek's way of dealing with generalization, and thus with equivalence of stimuli and with subsequent psychological events like abstraction and transfer. He might have modernized his speech and have talked about transmission of information, and then generalization would have become positive entropy, and the reinforcement of potential sensations negative entropy. But to everyone his semantic taste. Hayek's psychological system is one of smaller classes being grouped into larger, with all the possible cases of overlap independently functional. Thus you start with sensory elements and you get on by classification to larger and larger sensory structures by means of classification and both further classification and reclassification.

Every psychological system needs a dynamic principle in addition to the associative one—set, determining tendency, need, attitude, drive. This principle is superordinate to the basic associations and seems not often to

be thought of as depending on past frequency for its strength. Hayek recognizes this essential, but he reaches his goal by more classification—one class of sensory phenomena reinforces another that has been but potential so far as its having any effect goes, thus making it effective by reinforcement. This is the way set was often supposed to work before Ach (1905), and perhaps we may come back to it. Hayek does not convince me, though. His logic gets thinner the further it elevates us in the system.

Eventually Hayek deals with such concepts as consciousness, attention, and conceptual thought, but here he seems to me naïve. You do not need consciousness at all in a system of this sort. Why lug it in? Just to prove that the system leaves nothing out? Well, then set up some criteria for consciousness, and let them reflect a knowledge of the discussion of these criteria that has gone on for so many years. One would think from reading this chapter that Hayek believes that private experience belongs in science. You have to keep nudging yourself and saying: "Remember, consciousness is physical, a physical relationship."

And that point troubles me all through this discussion. Hayek talks like a mind-body dualist and yet presently insists that he is not. Such a seeming contradiction is logically possible, if you know the transformation formulas from sensory quale to neural relationship, but it is misleading to persons who have been brought up to believe that they can know about private consciousness that has never been published. They think they have private, absolute awareness of the sort of stuff that Hayek is considering. But he is not being phenomenological. He is considering neural relationships which are wholes because they have single common effects that in combination have their own joint later common effects, and so on. All this makes sense, but it is not common sense.

The book is remarkably tight. It is full of careful logical sense. It gets harder as you pass from the simple to the complex, partly because you are farther from your base, and partly because of this interference between logical consequence and common-sense phenomenology. Hayek is a Viennese economist, long resident in London, and a British citizen, now at Chicago, where he has fouhd stimulus and understanding in Heinrich Klüver. Half the time I read with amazement at the extent of his reading and comprehension in a field that is not his *Fach*. The other half I tear my hair at his lack of historical orientation in psychology. Even when he is right (and that, I should say, is most of the time), you wish he would do a reasonable share of the work in connecting up his thought with that of his predecessors. Physical theories of mind and consciousness, and relational theories, are not new, and one would like to be shown, not merely the content of Hayek's mind, but his theory in the perspective of the history of scientific thought about these matters. Nevertheless, let me add, to my reminder that Hayek's views have antecedents, that I feel sure that no one has done this particular kind of a job nearly so well. It is a physicalistic system of psychology, mind, and (if the word must be used) consciousness. It is not absolutist in any sense; the facts in it are relations and the system is not closed except, for convenience, by the skin of the organism. It is elementistic and it makes sensations, which are physical constructs and not phenomenal bits of consciousness, basic to the structures which it builds. Thus it can be physicalistic and not primarily behavioristic. I do not for a moment believe it is the last word on this matter, but it is one word, and the best word I have ever heard spoken from this platform.

EDWIN G. BORING
Psychological Laboratories, Harvard University

HOMOLOGY AND COHOMOLOGY

Foundations of Algebraic Topology. Samuel Eilenberg and Norman Steenrod. Princeton, N. J.: Princeton University Press, 1952. xv + 328 pp. $7.50.

THE greater part of the algebraic topology is concerned with the concepts of homology and cohomology, which owe their popularity to Poincaré's attempts to classify manifolds in terms of a calculable set of invariants. Certain groups, called homology and cohomology groups, are associated with topological spaces, and homeomorphic spaces have isomorphic groups. Thus they provide a set of invariants, which in certain cases can be calculated; but they are insufficient, except in the case of compact two-dimensional manifolds, to solve Poincaré's problem, for isomorphism of the groups does not necessarily imply homeomorphism of the corresponding spaces. This indicates that some part of the original structure which defines the topology has been lost in the transition to the groups. Despite this, a study of homology and cohomology reveals much beautiful and interesting mathematics, and there are many applications. One important feature is that topological problems are transformed into algebraic ones, which are usually easier to solve.

In this book the authors have attempted to place homology and cohomology on a firm basis by means of an axiomatic treatment. This differs from previous axiomatizations of parts of homology theory in that the transition from spaces to homology groups is subjected to axioms, which has never before been attempted. Full accounts of the more familiar homology theories are given, and they are discussed in the light of the axioms. A second volume on further aspects of the subject is in preparation.

Chapter I begins with certain topological and algebraic preliminaries, but some knowledge of the basic concepts of topology and group theory is assumed. The axioms for homology are then stated, followed by the dual axioms for cohomology. Various general theorems are proved, in particular the invariance theorem, and the chapter ends with theorems on the homology of Euclidean spaces. Chapters II and III are concerned with simplicial complexes, which belong to the oldest and probably most familiar part of algebraic topology. The necessary definitions and geometric details are

given in Chapter II, and in Chapter III homology on simplicial complexes is discussed. The main result is that two homology theories, with isomorphic coefficient groups, on the category of triangulable pairs, are themselves isomorphic. The ideas of "category" and "functor" are explained in Chapter IV. These place certain concepts on a formal basis, and avoid a considerable amount of repetition in the subsequent chapters. The ideas appear to have been of importance in the development of the book. Chain complexes are discussed in Chapter V, and in Chapter VI the classical homology theory of simplicial complexes is presented. Chapter VII is devoted to singular homology theory, and it is here that the existence of nontrivial homology theories is first established. Systems of groups and their limits are discussed in Chapter VIII, which leads up to the Cech homology theory in Chapters IX and X. In the final chapter, some applications are given, notably the Brouwer fixed point theorem and the fundamental theorem of algebra. Most of the chapters are concluded with notes and exercises, the former being partly on historical aspects and partly on the subject matter.

The book will, of course, appeal more to topologists and algebraists than to anyone else. The newcomer to algebraic topology will probably be confused at first. The axioms, coming near the beginning of the book, without previous explanation, place a burden on the uninformed reader, who has to accept them without knowing how they came about. This, of course, is unavoidable without a considerable amount of trouble, and only a slight knowledge of algebraic topology is necessary for an appreciation of the axioms.

The presentation throughout is clear and precise; definitions are always given where they are necessary, and no doubt should be left in the reader's mind as to what is being proved and how it is being done. One significant feature of the book is the extensive use of diagrams to illustrate various definitions and the proofs of certain theorems. These diagrams, which consist of networks in which the vertices represent groups and the edges represent homomorphisms, are extremely useful, and assist greatly in the understanding of the situation.

E. M. Patterson
Department of Mathematics, United College
University of St. Andrews, Scotland

WOOD PHYSICS

Textbook of Wood Technology. Vol. II, *The Physical, Mechanical and Chemical Properties of the Commercial Woods of the United States*. H. P. Brown, A. J. Panshin, and C. C. Forsaith. New York: McGraw-Hill, 1952. 783 pp. $10.00.

THIS is the second volume of the *Textbook of Wood Technology* to appear under the joint authorship of Brown, Panshin, and Forsaith. Although the responsibility for this volume is shared by all three authors, it is principally Forsaith's contribution to the textbook, and represents a greatly expanded and revised version of his *Technology of New York State Timbers* (1926), just as Volume I was a revision of Brown and Panshin's *Commercial Timbers of the United States* (1940), which was in turn a much-enlarged and modified version of their *Identification of the Commercial Timbers of the United States* (1934).

The two-volume set, according to the authors' preface in Volume I, was designed to cover the whole field of wood technology, so that a student could find all factual information within one text. And the publisher's statement on the jacket, "Here is the second volume of this outstanding two-volume work, which now stands as the only comprehensive text in English embracing all phases of wood technology," suggests that this design in fact has been accomplished. In order to report the objectives fairly, however, it must be noted that the preface to Volume I indicates that the chemistry of wood is to be dealt with only "in so far as its chemistry should be known to forestry students and to wood utilists other than chemists," and the preface to Volume II points out that it includes "an abridged survey of this very extensive subject."

Volume II deals with the physical and chemical properties of wood, in three sections: "The Physical Properties of Wood," "The Mechanical Properties of Wood," and "The Chemical Properties of Wood." Part I is concerned with the nonmechanical physical properties of wood and embraces such subjects as density and specific gravity, relationships of moisture (including shrinkage), relation to heat, sound, light, and electricity, and, surprisingly, wood bonding and finishing.

Part II considers the mechanical physical properties and, on a bulk basis, overshadows the other two parts, since it comprises almost two thirds of the book. It includes such subjects as the mechanics of short wood columns and wooden beams, standard wood-testing procedure, working stresses, variation in strength properties, strength of laminated beams and plywood, holding power of fasteners, and stresses in framed structures. It is pertinent to note that about a third of this section is devoted to fundamental concepts and applications of mechanics and strength of materials, which this reviewer believes are more explicitly dealt with in treatises on these specific subjects.

Part III, as already pointed out, is an abridged and elementary approach to the subject, expressly designed for the "layman approaching the subject for the first time," and encompasses only 40 pages. It is divided into three chapters: The Chemical Components of Wood, Effect of Chemical Treatments on Wood, and Thermal Reactions of Wood and its Decomposition by Biological Agents.

From the standpoint of teaching wood physics, the order of presentation of the broad subdivisions is quite satisfactory, and the frequent use of illustrative problems greatly facilitates the understanding and application of the principles and techniques that are presented. After using the volume in the classroom for one semester, however, it is this reviewer's opinion that considerably more painstaking editing and revising of

many parts of the manuscript and proof were needed before publication. There are numerous typographical and some textual errors, inconsistencies, and omissions that confuse the student and cause him to lose confidence in the text. And entirely aside from these errors, of which we found more than 40, many portions of the text lack the clarity and conciseness of expression that one would hope to find in a technical presentation of this nature.

In the preface to this volume, it is pointed out that no pretense is made of presenting an exhaustive study of the differences in physical reactions that characterize the different woods of the United States. It would seem, however, that inclusion of a few tables of basic data, such as the strength values of clear wood, shrinkage, and structural grades, with working stresses of the more important commercial species, would have come closer to realizing the goal stated in the preface to Volume I. Also it is unfortunate that some of the illustrations in Volume I, such as those pertaining to the fibrillar structure of cell walls, kiln-drying defects, and compression wood, are not duplicated in appropriate places in Volume II, for they are needed to supplement some rather involved word pictures. Finally, it would seem that more of the important references, and more references of recent date, should have been listed for most of the subjects covered. In this respect Chapter V, on Wood in Relation to Sound, Light and Electricity, is a welcome exception.

Although the book is definitely a contribution to the literature of wood technology, this reviewer regards it as not up to the standard of Volume I, and he hopes that an early revision will bring it more in line with that well-received work.

R. A. Cockrell

School of Forestry
University of California, Berkeley

MYTHS AND RELIGION

Djanggawul, An Aboriginal Religious Cult of North-Eastern Arnhem Land. Ronald M. Berndt. New York: Philosophical Library, 1952. (Printed in Great Britain.) xxiii + 320 pp. Illus. $7.50.

The author of *Djanggawul* is a lineal, if at times critical, adherent of the "functional" symbolic approach to mythology and ritual taken by Durkheim, Radcliffe-Brown, and Warner. In fact, he and the last-named have worked in the same area and dealt in much the same way with certain myths of the natives of Arnhem Land in Australia. Comparisons with the present work and *A Black Civilization* (1937) are therefore inevitable but must here be omitted.

Berndt deliberately minimizes theoretical formulations, especially the psychoanalytic, and stresses the raw material, with only enough interpretation and analysis to make it intelligible. This puts a severe strain on the nonspecialist, for he is provided with almost no ethnographic background and is assaulted with distracting native terminologies, dreary and repetitious song texts, and multiple-guised persons and objects. It is consequently doubtful, as the author wishfully believes, that the beauty and profoundness of the Djanggawul cult will influence administrators, missionaries, and teachers, and cause them to adopt a more tolerant attitude toward aboriginal life.

Nevertheless, for the professional anthropologist the book has solid merit and exciting data. Using the translated texts of 188 songs as a basis, it describes the Djanggawul cult. The Djanggawul are three ancestral beings—two sisters and a brother—who lived in the mythical past, and the narrative that depicts their actions also serves to substantiate and sanction the rituals which accompany them. The three ancestors institute religious ritual, dogma, and behavior. They are associated with the sun, which, with its warmth, causes people and animals to grow. They are, too, creative ancestors and therefore held in great reverence. Certain sacred symbols carried along with them in their travels have procreative significance and are identified with them. The myth shows the sensitive awareness of the aborigines that sex is vital for the survival of all life. Thus, the Djanggawul bring rain, germinate the soil, and ensure the natural resources of the land.

Djanggawul, then, is drenched in sex, without, as Berndt points out, being erotic. It is a good argument for illustrating the heights which can be reached by an "old Stone Age" people.

William A. Lessa

Department of Anthropology
and Sociology
University of California, Los Angeles

ADVANCES IN MEDICINE

Recent Progress in Hormone Research. Proceedings of the Laurentian Hormone Conference, Volume VII. Gregory Pincus, Ed. New York: Academic Press, 1952. 527 pp. $9.50.

This book is a stimulating, if somewhat overwhelming, tribute to the almost frightening strides with which the science of endocrinology hurries forward. Certainly no one who, for pedagogical or clinical reasons or simply as a matter of intellectual curiosity, feels compelled to keep abreast of this rising flood of information, and who possesses the necessary background information, can afford to miss this volume. All such readers will feel a debt of gratitude toward its contributors.

A record of the Proceedings of the 1952 Laurentian Hormone Conference, the book is divided into four sections entitled, respectively: "The Pituitary Hormones," "Sex Cycles," "Aspects of Steroid Hormone Chemistry and Physiology," and "Hormones and Metabolism." Each section contains four or five papers, all of them interesting and timely. As is usual nowadays, emphasis falls heavily on the steroid and pituitary hormones.

This reviewer, admittedly guided by his own limitations and interests, was particularly impressed with Long's masterly summary of his views concerning the

regulation of ACTH secretion, Markee and his associates' account of their researches on the nervous control of gonadotrophin release, and Folley's studies on the physiology of lactation. The discussion of the physiology of the human menstrual cycle by Smith and Smith was somewhat disappointing, for their highly significant work deserves to be brought into sharper focus than was done here.

Gassner's report on the physiology of the gonadal cycle in domestic animals was one of the most thought-provoking. Certainly some of the most exciting work on the physiology of reproduction today is being done in the field of animal husbandry, work which is not, on the whole, too well reported in the medical literature. As medical science prolongs the life span, agricultural and veterinary science must provide an ever-mounting supply of food. Intimate knowledge of the mechanics of reproduction not only aids the obstetrician and the gynecologist, but it also insures more butter, eggs, and meat to support mankind's growing appetite. Anything that keeps the medical and veterinary scientists in step with one another will postpone the day, considered inevitable by many, when the world's population outstrips its food supply.

Other equally important papers in this volume deal with the chemistry and synthesis of steroid and pituitary hormones; hormonal regulation of fat and carbohydrate metabolism, as well as water and electrolyte balance; the relation between hormones and antibody production; hormones and hypersensitivities; and the histological changes produced in the adrenal gland by inanition.

The book is well organized, printed, and edited—a thoroughly acceptable companion to its predecessors. No other conference seems to encompass quite so much endocrinology as does this one. No doubt this is due to the peculiar genius of Gregory Pincus, who has edited this as well as other volumes in the series. Omissions are inevitable in any single year and of no consequence if rectified later. Certainly one of the conferences in the not too distant future should devote some time to the growth hormone, the thyroid hormone, and possibly even the parathyroid hormone. Perhaps it might not be out of place to include as well something on the growing body of information about the action of hormones in the invertebrates, especially in the insects and the crustaceans.

DIETRICH C. SMITH
School of Medicine
University of Maryland, Baltimore

NAIVE HYPOTHESIS?

The Lost Discovery: Uncovering the Track of the Vikings in America. Frederick J. Pohl. New York: Norton, 1952. 346 pp. Illus. $3.75.

IN *The Lost Discovery* Frederick J. Pohl sets forth his opinion that Bjarni, lost on a voyage from Iceland toward Greenland in A.D. 986, saw Cape Cod, Massachusetts, Nova Scotia, and Newfoundland before finally arriving at his destination. He also believes that Lief Ericson, a few years later, made his way around Monomoy on Cape Cod to enter and build a camp upon the ponds at the head of Bass River in southeastern Massachusetts. Thorvald Karlsefni, Lief's brother, followed him to Vinland and eventually was killed, according to Mr. Pohl, in an encounter with the Indians at the mouth of Somes Sound, Mount Desert Island, Maine. Thorvald is said to be buried in a field there; his remains are yet to be found. The tale continues, and practically every place, no matter how briefly or incompletely described in the sagas, is rather precisely identified. Pohl even goes so far as to interpret the extremely controversial Zeno narrative to mean that the harbor called Trin is Guysboro Harbor in eastern Nova Scotia. He figures that this harbor was entered by the expedition on Trinity Sunday, June 2, 1398! These notes comprise only a selected and perhaps unsatisfactory listing of a few of the amazing results of the author's ingenious and indefatigible work during the past few years. The significance and validity of the results are another matter.

There is no term like "science fiction" to describe books like this, dealing as they do with a combination of geography and history. Like Holand, Goodwin, Horsford, and a host of others, Pohl has let his vigorous and untrained imagination go unchecked as he reads and dissects the translations of the sagas. The results appear in a well-organized book, the style of which is much better than usual in this type of literature. He has constructed an amusing and highly readable yarn about the comings and goings of the Norse. There are minor inconsistencies in this, as is to be expected, and at times the so-called logical word-by-word interpretation is incredible if not ridiculous, but after all a reader must not be too particular under such circumstances.

A scholar seriously concerned with locating places where the Norse landed in America, and in the history of the encampments and voyages, will find nothing new in the book except a lot of ideas that have little or no basis in fact. Mr. Pohl has put forward a naïve hypothesis. The interpretations of the translated texts of the sagas are based upon rather detailed and specific statements concerning navigation and conditions at sea and along the New England coast. It is quite obvious that the author is either unfamiliar with the whole subject or that he has not understood the wealth of accurate nautical information available.

To support his contention that the Norse landed at various spots along the coast, the author refers to the numerous artifacts and other features commonly mentioned in the "Norse literature." All these things are matters of extreme controversy, and many of them have been completely repudiated by experts who are more fully informed than is Mr. Pohl. The author produces little or no information which abates the controversies. In fact, in some cases he does not even admit that variant and important opinions concerning the validity of the evidence exist. In other instances, particularly regarding archaeological evidence, Mr. Pohl naïvely dis-

misses the careful recent work of experts in a manner that reveals both his uncontrolled bias and his ignorance of the fundamental concepts upon which the work was based. In summary, this is a pleasant book that can keep a reader entranced with the workings of a man's imagination. One is gratified to read Mr. Pohl's easy solutions to the many problems that have frustrated scholars for practically a century. However, this book adds no new facts or sound opinion to what was previously known about the Norse in North America.

FREDERICK JOHNSON

R. S. Peabody Foundation
Andover, Massachusetts

FIELDIANA

Mogollon Cultural Continuity and Change: The Stratigraphic Analysis of Tularosa and Cordova Caves. Fieldiana: *Anthropology,* Vol. 40. Paul S. Martin, John B. Rinaldo, Elaine Bluhm, Hugh C. Cutler, Roger Grange, Jr. Chicago: Chicago Natural History Museum, 1952. 528 pp. Illus. Paperbound, $8.00.

THE name Mogollon, derived from a nearby mountain range, is used to designate a series of archaeological assemblages occurring in the San Francisco River drainage in west central New Mexico and adjacent portions of Arizona. Long-term archaeological investigations were started there in 1939 by the Field (now Chicago) Museum of Natural History. The work of earlier seasons, previously reported upon, was restricted to sites in the open, and although a useful body of data pertaining to Mogollon remains was accumulated there was still need for stratigraphic evidence to substantiate the sequences developed from typology and to obtain a collection of objects made from perishable materials which would be representative of that part of the material culture. Formerly occupied caves are ideal for such purposes, and, although formations of that nature are scarce in the area, two were found that answered the requirements. Both are in the Apache National Forest, Catron County, New Mexico. Tularosa Cave, located about a mile east of the town of Aragon, was excavated in the summer of 1950. The other, Cordova, is about six miles south of Reserve and was dug during the 1951 field season. Tularosa Cave yielded 2130 well-preserved specimens and large quantities of plant remains. Cordova Cave was not as productive; only 1200 artifacts and considerably less plant materials were recovered, because an extensive conflagration had destroyed large areas of the deposits during the period of aboriginal occupation.

The major portion of the publication is devoted to detailed descriptions of the artifacts from the caves and discussions of their significance and relationships. Information about the various items is combined with that obtained from the open sites during previous years, and all the traits of the tangible Mogollon culture are listed. In their study of the data the authors arrived at the conclusion that there were three major changes in the growth of the cultural pattern, each coinciding with the beginning of what they call a "Phase." The causes for the changes are not completely known, but it seems certain they were in no small degree the result of influences derived from Mexican cultures in the south, the Hohokam culture in southern Arizona on the west, and the Anasazi in the plateau area to the north. Throughout, however, certain basic traits appear to have persisted and to have provided a definite continuity. Certain changes in plant and animal remains seem to correlate with those in the culture, and marked modification of the subsistence pattern may have had a direct bearing on them.

Carbon 14 tests on corn from the lowest levels of Tularosa Cave gave dates of 2300 ± 200 and 2223 ± 200 years before the present, and the authors estimate that it was abandoned between A. D. 1000 and 1200. They think the cave was more or less continuously occupied for about 1500 ± 500 years. The major changes in the culture are believed to have occurred at about 150 B. C., A. D. 700, and A. D. 1000.

In the opening section, which discusses the organization of the report, it is stated that the authors have tried to put it together so "as to make it easily usable by specialists, students, and general readers, and to make it readable." It may be said that on the whole they have succeeded, but general readers, as well as some students and specialists, may find it difficult to grasp their concept of the meaning of a Phase and the arbitrary assignment of chronological positions or phases to levels on the basis of the percentage of pottery types present. The numerous illustrations will be very helpful to those making comparative studies. The publication certainly is a contribution to the knowledge of Southwestern archaeology.

FRANK H. H. ROBERTS, JR.

Bureau of American Ethnology
Smithsonian Institution

THE RECORD OF THE ROCKS

The Origin of Metamorphic and Metasomatic Rocks. Hans Ramberg. Chicago: University of Chicago Press, 1952. xvii + 317 pp. Illus. $10.00.

THE subtitle, "A Treatise on Recrystallization and Replacement in the Earth's Crust," better expresses the coverage of this important book. It deals with the thermodynamics of metamorphic processes (39 pp.) and gives equilibrium diagrams (54 pp.) of metamorphic minerals. In the section on the chemical kinetics of metamorphism, Dr. Ramberg does not regard the natural moisture in rocks as the medium for mineral solution, transfer, and subsequent deposition. He likens the "pore fluid" to the artificial ether for the propagation of light. He prefers "the reactive ions, atoms, and molecules in the rocks." The author emphasizes granitization, not magmatism.

Ramberg discusses the belief of some structural geologists that the lineation of spindle-shaped minerals is perpendicular to the direction of plastic flow, whereas others think it coincides. An experimental solution of

this is difficult because it is a very slow process. The results of the deformation of metals have been used as a clue to what may happen in rocks. The yield point of metals is not constant under a given P–T condition but depends on the duration of the test. The author leaves the structural properties of deformed rocks to others, as it is too large a subject for a book of this size. He shows briefly how the old and new minerals reorient themselves under the influence of stress.

The author is of the opinion that quartz-feldspar rocks are more readily deformed than the basic types. In dry melts the relationship is reversed. He believes the higher concentration of —O—Si—O— bonds in acidic rocks is the cause of the difference. It is possible that ultrabasic rocks are injected as solid aggregates.

Thirty-three pages are devoted to mineral facies in metamorphic rocks, following the work of Goldschmidt and Eskola. Ramberg presents 17 triangular (ACF) diagrams and 40 equations. The latter are so simplified that the polycomponental nature of many minerals is not evident.

The most interesting, and at the same time the most controversial, section of the book is the "transfer of matter through rocks." Ramberg thinks that the rocks, regarded by some as "soaked" by magmatic matter with a low viscosity, are in reality affected by the "agency of metamorphism," penetrating crystalline aggregates and passing through interconnecting capillary pores; or that the rocks behave "like a semipermeable 'membrane,' permeable to individual atoms, ions, or molecules but behaving like an insulator against the bodily flowing pore fluids" (p. 174). In this manner potash, soda, and silica penetrate rocks and change hornblende into biotite and epidote, muscovite into potash feldspar, and so on.

Two very short chapters, one on "contact metamorphism" and the other on the "metasomatism in sedimentary rocks," leave much to be desired. The only reference in the latter is to Pettijohn. The author lists dolomitization and silica-replacement. His last major section is devoted to the metamorphism in regionally metamorphosed complexes. Here he is on familiar ground: the plastic deformation, recrystallization, and replacement within pre-Cambrian shield areas. This leads to the questioning of the actual existence of quartz-feldspar plutons with high viscosity and to the supposition that they were "developed in the solid state by activation, diffusion, and consolidation of ions, atoms, or molecules."

Dr. Ramberg's contribution would be perhaps more attractive if a better balance could have been attained between the processes of the crystallization of magmatic melts ("crucible" petrology), on the one hand, and metasomatism by liquid and gaseous material attacking the consolidated products, on the other. The word "deuteric" does not appear in the index.

A discussion of the chemical bonds in crystals composes the appendix.

The book is not always easy reading, as the author uses some technical terms in special ways; the scapolite end member, meionite, is spelled "mejonite." Ramberg uses the term "mixed crystals" for solid solutions and regards perthite as the result of "unmixing" rather than of exsolution. The book is well printed and bound, and it should be in the library of all geologists concerned with rocks.

HAROLD L. ALLING

Department of Geology
The University of Rochester

BRIEFLY REVIEWED

Mathematical Models. H. Martyn Cundy and A. P. Rollett. New York: Oxford University Press, 1952. 240 pp. Illus. $5.50.

THE opening sentences of this book are: "Mathematics is often regarded as the bread and butter of science. If the butter is omitted the result is indigestion, loss of appetite, or both. The purpose of this book is to suggest some ways to butter the bread." The authors certainly did not ration their butter and the result is a fascinating book giving models to illustrate mathematics.

Although the book is mainly written to encourage secondary school teachers to interest their pupils in mathematics by letting them make collections of models, it certainly can be recommended to a much wider public. The authors describe in detail the models that are part of the Sherborne collection and how they can be reproduced.

One chapter deals with models in plane geometry; another one with polyhedra, including illustrations of all the polyhedra discussed. In Chapter 4, which also deals with models in solid geometry, such fascinating subjects as torus, showing a seven-colored map, a Moebius surface, and a Klein bottle are discussed.

The last chapter deals with, strictly speaking, physical rather than mathematical models, such as the Galton Quincunx (to demonstrate binomial distributions) and machines for solving equations, such as the hydrostatic equation-solver and Mellock's electrical machine.

Altogether, this is a book which anyone who has an aesthetic sense, whether he is mathematically inclined or not, will greatly enjoy, and which should be a compulsory text for mathematics masters of secondary schools.

D. TER HAAR

Department of Natural Philosophy
The University, St. Andrews, Scotland

Advances in Geophysics, Vol. I. H. E. Landsberg, Ed. New York: Academic Press, 1952. xi + 362 pp. Illus. $7.80.

THIS volume inaugurates a new series, edited by Dr. Landsberg with the aid of a distinguished advisory committee, and intended to present critical

reviews, summaries, and progress reports for the various branches of geophysics. The potential usefulness of such an undertaking is beyond question, and the eight papers of the initial volume establish, on the whole, a high standard. Discussions of problems of the atmosphere and upper atmosphere account for nearly two thirds of the space; in addition, there are papers on "Some New Statistical Techniques in Geophysics," "Estuarine Hydrography," "The Earth's Gravitational Field and its Exploitation," and "Aeromagnetic Surveying." Treatment of other aspects of geophysics is promised for subsequent volumes.

Some of the writers have failed to keep in mind the possibility that their subjects may interest nonspecialists, and consequently have neglected to include sufficiently comprehensive introductions; a few of the papers would have benefited from more careful editorial scrutiny and better proofreading. But these are minor defects in a generally well-conceived, well-edited, and well-printed volume.

FRANCIS BIRCH

Department of Physics, Harvard University

Understanding Heredity: An Introduction to Genetics. Richard B. Goldschmidt. New York: Wiley; London: Chapman & Hall, 1952. x + 228 pp. Illus. $3.75.

JUST twenty years ago Richard Goldschmidt placed in my hands the gift of a copy of his little book *Die Lehre von der Vererbung.* It took only a few pages of reading to discern that here was a book about heredity written with such unexampled clarity and penetrating exposition and with such simplicity and directness of style that the young student found it quite as helpful a guide to the German language as the novice to the study of heredity. The only book to be compared with it in these respects was Karl von Frisch's entrancing survey of general biology, *Du und das Leben.* The wonder arose then and persisted for many years that no one had been found to do the like in English, or even to translate these two masterpieces. Now at last we possess one of them in our own language, translated under the title *Understanding Heredity,* and preserving all the admirable qualities of the original by having been rendered into English by its own author, who for many years has been one of the leaders of genetic research and teaching in this country, since the rising tide of conflict between politics and genetics drove him from his homeland.

To bring the account of modern genetics up to date, a final chapter has been added, in addition to minor alterations throughout. This last chapter, which is entitled A Glimpse of More Technical Facts and Problems of Genetics, deals briefly with such topics as the experimental production of mutations, chromosome rearrangements, giant chromosomes, cytogenetics, the genetics of lower organisms, the role of the cytoplasm in heredity, sex determination, the action of the genes, biochemical genetics, and the relation of genetics to evolution. In these discussions the same admirably clear exposition that characterizes the rest of the book has been maintained. The aim of the author has been to whet the reader's interest and to arouse eagerness for a greater knowledge of the subject, and not to provide an encyclopedic account.

Those who know Professor Goldschmidt's eminence in the field of physiological genetics, and his "racial" views of the constitution of the genetic material and the nature of the mutational changes that participate in evolutionary processes, will be surprised to see that these radical ideas are scarcely alluded to, and Goldschmidt's own realms of genetic investigation are by no means overemphasized. To Goldschmidt the first facts and principles to which the learner in this field needs an introduction are the facts and principles of "classical" genetics. These have never been set forth better.

BENTLEY GLASS

Department of Biology
The Johns Hopkins University

The Literature on Streptomycin. Rev. ed. Selman A. Waksman. New Brunswick, N. J.: Rutgers University Press, 1952. 553 pp. $5.00.

SINCE the appearance in January 1944 of the first paper announcing the isolation of streptomycin, almost 1200 publications dealing with this antibiotic had appeared in the nearly four years up to the publication of the first edition of this bibliography in late 1948. By early 1952, when the present edition appeared, over 5500 papers were listed. This calculates to a rate of publication of over two papers per day.

For the future, annual supplements rather than new editions are planned, along with digests dealing with various uses of streptomycin, such as clinical uses other than the treatment of tuberculosis.

The present volume contains 43 general references dealing with actinomycetes, antagonistic properties, and streptothricin. These are followed by 5550 references on streptomycin listed roughly in the chronological order of appearance of the papers. An author index of 67 pages includes every author mentioned in the bibliography. Forty-four pages of subject index include only the major theme of each publication.

The subject index is especially useful in providing a survey of the work done with streptomycin in particular diseases. For example, there are 89 references to its use in tuberculous meningitis, 61 in hemophilus influenzae meningitis, and 21 in gonorrhea.

This compilation is an invaluable contribution to workers in the entire antibiotic field, as well as to those specializing in streptomycin and its derivatives, since it covers all the byways as well as the highroads involved in mapping the usefulness and limitations of an antibiotic.

GUSTAV E. CWALINA

School of Pharmacy, Purdue University

LETTERS

IS HEALTH MAN'S BASIC VALUE?

ARTICLES in THE SCIENTIFIC MONTHLY about social science usually please me, but Mr. Kattsoff's essay (**76**, 24 [1953]) raises some serious questions. On the third reading, I checked the statements that to me seem incorrect, straw-manish, or subject to debate—twenty-six in the eighteen paragraphs.

Among the straw men attacked are the "mob mind," "social forces" (as metaphysical entities), and organism theories. For fifty years or more, social scientists have been attacking oversimplification and single-factor explanations. Yet Mr. Kattsoff tells us that all values should be evaluated—and subordinated—to the single factor health; but his own discussion on page 25 tends to refute his thesis.

His term "intersubjective" suggests that he is unfamiliar with what sociologists and social psychologists were writing fifty years ago. The reciprocal interaction of persons and social structures is one of the oldest and most basic concepts of anthropology, social psychology, and sociology. Only a few social scientists have ever implied that a person can *interact* with anything except other persons. We *react* to physical and biological stimuli and *interact* (or "transact," as Dewey says) with persons.

Kattsoff evidently has not understood the work of Stuart Dodd, who makes values (desiderata) a central concept. Mr. Kattsoff also seems to misapprehend the function of statistics in scientific procedures. He appears to reify values and to believe that some values are antecedent to social conditioning and are unaffected by it. This is a curious position unless he means by values (never clearly defined) *all* factors that *cause* social behavior. Most social scientists probably would hold that social behavior often "causes" (is antecedent to) values, and that in all cases the two factors are reciprocal. He says (speaking of Dodd), "But his variables themselves often involve variables." Are there any variables that do not involve other variables?

How would fifty leading social scientists answer the following questions?

1. Does the article properly reflect present theory and method in your field?
2. Does it contain constructive criticism of theory or method in your field?
3. Does it attack straw men?
4. Is the discussion semantically rigorous?
5. Does it properly appraise the use of statistics in social science?
6. What are its ontological and epistemological implications?

READ BAIN

Department of Sociology
Miami University

THE HUMAN FACTOR IN MACHINE DESIGN

AS THE leading manufacturer of aircraft instruments for the past quarter of a century, we read with interest the article by Leonard C. Mead and Joseph W. Wulfeck entitled "Human Engineering."

Although otherwise excellent in detailing the place of human engineering in aviation safety, the authors are grossly misleading and inaccurate in their reference to our products. Since these Kollsman instruments are in general and often compulsory use in both commercial and military aviation, any imputation as to their reliability is a matter of public interest. We feel, therefore, that the facts should be set straight.

In the December (1952) issue of THE SCIENTIFIC MONTHLY, Figure 5, page 377, shows the dial faces of two different altimeters plainly marked Kollsman. The Kollsman "160," or counter, altimeter is referred to in the caption as the "redesigned altimeter face now in use." The three-pointer type is alluded to as "the old-type altimeter face responsible for many accidents (after Grether)." Neither of these statements is true.

Here are the relevant facts:

1. In the complete CAB investigations of aircraft accidents covering the years 1948 through 1951, there is not a single instance of an accident attributed specifically to a misreading of the altimeter. These official investigations cover all scheduled, irregular, and Alaskan carrier operations. Approximately 35 per cent of accidents reported are laid to pilot error. Although it is almost impossible to determine all the facts incontestably, it must be admitted that the "complexity" of the altimeter dial and the time needed to read it may have, along with situational tensions, contributed to accidents brought about by pilot error. This, of course, is a far different statement from the one appearing in THE SCIENTIFIC MONTHLY.

2. The authors of "Human Engineering" credit Walter F. Grether, of the AF Aero Medical Laboratory, with placing accident blame on the altimeter. Actually Grether is basically concerned with the indices of pilot error reading and makes no claim that the multiple-pointer dial is responsible for many air accidents. This is the apposite statement made by Grether in his article in the *Journal of Applied Psychology* (August 1949):

> Numerous fatal and non-fatal accidents have been attributed to such instrument reading errors, and without doubt many of the unexplained crashes *resulted from similar human error* [italics ours].

Dr. Grether's emphasis is significantly different from that of Mead and Wulfeck. As a psychologist he takes

due account of the human factor. In his researches on clock readings, for instance, Grether found that even on the best designed 24-hour clocks AF pilots scored a 7 per cent reading error!

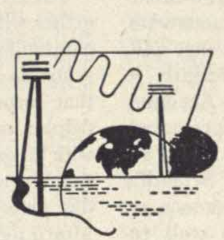
3. Finally, the "redesigned altimeter" referred to by Mead and Wulfeck is not yet in use. Rather, this counter altimeter represents a prototype, a phase of Kollsman research in the direction of simplifying the altimeter dial. Encouraged by the USAF, we developed this model partly in answer to a study by Grether in which he demonstrates the types of error likely to occur in reading the multiple-pointer altimeter. The advantages of the counter dial are made evident in the Grether study; however, the mechanical problem of introducing such a counter into a pressure altimeter design has been tremendous. Its solution has depended upon an almost total reduction of friction so that delicate diaphragm impulses might be translated into accurate counter readings. We are hoping to begin volume production on this counter altimeter shortly.

MILLA ALIHAN
Kollsman Instrument Corporation, New York

DR. ALIHAN's letter of February 18 was most appropriate. We are guilty of the kind of error that so often occurs when two or more people work on the same paper. The caption for Figure 5, page 377, of our article should have been worded "redesigned altimeter face proposed for use" and "the old-type altimeter face responsible for many reading errors." To follow the text and the experimental data, strictly for the record, no relation between altimeter face and accident rate should have been implied.

Certainly Kollsman Instrument Corporation is to be congratulated and admired for its great interest, enthusiasm, and cooperation in working toward the goal of greater safety through greater instrument legibility and interpretability. It was most certainly *not* our purpose to deprecate its product by identifying it as "responsible for many accidents."

JOSEPH W. WULFECK
Institute for Applied Experimental Psychology
Tufts College



SMO ON THE AIR

Station	Sponsor	Time
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Broadcast Limited (Modern Science)	9:45 P.M.

The Editor of THE SCIENTIFIC MONTHLY will be glad to cooperate with university or other educational stations interested in securing scientific material suitable for broadcasting.

ASSOCIATION AFFAIRS

A REPORT OF THE ST. LOUIS MEETING DECEMBER 26–31, 1952

GOOD weather, the enthusiastic cooperation and interest of St. Louis citizens, and warm hospitality on the part of local scientists combined to make the 119th meeting of the American Association for the Advancement of Science a decidedly pleasant and memorable occasion. The Academy of Science of St. Louis arranged a luncheon at which the Executive Committee and administrative staff of the Association were guests, and many resident scientists invited out-of-town speakers and colleagues to their homes. Apart from its friendliness, this annual meeting of the AAAS was one of the most important and significant conventions in the long history of the Association, now in its 105th year.

The sixth St. Louis meeting was noteworthy in more than one respect. Among the items of business transacted in the two sessions of the Council, at which 106 members were present, was the adoption, by unanimous vote, of a sixth constitution and a new set of bylaws that will, it is hoped, enable the Association more efficiently to continue its work of advancing science. The Academy Conference, with 29 academies officially represented, had a particularly successful series of sessions. This recurrent conference and the Conference on Scientific Manpower were joined this year by a Conference on Scientific Editorial Problems, which was so well received that the participants decided to organize it, also, to continue at future AAAS meetings. The special sessions, which add so much to the meetings each year—the distinguished evening addresses sponsored by the American Mathematical Society, the National Geographic Society, the Scientific Research Society of America, the Society of the Sigma Xi, and the United Chapters of Phi Beta Kappa, as well as the presidential address of the AAAS itself—attracted large and appreciative audiences. The AAAS Science Theatre, which showed the latest in foreign and domestic scientific films, was consistently well patronized. The Annual Exposition of Science and Industry, with 72 exhibitors and 116 booths filling Convention Hall of the Kiel Auditorium and overflowing into the promenade, was up to the high standard of recent years and drew an appreciative audience. There were programs in all principal fields of science and there was a universal concurrence of opinion regarding their high quality. All these aspects of the meeting merit more than passing mention.

Symposia. The number and quality of the symposia were impressive. The three general symposia were much more than reviews of the literature of their subjects. "Disaster Recovery," with eminent authorities from a variety of fields, analyzed the characteristics of natural and manmade disasters and examined the common principles of recovery. "Applications of the Theory of Games" pleased the mathematicians and statisticians, and interested the laymen who attended. In "The Nation's Nutrition," eight national leaders in the field presented, primarily, their own most recent research. With highly qualified discussants in addition, this program commanded a gratified audience that exceeded two hundred in number.

Another highlight of the meetings—an innovation that proved popular and will be repeated—was the series of four public discussion book panels arranged by the Society of Systematic Zoology and cosponsored by Section F. These sessions, set up in TV fashion with authors and discussants present, drew audiences approaching two hundred. Audience participation was encouraged and, in one of the panels on evolution, spirited discussion prolonged the session long beyond the scheduled time.

The 18 sections and subsections of the Association, together with the 35 participating societies and other organizations that had programs of their own, arranged 62 symposia and panel discussions—a record-breaking total that surpassed the previous high of 42 at the Philadelphia meeting of 1951. All were of high caliber, but with 90 sessions and 381 speakers, there was competition that cut attendance at some programs during the four-day period in which most of them were concentrated. Most program chairman expressed satisfaction with the attendance.

The proportion of symposium papers to short contributed papers was high because relatively few of the larger scientific societies met with the Association this year. Of the 53 organizations that participated, 35 had programs of their own and, of these, only 24 had sessions for contributed papers. For many of these participating groups, however, either it was not their national meeting or their memberships are modest in size. Their participation, it should be emphasized, manifested their interest in the AAAS and the work it is doing, and their contributions were substantial.

At St. Louis, the emphasis on symposia and panels constituted a further advance toward the point of view developed in the Arden House Conference—that AAAS meetings should offer programs that not only will provide opportunities to integrate the several scientific disciplines but will also serve to increase public understanding and support of science. How best to attain these worthy objectives merits careful study. A general symposium, planned in part for intelligent laymen who are local residents, must have a strong social appeal if it is to compete successfully with the daily routine and social habits of business and professional persons. A technical symposium can be expected to attract specialists but it is handicapped if it stands alone and is not supported by paper-reading sessions in the same field. Individual scientists, who attend a scientific meeting primarily to

read a paper and to hear several others in their specialty, constitute an important and receptive audience for symposia that integrate the sciences, develop neglected interdisciplinary areas, or deal with problems of common concern to all scientists. The number present at a AAAS meeting is in direct proportion to the number of contributed papers. At St. Louis ten AAAS sections contributed substantially to the attendance by arranging 25 sessions at which 165 contributed papers were read. Indeed, the 24 societies that had scheduled 54 sessions for paper-reading reported only an additional 209 short papers, of which 94 were given before the three mathematical societies.

A number of section secretaries and others interested in scientific meetings have independently reached the following basic conclusions:

1. Most people go to meetings primarily to see other people in their own or allied fields.
2. The reading of a paper commonly means financial assistance to the author in getting to the meeting.
3. Advance publication of the program, by providing a partial list of those who will be present, stimulates attendance.
4. Once at the meeting, the individual scientist enjoys the symposia and general sessions that are available, but only exceptionally can a scientist afford to attend symposia at a meeting in which sessions for contributed papers in his field are nonexistent.

As long as a AAAS meeting has a core of sessions for contributed papers, a good potential "core attendance" for symposia is assured, and it is gratifying to know that the number of contributed papers will increase at the AAAS Boston meeting of December 26–31, 1953, because full-scale national meetings of the American Society of Zoologists, the American Society of Naturalists, the Genetics Society of America, the American Society of Human Genetics, not to mention sessions of other societies, are scheduled to take place in conjunction with the 120th meeting of the Association.

Participating Societies at St. Louis. In addition to the 18 sections and subsections of the Association, a total of 53 societies and other organizations officially participated in the 119th meeting.

An analysis of the 264 separate sessions of the St. Louis meeting yields some interesting statistics (Table 1).

TABLE 1

Number of sessions for contributed papers	79
Number of symposia and panels	95
Number of business sessions	32
Number of meal functions (at many of which addresses were given or business transacted)	27
Number of round-table sessions or conferences	10
Number of sessions devoted exclusively to addresses	21
Total	264

There were at least 912 speakers, including authors of contributed papers, 374; authors of symposium papers and panel members, 403; discussants, 83; speakers giving introductions and principal addresses, 52.

Attendance. The official number of registrants (exclusive of representatives of the press, exhibitor personnel, and others who assisted with the conduct of the meeting) was 1938. At first thought, this may seem to be an exceptionally low number for a AAAS meeting, whereas, actually, this figure does not compare unfavorably with past meetings in St. Louis, nor does it even approximate the total attendance. Two experiments apparently account for the low registration: One was the sale of copies of the General Program–Directory to institutions and to individuals, separately, at cost ($1.50), without requiring registration. Programs sold in this way totaled 626. Although not every purchaser attended the meeting, there is reason to believe that a substantial number did, without completing their registration by paying an additional dollar for a convention badge. Second, because of the high degree of local support and cooperation, President Bronk proclaimed that all sessions of the meeting would be thrown open to interested adults and college students in Greater St. Louis. This proclamation, which was widely publicized locally,

TABLE 2

DISTRIBUTION OF REGISTRANTS BY STATES AND COUNTRIES

Alabama	15	North Carolina	10
Arizona	1	North Dakota	2
Arkansas	21	Ohio	86
California	40	Oklahoma	28
Colorado	23	Oregon	4
Connecticut	20	Pennsylvania	45
Delaware	3	Rhode Island	7
District of Columbia	60	South Carolina	3
Florida	15	South Dakota	6
Georgia	5	Tennessee	34
Idaho	5	Texas	36
Illinois	265	Utah	9
Indiana	80	Vermont	1
Iowa	57	Virginia	31
Kansas	55	Washington	6
Kentucky	15	West Virginia	9
Louisiana	17	Wisconsin	40
Maryland	42	Canada	14
Massachusetts	25	Cuba	1
Michigan	76	England	1
Minnesota	37	Finland	1
Mississippi	11	India	1
Missouri	495	Israel	2
Montana	5	Mexico	1
Nebraska	32	New Zealand	1
New Jersey	32	Philippines	1
New Mexico	2	Venezuela	1
New York	103	Total	1938

brought many persons, at least to the special sessions and the exposition, who otherwise might not have come at all. At the same hour on Monday evening, Dec. 29, for example, the National Geographic lecture in the Opera House had an audience of 3400; the Sigma Xi address in the Gold Room of the Hotel Jefferson enjoyed an attendance of 1000; the mathematicians' dinner had several hundred; more than 100 geologists were at Washington University; the science teachers had events at the Hotel De Soto; and there were scheduled sessions in psychology, the social sciences, and medicine. It is a conservative estimate that during the meeting a total of 8000 different persons attended one or more phases of the meeting.

The registration totals for the two previous meetings in St. Louis—2649 (March 1946) and 2292 (December 1935)—afford an interesting comparison. At these two earlier meetings, the botanical societies, the entomologists, the parasitologists, the geneticists, the horticulturists, the American Society of Zoologists, and other societies all held their annual national meetings with the AAAS. In 1952, without any of these organizations participating, registration was only 700 lower than the registration at the fifth St. Louis meeting in 1946. If allowance is made for the effects of the separate sale of programs and the proclamation of an open meeting, this year's attendance may have been considerably larger than that of March 1946.

At the Philadelphia meeting in 1951, the registration of 3702 exceeded the 3339 registrants of the 1940 meeting in that city, despite the fact that most of the societies mentioned above, and still others, took part in 1940 but not in 1951. Thus, for two consecutive years, it has been demonstrated that, with a core of paper-reading sessions in the sciences proper, there is an audience for the symposia, the conferences, and the special sessions that are distinctive features of the annual meeting of the Association. Analyses of the registration, geographically and by subject fields, are given in Tables 2 and 3.

TABLE 3

SUBJECT FIELDS OF REGISTRANTS

Mathematics	46
Physical Sciences	
Astronomy	16
Physics	116
Chemistry	204
Geology and Geography	106
Engineering	65
Biological Sciences	
Botanical Sciences	198
Industrial Microbiology	32
Zoology	244
Other Biology	88
Medical Sciences	
Dental Research	36
Pharmacy	65
Other Medicine	317
Psychology	72
Anthropology	29
Social Sciences	47
Science Teaching and Education	174
General	83
Total	1938

An inspection of these data indicates that the 119th meeting was definitely a national meeting, with only four states (Maine, New Hampshire, Nevada, and Wyoming) unrepresented.

Detailed comment on the analysis of subject fields seems unnecessary. Two of the paradoxes of the registration are the small number of mathematicians, notwithstanding the fact that the mathematical societies officially met with the AAAS, and the large number of botanists, with not a single botanical society meeting with the Association.

The St. Louis Reception Committee. The Preconvention Issue of SCIENCE (**116,** 611 [1952]) listed all those who served on the Executive Committee and the several subcommittees of the St. Louis Reception Committee, and who, in one way or another, contributed to the success of the convention. The Association was fortunate in having as general chairman, Charles Allen Thomas, president of the Monsanto Chemical Company. He maintained an interest in all phases of the meeting and welcomed the Association to St. Louis upon the occasion of the AAAS Presidential Address of Kirtley F. Mather. To Dr. Thomas and his executive assistant, Philip R. Tarr, the Association is greatly indebted.

Annual Exposition of Science and Industry. The Annual Exposition of Science and Industry is well established as an integral and important feature of the annual meeting of the Association. As its title implies, it provides those who use the tools and materials of science and those who produce and distribute them an opportunity to meet each other. The 1952 Exposition, with 72 exhibitors and 116 booths, which filled Convention Hall of the Kiel Auditorium and overflowed into the promenade, did not fall short of the high standard of recent years. In addition to the latest and best in scientific books, instruments, and laboratory supplies, there were excellent special exhibits and a splendid series of technical exhibits of large industrial firms—which are coming to realize the advantages of showing their latest technological accomplishments to an appreciative, highly trained, professional attendance.

Potential exhibitors in the St. Louis area were approached through a vice-chairman of the St. Louis Reception Committee, and the Association is conscious of its obligation to Leslie J. Buchan, vice chancellor and dean of the faculties, Washington University, for his personal efforts and keen interest throughout. In addition to the exhibitors listed on pages 619–25 of the December 5, 1952, issue of SCIENCE, the following had booths:

Anheuser-Busch, Inc.
Denoyer-Geppert Company
Folkways Records & Service Co.
General Van & Storage Company, White Motor Company, and Fruehauf Trailer Co.
Hospital Topics and *Northwest Medicine*
E. Leitz, Inc.
Ludlow Saylor Wire Company
McDonnell Aircraft Corporation
Missouri Bureau of Public Health Engineering
Natkin & Company, Inc.
Nuclear Research & Development, Inc.
Phillips Scientific Corporation
Westinghouse Electric Corporation

Booth space was endowed by:

Nooter Corporation
Olin Industries, Inc.
Southwestern Bell Telephone Company
The Technicon Company
Union Electric Company of Missouri

RAYMOND L. TAYLOR
Assistant Administrative Secretary, AAAS

BOSTON, 1953

THERE is no substance to the rumor that the AAAS relaxes or leaves on winter vacations as soon as the Christmas meeting of the Association is over. Work on the next annual meeting fits into the administration schedule as tightly and as quickly as an interchangeable part in a piece of machinery, which, with some adjustments for the new job to be done, continues to function with little more than a momentary pause.

The story of any annual meeting spreads over several years, as the case of Boston illustrates. As far back as the spring of 1950, Raymond L. Taylor studied The Hub's physical facilities and tentatively reserved all of them for Association use in December 1953. The AAAS Executive Committee designated Boston as the 1953 meeting place in October 1950; and promptly thereafter the tentative arrangements made by Dr. Taylor were confirmed. Correspondence was then started to stimulate thinking on the broader aspects of the program.

In late 1952, despite preoccupation with the final details of the St. Louis convention, the tempo was accelerated. Earl P. Stevenson, president of Arthur D. Little, Inc., agreed to serve as General Chairman of the Local Committee; and James B. Conant and James R. Killian readily agreed to work with Dr. Stevenson in the capacity of vice-chairmen. This situation was, of course, altered when Dr. Conant resigned from the presidency of Harvard to accept appointment as U. S. High Commissioner for Germany, but not before he had made several invaluable suggestions. Among them was the thought that, as a thread running through the fabric of the meeting, some such theme as "The Interface of Land and Sea" would enable the Association to make the most of the New England environment and of a special field of investigation in some of its institutions.

Meanwhile several affiliated societies have given thought to the feasibility of meeting with the Association at Boston, and a few—the American Society of Zoologists, the Society of Systematic Zoology, the American Society of Naturalists, Genetics Society of America, the American Society of Human Genetics, the science teachers, and others—have decided in favor of it, and at least a score more have indicated their interest in participating.

Although only a few weeks of 1953 have elapsed as this issue goes to press, key posts on the Local Committee have been filled, the personnel of the Symposium Committee has been selected, meetings of these committees and their subcommittees had been scheduled for early March, floor plans and contracts for booth space at the Exposition were ready to go to the printer for distribution March 15, specific programs are taking form—and administrative thoughts wander, perhaps from force of habit, to the Golden Gate and 1954, where the machinery is running smoothly—although idling—in preparation for the Association's first national meeting on the Pacific rim.

If there is no substance to the rumor of post-convention relaxation for the staff, there is even less to the unfounded impression that the Association's meetings are "outmoded," that its "programs have grown 'thinner'." Neither facts nor figures bear out these defeatist statements. Where else but at a AAAS convention can engineers, biologists, psychologists, industrialists, physical scientists, and public leaders assemble to consider Disaster Recovery? Or the Interface of Land and Sea? Or Problems of the Pacific Rim? It is not the Association that lags, but those who fail to comprehend the scope and the impact of its current program. Intellectual bankruptcy and deterioration will indeed set in if the AAAS turns from programming important science merely to ballyhooing the importance of science.

HOWARD A. MEYERHOFF

GLADYS M. KEENER, executive editor, SCIENCE and THE SCIENTIFIC MONTHLY, and HOWARD A. MEYERHOFF, administrative secretary, AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, and chairman of the Editorial Board, will voluntarily discontinue their duties with the Association on March 31, and will subsequently submit their resignations. Their reasons for taking this step, announced to the directors a month ago, are administrative. Although the Publications Committee has formulated publication policy and is responsible for the directives under which the journals are operated, the editors have been repeatedly and severely criticized by the president, E. U. Condon, both for the content of SCIENCE and for its rigorous editorial standards. Critical of AAAS journals and meetings, the president-elect, Warren Weaver, took vigorous exception to the editorial "Boston, 1953," (reprinted above from SCIENCE, **117**, adv. p. 3 [Feb. 20, 1953]), in which the administrative secretary reaffirmed the soundness of current AAAS policies and attempted to correct the unfortunate impression created by misstatements attributed to Drs. Condon and Weaver in a press interview. Although Mrs. Keener and Dr. Meyerhoff have been asked to remain in their posts, they choose to withdraw in the interest of harmony and from a desire not to hamper the two administrations to follow that of Detlev W. Bronk, retiring president and chairman of the Board of Directors, whose administration they wholeheartedly support. Mrs. Keener has been with the Association since 1945 and has been executive editor of both journals since 1950. Dr. Meyerhoff succeeded the late F. R. Moulton as administrative secretary in January 1949, and had prior service as secretary of Section E (1937–40), elected member-at-large of the Council (1941–44), vice president and chairman of Section E (1944), and executive secretary (1945–46). No announcement has been made with reference either to their future plans or to their successors.

New Books Received

The Anthropology of Iraq. Part II, No. 2. *Kurdistan.* Part II, No. 3, *Conclusions.* Henry Field. Papers of the Peabody Museum of American Archaeology and Ethnology. Vol. XLVI, Nos. 2 and 3. ix + 174 pp. + 76 pls. $6.85. Peabody Museum, Cambridge, Mass. 1952.

Selected Petrogenic Relationships of Plagioclase. The Geological Society of America. Memoir 52. R. C. Emmons, Ed. x + 142 pp. Illus. Geological Society of America, New York. 1953.

Innovation. The Basis of Cultural Change. H. G. Barnett. McGraw-Hill Series in Sociology and Anthropology. xi + 462 pp. $6.50. McGraw-Hill, New York. 1953.

Science and Human Behavior. B. F. Skinner. x + 461 pp. $4.00. Macmillan, New York. 1953.

The Philosophy of Science. An Introduction. Stephen Toulmin. 176 pp. $1.80 text; $2.25 trade. Hutchinson's University Library, London; Longmans Green, New York. 1953.

Vegetable Production. John H. MacGillivray. viii + 397 pp. Illus. $5.00. Blakiston, New York. 1953.

The Human Senses. Frank A. Geldard. x + 365 pp. Illus. $5.00. Wiley, New York. 1953.

Traité Élémentaire de Physiologie Humaine. (3rd ed.) Henri Fredericq. 812 pp. Illus. Masson et Cie., Paris. 1952.

Saudi Arabia. With an Account of the Development of its Natural Resources. K. S. Twitchell. xxi + 231 pp. Illus. $5.00. Princeton University Press, Princeton, N. J. 1953.

The Vienna Circle. The Origin of Neo-Positivism. A Chapter in the History of Recent Philosophy. Victor Kraft. xii + 209 pp. Illus. $3.75. Philosophical Library, New York. 1953.

Astronomy for Everyman. Martin Davidson, Ed. xviii + 494 pp. Illus. $5.00. Dutton, New York. 1953.

Glycerol. ACS Monograph No. 117. Carl S. Miner and N. N. Dalton, Eds. xiv + 460 pp. Illus. $12.00. Reinhold, New York. 1953.

Small Particle Statistics. G. Herndan. xxiii + 520 pp. Illus. $12.00. Elsevier, Houston. 1953.

Chemical Analysis of Industrial Solvents. Chemical Analysis, Vol. 7. Morris B. Jacobs and Leopold Scheflan. xxii + 501 pp. Illus. $10.00. Interscience, New York. 1953.

Elsevier's Encyclopaedia of Organic Chemistry. F. Radt, Ed. Series III, *Carboisocyclic Condensed Compounds:* Vol. 14, *Supplement.* Pp. 939s–1346s. *Tetra- and Higher Cyclic Compounds. Triterpenes.* Dora Stern, Coeditor. xxxii + 407 pp. + 50 pp. index. Illus. $40.00 single issue; $35.00 series subscription; $30.00 set subscription. Elsevier, Houston. 1952.

Volumetric and Phase Behavior of Oil Field Hydrocarbon Systems. M. B. Standing. vi + 123 pp. Illus. $10.00. Reinhold, New York. 1952.

Pile Neutron Research. Donald J. Hughes. Addison-Wesley Nuclear Science and Engineering Series. xiii + 386 pp. Illus. $8.50. Addison-Wesley Press, Cambridge, Mass. 1953.

Mechanics of Materials. Seibert Fairman and Chester S. Cutshall. x + 420 pp. Illus. $5.75. Wiley, New York. 1953.

Iodine Content of Foods. Bibliography 1825–1951. vii + 183 pp. 21*s*. Chilean Iodine Educational Bureau, London. 1952.

L. Farkas Memorial Volume. Research Council of Israel Special Publication No. 1. Adalbert Farkas and Eugene P. Wigner, Eds. viii + 309 pp. Illus. $6.00. Research Council of Israel, Jerusalem; Interscience, New York. 1952.

Hydrazine. Charles C. Clark. ix + 133 pp. Mathieson Chemical Corporation, Baltimore. 1953.

Biostratigraphy of the Maqfi Section, Egypt. Geol. Soc. Amer. Memoir 54. L. W. LeRoy. vii + 74 pp. Illus. Geological Society of America, New York. 1953.

Paleozoic and Mesozoic Arachnida of Europe. Geol. Soc. Amer. Memoir 53. Alexander Petrunkevitch. xi + 128 pp. Illus. Geological Society of America, New York. 1953.

Manual for Testing Agricultural and Vegetable Seeds. Agricultural Handbook No. 30. U. S. Department of Agriculture. vi + 440 pp. Illus. $4.00. GPO, Washington, D. C. 1952.

The Younger American Scholar: His Collegiate Origins. Robert H. Knapp and Joseph J. Greenbaum. xiii + 122 pp. $3.00. University of Chicago Press, Chicago. 1953.

The Trichoptera (Caddis-Flies) of Australia and New Zealand. Martin E. Mosely and D. E. Kimmins. 550 pp. Illus. £4, 10. British Museum (Natural History), London. 1953.

The Hopis: Portrait of a Desert People. Walter Collins O'Kane. xii + 267 pp. Illus. $5.00. University of Oklahoma Press, Norman. 1953.

An Appraisal of Anthropology Today. Sol Tax, Loren C. Eiseley, Irving Rouse, and Carl F. Voegelin, Eds. xiv + 395 pp. $6.00. University of Chicago Press, Chicago. 1953.

Digging Beyond the Tigris. Linda Braidwood. xii + 297 pp. Illus. $4.50. Schuman, New York. 1953.

Enchanted Island. Walter Lowrie. 200 pp. $3.00. Philosophical Library, New York. 1953.

Essay in Politics. Scott Buchanan. xiii + 236 pp. $3.75. Philosophical Library, New York. 1953.

The American Church of the Protestant Heritage. Vergilius Ferm, Ed. 481 pp. $6.00. Philosophical Library, New York. 1953.

Religion, Philosophy and Psychical Research. Selected Essays. C. D. Broad. International Library of Psychology, Philosophy and Scientific Method. vii + 308 pp. $4.50. Harcourt, Brace, New York. 1953.

Solitude and Privacy. A Study of Social Isolation, its Causes and Therapy. Paul Halmos. xvii + 181 pp. $4.75. Philosophical Library, New York. 1953.

The Mathematical Theory of Relativity. (2nd ed., repr.) A. S. Eddington. ix + 270 pp. $6.00. Cambridge University Press, New York. 1953.

THE SCIENTIFIC MONTHLY

APRIL 1953

Public Education in the Philippines—Footnote to the Future*

J. CAYCE MORRISON

In 1949, at the request of the government of the Philippines, Unesco sent a Consultative Educational Mission to the Philippines to make a survey of the nation's public schools. Following publication of the mission's report, in 1950, the government of the Philippines asked Unesco to send a technical adviser to assist the government in implementing Unesco's recommendations on educational administration and finance. Dr. Morrison was given this assignment. His proposals for implementing Unesco's recommendations of 1949 are embodied in a bill entitled "A Foundation Program for Financing Public Schools in the Philippines." The proposals have the endorsement of the Department of Education and the Philippine Association of School Superintendents. The proposed bill is before the appropriate committees of the Congress of the Philippines for consideration during the 1953 session.

ON August 23, 1901, the first contingent of American teachers landed in Manila. On July 4, 1946, the Republic of the Philippines took its place among the nations. Between those two dates American teachers had helped a willing people to develop the qualities of responsible citizenship. In less than half a century a people inhabiting a thousand isles, divided by nearly eighty languages, the victims of three centuries of exploitation, had found unity in an alien tongue and accepted responsibility for governing themselves. Nothing quite like this had ever happened before in the history of peoples. Probably in no other country is there such an excellent opportunity to observe the basic contribution of free public education to developing capacity for citizenship and self-government.

To understand what is happening in the Philippines, it is important to understand the longing of the Filipinos for education. This was vividly expressed by José Rizal,[1] more than ten years before the Americans arrived:

> The school is the basis of society. The school is the book in which is written the future of nations. Show me the schools of a people, and I will show you what that people is.
>
> I desire the country's welfare; therefore, I would build schools.

Almost without exception the Filipino leaders

* Based on the address of the retiring vice president, Section Q, at the Annual Meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, St. Louis, Missouri, December 30, 1952. The paper is also appearing in the *Philippine Educator*.

sought to keep their schools alive, to free them from clerical control, to make education compulsory, and to establish public schools secular and free. These ideals found expression in the four great constitutional documents of the revolutionary period.[2]

Before the Americans arrived in the Philippines, under the Spanish Education Act of 1863, approximately 1000 primary schools had been established, with an estimated enrollment of about 200,000. It was on this foundation that the Americans started to build.

Legal Basis of Public Schools

The legal basis of the present public school system in the Philippines was established by Public Act No. 74 of the American Commission in January 1901. Except for Commonwealth Acts of 1938 and 1940, the structural changes, to date, have been minor.

In 1938, the Congress of the Philippines enacted a law known as Commonwealth Act 381. This reaffirmed the responsibility of the national government for financing primary education (Grades 1–4), but shifted responsibility for intermediate education (Grades 5–7) to the chartered cities and municipalities. In order that they might carry this responsibility, the Congress required each local authority to levy a tax of 2/8 of 1 per cent on the assessed value of real estate, and made available to the local governments certain additional tax resources for the support of schools.

Finding that many of the municipalities could not maintain their intermediate schools under the provisions of C. A. 381, the Congress, in 1940, repealed the act and substituted for it C. A. 586. The latter carried over the provisions of C. A. 381 as they applied to chartered cities, but transferred to the national government responsibility for financing intermediate schools in the municipalities. As an economy measure the Congress, by C. A. 586, abolished the seventh grade, limiting the intermediate school to Grades 5–6. The debates on this measure resulted also in pressure on the Bureau of Education to experiment with the one-session, or half-day, program in the elementary school.

The War Years

The school year 1940–41 opened with about 2,000,000 children enrolled in the public schools. English was the chief language of instruction in all schools. In general, public secondary schools had been established only in the provincial capitals and a few other populous centers. They were supported in considerable measure from provincial funds and in some cases were tuition-free. The census of 1939 had reported approximately 50 per cent of the population above ten years of age as literate.

The Japanese entered Manila on January 2, 1942, and by the following June were in control of most of the provincial centers in the Philippines. Wherever they took control, the schools were made an instrument of propaganda. Children left school either from the necessity of escape with their families or as a patriotic duty. In many centers the Japanese established their headquarters in the public school buildings, which then became objects of attack by guerrillas and later by the Americans in attempting to dislodge the enemy. When the liberation from the Japanese came, nearly half the public school buildings had been destroyed, books were gone, teaching staffs were scattered, the new government was yet to be formed, and the country was desperately impoverished. Between January 1942 and April 1945, enrollment in the public elementary schools dropped from about 2,000,000 to about 200,000.

Wherever the American Army moved in to give protection, however, the first act of the local government was to open the schools. There was no money, no material for permanent buildings; there were few books and few qualified teachers available; and for three or more years children had been subjected to all the demoralizing influences of war. The national government agreed to provide a teacher wherever the people would provide a classroom. For the most part, these temporary classrooms were erected on school sites; they had woven bamboo walls, nipa shingle or thatched roofs, dirt floors, and open spaces for windows with wooden shutters. Teachers and pupils found books, paper, and pencils where and as they could. By July 1946, most of the children who had left school wanted to return. Such briefly, was the situation in public education when the new government took over on July 4, 1946.

First Six Years Under the Republic

As of July 4, 1952, public schools in the Philippines had completed six years under the Republic. No significant change had been made in their legal structure since the Commonwealth days. But significant gains had been made, and even more promising developments were in prospect.

The total annual enrollment of public elementary schools, Grades 1–6, had reached 4,000,000, with an average daily attendance of over 3,600,000. It was estimated that 80 per cent of all seven-year-old children were entering the public primary

schools and that 50 per cent of those who entered the first grade would, under present conditions, stay in school to the sixth grade. The War Damage Commission had restored a large percentage of the school buildings destroyed in the war, and local governments with the help of parents and other citizens were continuing to build temporary classrooms where needed. In 1948, teachers' salaries had been increased. Most teachers had satisfied the requirements of certification, and few new teachers were being employed who had less education than graduation from a two-year normal college course.

A number of current developments in the work of the public schools give us confidence in the future of the Philippines. These changes are finding expression as phases of the school community program, which may become the chief contribution of the Philippines to education. The school-community program has the endorsement of the Bureau of Public Schools and of the entire Department of Education. Superintendents are encouraged to promote the program, each in his own way, in terms of the needs and resources of the city or province he serves. This has led to the coordination of major educational objectives into one program, which is displaying new vitality throughout the country. In general, the school-community program is designed to improve the economic status of the people, reduce illiteracy, improve public health, develop civic consciousness, and train for the responsibilities of citizenship in a democracy.[8]

The school-community program is commonly initiated by a survey of the community, conducted by the teachers and children. Each teacher is given an area. She visits the homes, talks with the people, and reports the facts concerning the number of people, the degree of their literacy, the character of the homes, the sanitary conditions observed; the economic features of the home, such as livestock, poultry, and garden; and aesthetic features of the neighborhood that might be improved. Through the children, visits to the homes, and parents' visits to the school, steps are taken to improve conditions where such improvement may be achieved by the school's participation or its dissemination of knowledge.

The school-community program is giving a new impetus to the effort of the schools to increase the subsistence level of the common people. From the beginning, American teachers emphasized the development of school gardens. Under the Republic, the schools are expanding and giving new vitality to school gardening both as a method of education and as a means of raising the economic status of the people. This expansion takes various forms, depending, in part, upon the occupation of the people, the fertility of the soil, the prevailing climate, and the vision of those in charge. In one province, where banana culture had been more or less haphazard, the school children, in one year, set out 300,000 banana trees. In every community visited in that province the school banana plot was the best cultivated banana planting in the area. The intent is that the knowledge gained in school will be carried over into the care of banana plants in home gardens, and ultimately to the improvement of banana culture on a commercial basis. In another area visited, where the climate is specially well adapted to the growing of flowers, the schools have actively promoted their culture. Almost every home, however humble, has its own flower garden, and collectively these home gardens have become part of an industry that supplies Manila, hundreds of miles away, with flowers the year around. The most prevalent extensions of the school garden are piggery and poultry projects. The schools are obtaining from the state agricultural colleges good stock, not only demonstrating what can be accomplished through better breeding and scientific feeding, but also serving to improve the stock in the area served by the school. In mountainous areas the schools are beginning to establish goat farms. Along the coast, the schools are establishing fish ponds and hatcheries. Through showing the people how to do better the things they were already doing, the schools are helping to improve the economic status of the people of the Philippines. To the achievement of this goal, the Mutual Security Agency of the United States is making an important contribution by helping the government of the Philippines to expand and equip the vocational schools at all levels. The influence of this contribution is reflected in the work of the intermediate and primary schools.

In the school-community program, emphasis is also placed on community improvement. As teachers and other adults gain experience in working together, the school becomes a center for adult groups to discuss community problems and to consider ways and means of solving them. Much consideration is given to improving sanitary conditions, both in the homes and the community at large. Reading centers are established, in part as a means of promoting social intercourse. Consideration is given to aesthetic improvements. Improving conditions in the barrio or the municipality is the beginning of practical training in citizenship. The importance of this movement is that pupils and parents, teachers and citizens, are working together and that the school is the center of a movement

that has immense possibilities for raising the status of an entire people.

In some provinces, the school-community groups are organized to study the theory and practice of democracy at the local level. In many places, the provincial governor, the municipal mayors, and councilmen participate actively in the movement. The writer attended a conference of the leaders of school-community groups in one of the larger municipalities, where the chief speakers were the governor of the province, the mayor of the municipality, and the division superintendent of schools. The discussion, carried on in Tagalog, covered local taxation, municipal finance, obtaining civic improvements such as artesian wells, appraisal of the promises and performance of their representative in Congress, and so on. The conference was held in a large central elementary school. Time was scheduled for the delegates to visit elementary classrooms, where each class had brought, or was in process of bringing, to conclusion a term study of some important subject or problem in historical, civic, social, or economic development of the community, province, or nation. Here was a social studies program in the school, intelligently articulated with a community program that enlisted the active participation of adults, that was directed to helping old and young grow in understanding and practice of the way of life set forth in the Constitution and the declared policy of the Republic.

One would be daring indeed to predict the influence of the school-community program on the future development of citizenship in the Republic of the Philippines. There are observable beginnings that promise well. In one province, at least, many of the Huks have established their families in settled communities where their children can and do attend the public school. In a quiet way the schools are moving to win the hearts and minds of the children, and there is hope that they may win the confidence of the fathers as well.

The language problem has been and still is a deterrent to the development of a literate and intelligent citizenship in the Philippines. The 1948 Census reported about 60 per cent of the population above ten years of age as literate. Since 1901, except for the period of the Japanese Occupation, English has been the language of instruction in the schools and of the government. Under the Commonwealth, Tagalog was declared the basis for development of a national language. The practical effect of this legislation thus far has been to make Tagalog a required subject of instruction, one period daily for all pupils in all schools. But since there are about 80 different native languages and dialects in the Philippines, the development of a native language based upon Tagalog is progressing slowly. During the past four or five years an experiment has been conducted in the province of Iloilo, in the use of the native dialect as the language of instruction in Grades 1 and 2, introducing the use of English at the end of Grade 2 or the beginning of Grade 3. Emphasis is placed on drawing the parents into the school, encouraging the children to tell their parents about the things they learn in school, and encouraging parents to learn to read with their children. Experience to date indicates that by the end of the fourth grade, the children can use English quite as well as others of equivalent ability who were under English instruction during the entire four years. In addition, the children have gained some skill in reading the native language, and the school has developed a closer bond of understanding with the parents. The school-community program, thus makes a reform in the teaching of language a means of reducing illiteracy among adults, thereby drawing them into closer support of the schools.

A Proposal for Financing Public Schools

Not the least gain of the post-liberation period has been the achievement of a general agreement as to what are the current educational problems of the Philippines. This agreement has been reached through the work of the Joint Congressional Committee[4] on Education 1948-49, the survey conducted by the Unesco Consultative Educational Mission[5] of 1949, and by the continuing activities of temporary committees on education. The needs and the problems reported by these two major survey groups may be briefly summarized as follows:

a) *All classes in the primary schools should be placed on a two-session, or full-day, program.*

In 1951–52, about 60 per cent of all primary classes were on a one-session, or half-day, program.

b) *Elementary schools should be made free and available to all children, and compulsory education should be enforced to the completion of the intermediate school or till the age of fourteen.*

The present compulsory education law applies only to the primary school, Grades 1–4, and is not strictly enforced. In the sparsely populated areas there are some municipalities and many municipal districts that have no public intermediate school and a few that have no primary school.

c) *The seventh grade, abolished in 1940, should be restored.*

Both the Unesco Consultative Mission and the Joint Congressional Committee recommended that the seventh grade be restored to the intermediate school. However, a bill introduced in the 1952 session of the Congress of the Philippines proposed making the seventh grade the

first year of a five-year high school program. Unesco's technical adviser in 1952 approved the original recommendation for the following reasons: (1) public intermediate schools are much more widely distributed and, therefore, are more accessible than public secondary schools; (2) since secondary schools are currently financed chiefly from tuition fees, the number of sixth-grade graduates obtaining a seventh year of instruction in secondary schools would be much less than in the intermediate school; and (3) the program of the intermediate school is much better adjusted to improving the social-economic life of the people than is the current high school program.

d) *As rapidly as the resources of the country will permit, public secondary education should be made free and available.*

As of 1950–51, the average daily attendance in Philippine secondary schools was about 425,000. Of these, approximately 183,000 were in public and 242,000 in private secondary schools. It was estimated that a little less than 25 per cent of the youth of high school age were in secondary school, public and private. In the public secondary schools about 85 per cent of the cost of current expenses came from matriculation and tuition fees. The national government made some contribution to the support of vocational education.

e) *Reduction of class size.*

As of 1951–52, the maximum class size was fixed officially at 60 pupils. The average class size was 46 in primary grades and 40 in intermediate schools. There was general agreement that the maximum should be reduced to 40 as soon as possible. The Joint Congressional Committee endorsed a proposal for special teachers in the intermediate schools that would reduce the pupil-teacher ratio to 24–27 pupils per teacher.

f) *Levy of a local property tax in partial support of public schools.*

The Unesco Consultative Mission thought that, ultimately, 50 per cent of the cost of public schools might be financed from local taxes. The Joint Congressional Committee adhered more closely to the tax provisions previously noted in Commonwealth Act 381.

To solve the foregoing and other related financial problems the Department of Education, with the assistance of the technical adviser provided by Unesco, has drawn an Act entitled, "A Foundation Program for Financing and Improving Public Schools in the Philippines." The main provisions of the proposed bill are as follows:

1. Each chartered city and municipality would be required to levy a minimum specified tax on real estate, the proceeds from which would be used exclusively for the support of elementary schools. The municipal governments would be given the same additional tax resources for the use of schools that are now available to the chartered cities.

2. The national government would pay the difference between the cost of the foundation program and the income from the required or minimum local real estate tax.

3. The local governments would be permitted to levy additional taxes for the purpose of improving or extending the foundation program.

4. When half or more of the local taxing units had provided an improvement or extension, the national government would add such feature to the "foundation program," thereby making it available to all the children of the country.

5. The provinces and chartered cities would be required to levy a token tax of ⅛ of 1 per cent and would be permitted to levy an additional tax on real estate for the support of secondary schools.

6. The national government would reserve at least half the real estate tax potential available to local governments for the support of public schools.

The "Foundation Program for Financing Public Schools in the Philippines,"[6] if enacted into law, would be a first step in developing a permanent partnership of the national and local governments in the support of public schools. It would create a more effective working relationship between the schools and the provincial, city, and municipal governments. It would enable the government of any local unit to provide a better program than that mandated for all the schools. Over a five-year period, it would greatly strengthen the elementary school program and establish the concept that ultimately secondary schools should be free and available to all youth. It would provide an incentive to parents and others, through channels now available, to participate in the improvement of their schools and ultimately to achieve more effective participation in the planning and management of schools at the local level.

Back of all proposals for increasing the cost of public schools in the Philippines is the question of economic resources. Currently, the situation is encouraging.

Since 1946 there has been a steady growth in the national income of the Philippines. In the six-year period 1946–51 the gross national income nearly doubled. The gain for 1951 over the preceding year was about 20 per cent. In 1951 there was no appreciable increase over 1950 in the cost of living. There was substantial increase in national income from trade, manufacturing, and other sources, as well as from agriculture. Many factors may alter the trend in national income, but there are many signs that the upward trend will continue.[7]

There is deep loyalty throughout the Philippines to the purposes and methods of the American public school. Even so, there is growing realization that much of the content of courses of study and textbooks must be developed out of Philippine experience and be oriented toward solving Philippine problems. This realization of need is finding expression in the leadership of superintendents in their respective cities and provinces and in collective efforts expressed through their national association. The spirit of this leadership was well expressed by J. C. Laya, in the preface of his book *Little Democracies*.[8]

This is a book on education . . . , but it is also the diary of a creative spirit who, being inexperienced and unbroken, took the tolerance of his Director for encouragement and went out on a daring and somewhat strange search that took him through the minds and hearts and purposes of the people of an entire province. This is the story of the wonderful things he found out at the first leg of his journey. The adventure has just begun. It is an unfinished story.

One thing is sure: It is a tentative record of a fast-changing scene, a freezing of a moment that as we look on is fading into other patterns and other shapes. But even if it is evanescent it has the permanence of the recorded past and will be useful as a point of reference when we plan for the future.

For J. C. Laya, the unfinished story was ended by his untimely death on August 3, 1952; but the story he recorded will be a point of reference for hundreds of men and women in the public schools of the Philippines who will continue to write in their daily work the story of how public schools helped to create a republic and shape its service to its own people and to the world at large.

References

1. Selected quotations from the writings of José Rizal. *Rizal Day Commemoration Souvenir Program,* 22. (Dec. 30, 1951).
2. Hayden, J. R. *The Philippines—A Study in National Development.* New York: Macmillan, 466 (1942).
3. *Education in Rural Areas for Better Living* (1950); *Adult Education in Action* (1951); *Curriculum Development for Community Schools* (1952). Manila: Div. of Instruction, Bur. of Public Schools, Republic of the Philippines.
4. *Improving the Philippine Education System—Report of the Joint Congressional Committee on Education to the Congress.* Manila: Bur. of Printing (1951).
5. *Report of the Mission to the Philippines.* Paris: Unesco (1949).
6. Morrison, J. C., *et al. A Foundation Program for Financing Public Schools in the Philippines.* (Mimeo.) Manila: Dept. of Education, Republic of the Philippines (1952).
7. Abraham, W. I. *The National Income of the Philippines and Its Distribution—Report and Recommendations.* New York: United Nations Technical Assistance Programme (1952).
8. Laya, J. C. *Little Democracies.* Manila: Inang Wika Pub. Co. (1951).

ASTRONOMERS

Copernicus and Ptolemy
Had much thought for infinity.
They had a universe to chart,
A room where man might play a part.
The double suns and clusters, spheres,
Became a part of their swift years.
Their world was smaller and had bounds:
In brief, they stood on firmer grounds.
For they knew not as much as us—
Old Ptolemy, Copernicus.

Daniel Smythe

Delanson, New York

Creative Technology*

EARL P. STEVENSON

Earl P. Stevenson, president of Arthur D. Little, Inc., a consulting research and engineering organization, entered its ranks in 1919 as director of research. He became vice president in 1935, and since that date has held his present position. During the war years Dr. Stevenson was Chief, Chemical Engineering Division, National Defense Research Committee, and is now a consultant to the Department of Defense and the Chemical Corps. He is president of the Board of Trustees, Wesleyan University, and a member of the board of the National Science Foundation.

WITHIN my lifetime the greatest revolution in the long history of mankind has occurred. Had the prophet Habakkuk been our reincarnated contemporary, he might have been inspired to exclaim as of yore, "Behold ye among the heathen, and regard, and wonder marvelously; for I will work a work in your days, which ye will not believe, though it be told you." This might well be my theme, for it summarizes the achievement of creative technology, which is so largely responsible for the position of the United States today in world affairs. The quickening translation of new scientific knowledge into terms of useful devices is largely due to the development of a system for utilizing the results of science—a system of teamwork among scientists, engineers, and manufacturers.

This new methodology has come into being in the past twenty-five years. It had its beginning after World War I and began to take root as an idea in the twenties, but its growth was greatly retarded by the depression. The unprecedented technological demands of World War II provided the impetus, and the accumulated basic scientific knowledge, the nourishment, for the postwar growth of the process which integrates science and engineering.

The electrical and process industries are probably the major beneficiaries. Within my working years, the modern catalytic cracking plant has evolved out of the Burton invention. The development of 100-octane aviation gasoline was not the accomplishment of one man; it depended upon an application of fundamental knowledge in the field of organic chemistry. This gasoline is a synthetic product and not simply the result of breaking down large molecules to form smaller ones under thermal shocks. The gasoline we use in our cars today is composed of 90 per cent "new" molecules.

The chemical industry owes much of its present glamour, and certainly most of its achievements, to research and engineering. Within the past fifteen years the pharmaceutical industry has also become the beneficiary of these same developments. With the advent of the sulfa drugs, chemical therapy took a new lease on life in the promise of new specifics. The antibiotics brought about another revolution in this age-old industry. Industrial research in its new and broader conception has played a vital role in translating the laboratory disclosures of Fleming and Florey into practice.

The story of the magnetron—the tube that is the heart of radar—offers another example. As far back as the 1920s, two American scientists, working on the basis of Hertz' discoveries, found that distance to an object (in their case, the height of a specific atmospheric layer) could be measured by bouncing radio waves against it. In the 1930s, Sir Robert Watson-Watt, in charge of a small research group carrying out further measurement studies, suddenly noted that aircraft overhead also gave reflected signals. Quickly huddled under a British Air Ministry security blanket, a special research team developed practical installations: the British chain of early warning stations of 1939—which swayed the Battle of Britain in England's favor. As one Englishman has put it, "Britain had fewer fighters than the enemy—but knew where to put them."

The great advance in radar began, however, with the cavity magnetron of Randall and Booth, the prototype of which they put together with "strings and sealing wax" in a university laboratory. Sir Henry Tizard brought a prototype to America in September 1940, and within six months magnetrons were in full-scale production here. This is not merely a story of mass production, but an example of enthusiastic creative teamwork.

* Based on an address presented in the Symposium on Methodology in Engineering Research at the Annual Meeting of the AAAS held in St. Louis, December 26–31, 1952. The paper will appear simultaneously in *Mechanical Engineering*, journal of the American Society of Mechanical Engineers.

The development of the atomic bomb is probably the outstanding example of what can be done when creative effort is organized and focused upon a single objective—when engineers and scientists work closely together as a team to bridge the gap between scientific knowledge and engineering requirements.

Organized creative technology falls easily within the framework of our American political and social concepts and reflects our way of life in giving full play to the genius of the individual, in giving him freedom to exchange ideas with his fellows, and opportunity without artificial restraints for joining with others in creative work. European observers today note what that astute traveler Alexis de Tocqueville remarked about American democracy in the mid-nineteenth century: that the home training of the American child, and his subsequent schooling, engender in him an attitude toward community action that knows no counterpart elsewhere in the world. The American has a strong sense of individuality, but at the same time an urge to work with others in the solution of a common problem. The concept of civic responsibility has nurtured in the typical American an ability to act responsibly and creatively in a group—and without government decree as the motivating force.

Against the background of these introductory remarks, I should now like to develop my theme. At the outset, the methodology of engineering research requires definition. I shall assume for the purposes of my approach and limited traverse that this may be translated, in one-syllable words, into "how to get things done"—meaning to develop new techniques, processes, equipment, or products.

The term "engineering research" requires particular attention. Narrowly construed, it might mean the determination of more exact and comprehensive values and the measurement of important properties of materials in terms of specific uses. Even this restricted definition is evasive and debatable, leading as it does to the conclusion that most physical research is of this kind. It can be pointed out that, since no new laws of thermodynamics, for example, have been discovered since the pioneering work of Helmholtz, Kelvin, and Gibbs, all research in the domain of thermodynamics is, a priori, engineering research. I am sure, however, that this apparently logical conclusion would not be generally accepted.

Definitions can become an ordeal in semantics. Understanding must be reached in practice and not in terms of abstract definitions. As a general proposition, I conceive the area of engineering research to be the gap between the knowledge and understanding supplied by fundamental research and that required by the engineer in undertaking to carry out a definite assignment.

As R. E. Gibson presents the position of the scientist:

> Scientific research in its most elemental form is a very private occupation which eludes all attempts to bring it under control of conventional management. The rule for the organization of pure research is, "Don't try to organize it." It is concerned with *problems.* A research problem grows in the mind of one man when what he knows from experience, intuition or doctrine conflicts with what he observes. To resolve this conflict, he must analyze the experience he has acquired or the doctrine he has inherited, and reassure himself of their consistency and sufficiency; at the same time, he must refine the processes of his observation to insure that he really knows what he is measuring and that he has observed a *reproducible scientific fact.* This in a few words describes theoretical and experimental scientific research. When a new phenomenon is explained in terms of the system of the entities and logic which has grown gradually under repeated analysis and criticism since the time of Bacon and Newton—the system we call for short "scientific knowledge"—we say that we understand it.†

In this spirit the scientist is constantly re-examining old precepts as new problems arise through the study of nature, in the writing of books, in the teaching of students, or in the practice of the useful arts. The theoretical framework of science is seldom radically modified, but rather extended. Einstein's speculations did not invalidate the Newtonian laws of gravitation when these failed to comprehend certain observable phenomena. In this unorganized and highly individualized effort, the great edifice of scientific knowledge has been created.

Where basic scientific research might be said to end, engineering research begins. Increasingly, the engineer and the scientist appear to operate in the same area; their guiding philosophy is quite different, however. The difference in objective is in the "think" and the "thing." An example will serve to clarify this idea.

Take the scientific problem of *ortho* and *para* hydrogen, for example, particularly as it relates to the design of a system for production and storage of liquefied hydrogen. Peculiarly enough, theoretical physicists had predicted on the basis of the quantum theory that two different kinds of hydrogen molecules should exist and should have quite different physical properties—particularly at low temperatures. This peculiarity arises from the requirement that the thermal energy of rotation of molecules is quantized or, in other words, the de-

† Address to Spring Conference, Society for Personnel Administration, May 1951.

gree of excitation can have only certain discrete values—called energy levels. Furthermore, some molecules can have only odd energy levels and the others can have only even ones, depending on whether the two nuclei in the molecule have their spins parallel or antiparallel. This gives rise to two different kinds of hydrogen molecules, those with even energy levels and those with odd, known as ortho and para, respectively. Chemically, the two forms are of course indistinguishable, but physically they are considerably different—different heat capacities, heats of vaporization, enthalpies, and entropies. It might be thought that normal hydrogen at room temperature would contain nearly equal amounts of the ortho and para forms, but this is not the case. At room temperature and higher, normal or "equilibrium" hydrogen contains 75 per cent of the ortho form and 25 per cent of the para. As the temperature is lowered, the equilibrium composition slowly changes and is substantially 100 per cent para at the liquefaction temperature (20° K). If, however, ordinary hydrogen is cooled and liquefied, it retains its room-temperature composition unless a suitable catalyst is present to allow the conversion reaction to take place. In the absence of such a catalyst, normal hydrogen will liquefy with a composition of 75 per cent ortho and 25 per cent para; then, over a period of days, it will slowly convert to the equilibrium state of substantially 100 per cent para, liberating a considerable amount of heat. The heat of conversion is actually greater than the heat of liquefaction, and consequently freshly liquefied, unconverted hydrogen evaporates very quickly, even if it is contained in a perfectly insulated vessel.

I mentioned earlier that theoretical scientists had predicted the existence of ortho and para hydrogen before such a thing was found in nature; indeed this would seem to be one of the most convincing achievements of the quantum theory. It remained for the experimentalists to verify the prediction and to explore the exact consequences of the peculiarity.

But the knowledge thus gained is not sufficient for the engineer who is interested in liquefying and storing hydrogen with minimum power expenditure and vaporization loss. In order to deal expeditiously with the conversion phenomenon, he must know the rate of conversion of ortho to para hydrogen over various catalysts for many different conditions of temperature and gas pressure. He must also assure himself of a reasonable lifetime of the catalyst under the conditions that are to prevail. What of the effects of traces of impurities such as water vapor or air? Are the physical properties of the catalyst suitable? And the heat transfer properties? All these and other, similar questions can be answered only by engineering research. There is no theoretical structure that can be resorted to for this kind of information; nor are there handbooks to refer to in view of the fact that ortho-para conversion of hydrogen was only a scientific curiosity a scant eighteen months ago. Engineering handbooks are compilations of the results of many years of engineering research. The researcher of today is writing the handbooks of tomorrow.

The designer of a new piece of equipment is fortunate indeed if he can find all he needs to know in existing handbooks. Even in the more classical areas of engineering research, such as strength of materials, heat transfer, fluid flow, and wave propagation, the information is sketchy at best. With increasing frequency, as design problems become more complex, the engineer must resort to experimentation to extend the available information or test the accuracy or sufficiency of some essential bit of scientific data for use in the problem at hand. From this discussion it may appear that engineering research is just a special example of applied research. I would take no exception to this conclusion. Where the scientist has new knowledge and understanding of natural phenomena as the primary objective, the engineer is concerned with the creation of a "thing" which in the perfection of detail can be built repetitively from commercially available materials with existing skills for use and maintenance. The automobile is one of his great monuments, as measured by these standards.

Our early industrial progress depended largely upon the genius of lone inventors. These men, by present-day standards, were often neither scientists nor engineers. Their efforts were inspired by intuition and supported by the rare qualities of patience and persistence. Their methods were empirical—trial and error—and their authority came from within themselves. They were in many instances the prophets of things to come; in the nineteenth century their practical achievements laid the firm foundations of our major industries.

Leonardo da Vinci anticipated in the fifteenth century many of our modern devices—the machine gun, the aerial bomb, the tank, the helicopter, the parachute, and double hulls for ships. The first sketches of such devices as the flying spindle, the circular pulley system, a differential gear, the jack, and the water turbine were made by da Vinci. His speculations, although reflecting understanding of the basic principles of mechanics, were not embodied in useful devices for several centuries.

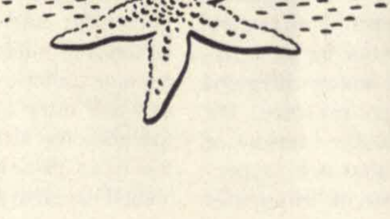
The two Stanley brothers, one of whom I was privileged to know as a neighbor, were successful inventors in several fields. To photography they contributed the first dry plate. They might well have conceived of Kodachrome or Technicolor, but I doubt very much if they could have developed these into acceptable use. Their steam-driven automobile was a great achievement, but a turbo-jet would have been beyond their reach. Such statements do not, however, deny inventive genius a vital place in the changing scene, as attested by the number of patent applications filed each year in the major fields of industrial research and engineering. The organized effort that has succeeded to the role of the Edisons and the Westinghouses makes good use of inventive talents. The fundamental scientist must often rely upon intuition for a first glance into the unknown, and use the empirical methods of the inventor where scientific understanding is for the moment lacking.

Engineering research has thus largely replaced the inventor, but his talents have been absorbed, not discarded. The increasing complexity of engineering goals demands something more than intuition and inventive inspiration.

The new working alliance between the scientist and the engineer has found one of its finest and most fruitful expressions in the development of scientific tools. A trip through our postwar laboratories—industrial, government, or university—cannot fail to impress upon anyone the greatly increased dependence of investigation upon instruments. The newer instruments of physical research were previously cobbled and put together in a makeshift way. There is now very much in evidence apparatus that has been highly engineered, with meticulous attention to details of design, making for reliability in measurements and for use by other than the most highly skilled scientist. *Fortune* magazine in its December 1952 issue notes that there are today about thirty United States firms turning out a dozen or more laboratory mechanisms that cost over $10,000. Supplying our research laboratories with engineered scientific tools has become big business. The *Fortune* article notes four of these as being in the forefront of this new era: X-ray diffraction equipment, the mass spectrometer, the ultracentrifuge, and the Collins helium cryostat. These instruments are playing a most important role in the new methodology of engineering research, of which they themselves are the product, as the tools of both the scientist and the engineer; they speak the language of both, which is numbers.

With improved communication, all barriers are breaking down. Creative technology is today largely dependent for its achievements upon a hybrid, the scientist-engineer. Which talent of this dual personality comes first in title depends on whether the individual is at a given time oriented toward the "think" or the "thing."

In this new era of creative technology, the engineer has become increasingly dependent upon the scientist, whose progress he no longer follows at a distance, but with the intimacy of the next-door neighbor. At the frontier of every art, the engineer is restricted by lack of understanding, data, or material. He may clearly perceive the direction in which improvements may be achieved or revolutionary developments accomplished, but before he can pioneer, the scientist must explore and achieve understanding. The scientist, in turn, is increasingly dependent upon the engineer for the design and construction of essential tools and for the challenge to push forward the frontiers of knowledge. This interdependence is significant for the future of both scientific discoveries and engineering progress.



Soil, Animal, and Plant Relations of the Grassland, Historically Reconsidered

JAMES C. MALIN

Dr. Malin's ecological studies have appeared several times in THE SCIENTIFIC MONTHLY, *but in his present article he appears in his proper guise as a historian. His discussion is based on a paper presented before a joint session of AAAS Section G, the Ecological Society of America, and the Grassland Research Foundation in a symposium on "The Western Range," which was held during the 1952 Annual AAAS Meeting in St. Louis. Dr. Malin is a professor of history in the University of Kansas.*

I. The Problem of Method and Point of View

WHEN a historian appears on a scientific program, it may be appropriate to ask some questions. What is science? What is history? The answer to these questions is not necessarily difficult. In dealing with the field sciences, as distinguished from the laboratory sciences, history and science may be only different facets of the same thing. Nowhere is this fact more relevant than when applied to any consideration of natural resources.

But, again, a question. What is a natural resource? The answer is that the properties of the earth become natural resources only as they involve man and are utilized by him. Without the intervention of man, although particular properties are actually present, they are latent, or unrealized. A natural resource is not determined by the properties of the earth, per se, but by the qualities of the mind of men. The first requisite of a natural resource is an idea. There are no known limits, therefore, to the multiplication of natural resources of the earth, and exhaustion of them is impossible, except, or unless, the capacities of man are exhausted—the capacities through which the latent properties of the earth are discovered and thus become properties new to man and available to his use as natural resources.[1] The record of the process by which the potential of the earth has been made actual is within the province of history, and to the study of it the historical method should apply, regardless of whether the intellectual enterprise is undertaken by the historian or by the scientist. In this context, history provides the background and prepares the setting, but at that point science may take over. Obviously, the boundary lines that have become traditional between the several accepted intellectual disciplines are artificial, and as they were adopted originally only for the purpose of making the intellect more effective through specialization, they are justified only so long as they accomplish, rather than hinder, that purpose. To achieve the goal of making the mind most effective, intellectual enterprise must possess both perspective and depth, and to sacrifice either defeats the full realization of the other. History is of particular importance in establishing perspective.

When the problem of history and historical method are introduced, the time has come to distinguish two schools of thought on the subject. First, the concept of objective history strives to reconstruct historical actuality as completely as possible, without respect to any possible use to which it might be put. Second, the subjective relativist functional notion about history holds that the only excuse for its practice is to make it useful for some present purpose. To accomplish this functional goal a selection from the whole corpus of historical actuality is made, utilizing only the so-called usable part. The great difficulty with this method is that the results are almost certain to be predetermined by the frame of reference adopted before the so-called historical investigation was begun, only those things being found, or considered applicable, that fitted the preconceived hypothesis.

The requirement of usefulness more often than not defeats itself. On the contrary, the first method described, the pursuit of knowledge as intellectual enterprise, without respect to usefulness, offers more probability of turning up something useful, although not necessarily anything that was in the mind of the investigator when he began.

II. Illustration: Roe, *North American Buffalo*

In order to reduce the problem to something tangible, the book by Frank Gilbert Roe, *The North American Buffalo: A Critical Study of the Species in its Wild State* (1951), is taken as a remarkable example of excellent history that is at the same time essential to the scientist and to the historian in more ways than the book reviewers have thus far recognized. All reviews of the book that have come to the notice of the present writer have been quite favorable, but in spite of that fact the impression conveyed is primarily that it is just a good book that should be read sometime. Certainly, without the intent of the reviewers, that is almost equivalent to damning it with faint praise; because Roe's *North American Buffalo* is a book that is of such outstanding importance that anyone interested in the grassland of North America, whether he be historian or scientist, should read it immediately, the whole of it; not only read it once, but reread it and digest the contents thoroughly. This book should be read not only for what is actually said, but it should be studied in all its implications in order to search out all the possible ramifications. Studied with thoroughness, it should become the springboard for a wide variety of investigations in several disciplines not even contemplated by its author.

Frank G. Roe is a resident of Canada, and he is not a professional historian. He became interested in the buffalo problem as a by-product of a study of the earliest roads in old England and arrived at the unorthodox conclusion that they "were probably *not* originally wild animal tracks; nor were the earliest human (Indian) trails of this continent [North America] buffalo tracks." The contradictory historical evidence relating to the buffalo led him into a fifteen-year study of the buffalo, with special reference to the part of the North American continent, north of approximately 40° north latitude. He devoted only limited attention to the country south of the Republican River. Another self-imposed limitation was to exclude scientific considerations, but no scientist should be misled into assuming that the book has no value for scientists. The facts are quite otherwise. Roe's statement of reasons is fundamental:

> In dealing with an animal now extinct as a free world species in its most characteristic native habitat, the first task is to ascertain and classify the historical evidence; and not until this has been done can biological investigation proceed with much profit.

The evidence collected appears to support the conclusion that some variation did exist within the buffalo species. In relation to the long-accepted tradition that the buffalo made general annual migrations from the south to the north and return, Roe has demonstrated conclusively that no such general movement occurred, especially north of the Republican River country, or about the 40th parallel. He concedes that some such movement occurred south of that boundary, but, as a part of the self-imposed limitation of the geographical scope of his study, he did not undertake to survey southern literature intensively.

Roe's contention, that the buffalo movements were primarily random wanderings, appears to be fully demonstrated. The Indians who were largely dependent upon the buffalo followed these random wanderings as best they could. Thus the numbers of buffalo that might occupy a particular spot could be enormous, and damage to vegetation disastrous, but the incidence was not continuous. There is still an opportunity for other investigators to study thoroughly the problem of overgrazing, drought, and dust storms under aboriginal culture.

Although Roe did not go into the problem of the consequences entailed by his conclusions, it is in this context that we learn that surface erosion by wind and water was present and upon occasion severe under aboriginal conditions—recurring drought, fires, overgrazing, and trampling by animals, especially buffalo, as well as the wearing of innumerable paths to watering places. Dust storms upon a large scale were not caused by the "plow that broke the plains."[2]

Roe has demonstrated that the migrations of the buffalo were primarily random. But there are other aspects of his treatment of buffalo movements that are not so satisfactory. He challenged the notion that buffalo changed their direction of movement deliberately on account of encountering sparse grazing, and that they sought out more productive pastures over considerable distances. Yet he accepted the view that buffalo moved between the plains and the Rocky Mountains in midlatitudes, and between the rough wooded areas and the plains in the north country. Probably he is correct in the sense that he ruled out in the first case the notion of buffalo capacity to make choices on the basis of memory or instinct akin to rationalizing from experience, but that does not explain

the behavior which he does seem to accept, that of seeking shelter in timbered areas during the winter storms or for shade from the intense heat of the sun. A suggestion is offered here that possibly guidance may be derived from the physicists' theory of the unpredictable behavior of individual particles, but the high degree of predictability in the sense of statistical probability as applied to behavior of large numbers.

Although Roe emphasized the fact that he was not a scientist and was deliberately excluding scientific aspects from his book, he did not escape making scientific blunders. One of these may be mentioned as illustrative of the importance of the historian's knowing something about science. In discussing the extensive deposits of buffalo bones scattered over the plains, which must "have been broken, crushed and stamped into the earth," he suggested that: "This also may have some bearing on the enrichment of the soil. A chemical analysis of Kansas virgin prairie soils might yield some interesting information" (p. 515, Note 116). He was unaware of the epoch-making monograph of E. W. Hilgard, as long ago as 1892, which demonstrated conclusively the fact of lime accumulation in soil of low rainfall climates, regardless of the parent materials from which they were derived.[3] Such additions to the lime content of the soil as buffalo bones or any other artificial additions of lime to lime-rich soil contributed nothing to soil properties or to productivity.

In reviewing the literature dealing with the buffalo, Roe has demonstrated that the most recent is not necessarily the best. Joel A. Allen's book, *The American Bisons, Living and Extinct* (1876), is among the earliest formal treatments and was the best of the lot. Roe demonstrated also that the scientist is not necessarily the best authority, both Hornaday and Seton being proved quite unreliable except upon limited aspects of the subject. George Catlin, an artist, emerged conspicuously as one of the most reliable observers of buffalo and Indian lore. Furthermore, Roe's study made embarrassingly clear that the medium through which a supposedly scholarly study is published is not necessarily an index of its authenticity. Again, Hornaday's monograph, the work of a scientist, published by the Smithsonian Institution, is the horrible example. This mid-twentieth-century culture is the victim of a naïve worship of formal training and specialization, forgetting too much the first principle, that competence in any field is grounded in the quality of the individual. Roe is not a professional historian, and he disavows explicitly any scientific pretensions, yet he has produced a major historical work that is fundamental to both historians and scientists. The only plea that may be appropriately advanced in this connection is that some formal discipline in history and science and their respective methodologies might have enabled Roe to produce a still better book. There is room for an argument in rebuttal, however, that the requirement of formal methodology and training might have killed all incentive to write the book.

As a result of Roe's study of the buffalo, both the historian and the scientist must largely rethink the whole problem of the interrelations of the buffalo and of man, and of many corollaries or inferences that are applicable to the grazing of domestic livestock on grass. One more point in emphasis from Roe may be permissible, one which is in a sense the major point of the present paper. Roe defined his idea of the relation of history to science, and with the qualifications given above, that declaration stands as the view of the present author:

> The first task is to ascertain and classify the historical evidence; and not until that has been done can biological investigation proceed with much profit.

III. Animal Exclusion Studies

Attention is now directed to another type of study—this one an experimental project conducted in the field. To facilitate objectivity, an English work is used, that of V. S. Summerhayes, "The Effect of Voles (*Microtus agrestis*) on Vegetation."[4] By exclusion of voles from the test plots over a period of seven years the conclusion was arrived at that the yield of dominant grasses was increased:

> On the removal of the vole attack the non-dominants, particularly the mosses, decreased in abundance, apparently as a result of the increased competition with the more luxuriant dominants. Voles therefore tend to preserve a relatively open vegetation, comparatively rich in species ["flowering plants and especially mosses"]. This is presumably effected by the direct eating or cutting up of the aerial parts of the dominants, and by the complicated series of burrows below the main surface of the vegetation, the formation and maintenance of these burrows preventing the development of large tussocks of grasses like *Molinia,* or thick-matted turf-like growth as in *Holcus Mollis* (p. 45).

The author let the matter rest with those conclusions, and refrained from any policy recommendations, but the customary policy conclusions drawn from such animal exclusion studies are that the predators should be exterminated to increase the grass yield available for livestock. Such policy conclusions do not necessarily follow. The duration of the experiment was seven years, but what might have been the result if it could have been continued

one hundred years? The central point is that soil as an object of study had no place in the experiment, yet any assumption about indefinite maintenance of the increase in yield of dominant grasses must be posited upon a parallel assumption of an indefinite maintenance of soil productivity. Questions that require answers on such a long-term basis include the status in the investigation of legumes and of deep-rooted forbs, and of the activities of animals and of the soil population, all considered in relation to the soil as an object of study. Had soil been included within the scope of the project under consideration, the seven years of effort might have meant being seven years further along on the study of the changes that occur in soil under the conditions of the experiment. What has just been said about the particular project under consideration applies substantially to similar work in the United States.

IV. Man within the Ecosystem

The process of the expansion of European culture throughout the world, a four- to five-century drive that has about spent itself, was characterized conspicuously by a contempt for the "savage" and the "backward" peoples of the globe. No branch of that culture was more conspicuous in that respect than the Anglo-American tradition. Belatedly, the situation is changing, during the mid-twentieth century in particular, and re-examination of old evidence and discovery of new facts are revealing fresh perspectives which impart to aboriginal culture a historical significance of outstanding importance. The conventional or traditional concept of the state of nature must be abandoned —that mythical, idealized condition, in which natural forces, biological and physical, were supposed to exist in a state of virtual equilibrium, undisturbed by man. The role of aboriginal man within the ecosystem must be recognized as a major ecological fact. The task of re-examination, largely historical in character, cannot be done in a day, and it has not been done for the North American grassland.

V. The Great American Desert: Semantic Problems, Myths, and Legends

In dealing with the North American Grassland historically, one of the first problems to be met is that of the semantics of the word "desert." Approached from the standpoint of the history of the usage of the word, many of the difficulties are revealed. The meanings varied widely in time, and otherwise. In the eighteenth century, and even during the early nineteenth century, good usage included the idea of an area that was deserted—especially deserted by man—therefore, desert, even though it was covered with forest. The word did not necessarily have reference to consequences of the relations of climate to vegetational cover.[5] Some forest men used the word in such a manner as to imply that a lack of trees and running water made a desert, even though a grass cover was present.[6] For the accurate interpretation of written documentary evidence, therefore, the necessity arises of determining what the original observer meant by desert, as well as the concept that existed in the mind of the person using the document.

A major myth developed during the nineteenth century, that a Great American Desert stretched across much of the western interior of the continent, and the label was placed upon some maps. That fact led to another legend that the myth of the Great American Desert was held universally, but of course that was not the case. At no time were either the literature or the maps in general agreement on the existence of a great desert or of its extent. In the monumental *Atlas of Historical Geography of the United States* (1932), edited by C. O. Paullin, ten maps were selected to illustrate the development of the cartography of the western United States, bearing dates from 1804 to 1867. Only two of these used the "Desert" label. Pioneers, eager to occupy the land, were optimistic about the possibilities of the country, as were promoters of railways to the Pacific coast, unless describing the route of a rival.

The gold rushes of 1849–59 contributed substantially to public education in the geography of the West. R. T. VanHorn, editor of the *Journal of Commerce*, Kansas (City), Missouri, on November 10, 1859, commented optimistically that the desert of the myth had retreated from Illinois westward and the gold rush of 1859 had finally extinguished it, except the Senatorial Desert, which existed only in the Senatorial Mind at Washington.[7]

To put the question more broadly, there was a general tendency for those who opposed the rapid settlement and development of the trans-Mississippi West to be receptive to the desert myth, whereas those favorable to the aggressive westward expansion were sure and determined that all that was necessary to make the grassland blossom like the rose was to let in the population. In an able editorial in the St. Joseph *Gazette*, June 14, 1854, Lucian J. Eastin reviewed explicitly this conflict in outlook, and the reversal in point of view after the annexation of the Southwest, the opening of emigrant roads, the gold rushes, and the establishment of trade routes.

VI. The Problem of Origin of the Grassland and Climate Change

Closely related to the desert myth problem is that of the "origin of the prairie." The single point dealt with here is the factor of fire, whether natural, accidental, or used deliberately by the aboriginal population. The notion of the fire origin of the grassland may be dismissed, but in transitional country, so far as climatic and local factors were concerned, fire did act generally to restrict tree growth. Recognizing that fact, some important historical conclusions are in order. During the years following the Civil War, the idea became widespread that the climate was becoming more favorable as a result of settlement. That interpretation was a quite reasonable one, if viewed against the background just indicated. Settlement eliminated fires, and woody growth spread at the expense of grass.* The assumption became easy that this would continue until the whole area would support tree growth, if permitted. The fact of the surprising extent of the spread of trees was inescapable, but the interpretation of the facts in terms of climate change was erroneous. The white occupants of the grassland did not understand the role of the multiple factors in the situation that had operated under aboriginal culture; hence, the misinterpretation of causal relations.†

VII. Soil as an Object of Study

The introduction into this discussion of the subject of soil as an object of study is a sharp reminder that the literature from which the history of the soil conditions under aboriginal culture and European man's attitudes toward them is as contradictory as the buffalo literature with which Roe dealt. Any clear and reliable understanding of the soil problem in its essential historical aspects awaits comprehensive historical treatments on a comparable scale.

Some accounts emphasize the hardness and impervious character of the soil as found under aboriginal culture, a compactness so repellent to water that the rainfall ran off into the streams, producing floods or severe erosion cutting deep channels. Thus in the upper Canadian River, about 103° west longitude, Lieut. J. W. Abert commented, in 1845, that the prairie, "baked in the hot sun, absorbs but little water. . . ."[8] Other accounts stressed the soft, yielding character of grassland geological structure, and the rapidity of erosion, by water and wind, of the unstable soils.[9]

Many of the early travelers and explorers were impressed by the activities of such burrowing animals as ants, pocket gophers, ground squirrels, and prairie dogs, and the activities of buffalo disruption of stabilized soil conditions. In 1846, on a military mission in the opening months of the Mexican War, Lieut. Abert commented on the activities of pocket gophers along the Santa Fe Trail between 96° and 97° west longitude. His journal entry for June 28 reads:

> Whenever we rode to the side of the road we noticed that our horses would frequently sink to the fetlock, and saw on the ground little piles of loose earth . . . formed by the sand rats, or gophers. . . . [Four days later he added]: The mounds of the gophers . . . were more abundant than heretofore, and in several places a number of these mounds had been so close together that the distinctness of each was completely lost in the mass, covering an area of five or six feet.[10]

This description applied to tall grass prairie, but Abert commented later on pocket gophers in the Arkansas Great Bend area. All the country in question was in the condition commonly defined as "virgin prairie," or "in the state of nature."

In the narrative compiled by the botanist Edwin James for the Stephen H. Long expedition of 1820, descriptions are given of extensive prairie dog towns in what is now Nebraska and Colorado.[11] Among the most detailed descriptions of prairie dog towns are those of Captain R. B. Marcy, covering exploring expeditions on the southern Great Plains in 1849, 1852, and 1854, especially the Red River report of 1852 which included the valley of the South Fork, a stream which the Comanches called Prairie Dog Town River.

The contradictory character of the literature, both historical and scientific, on these problems seems to call for comprehensive investigations of both types, not only of the animals, but also of soil, as an object of study under the influence of these animals, and after they have been eradicated. Sites that are known to have been occupied in Nebraska and Colorado in 1820 or in Texas in 1852 might profitably be studied to determine what influence such occupation imposed upon the soil. Sites might be selected where the date of eradication can be established, to determine what has happened to such soils without the presence of prairie dogs, or of other burrowing animals that

* In his book, *Colorado, A Summer Trip* (New York, 1867), Bayard Taylor gave a vivid description of the landscape in transition in eastern Kansas and eastern Nebraska. It is a significant document so far as it was descriptive of what he observed.

† In Book Three of his book *Virgin Land* (Cambridge, Mass.: Harvard Univ. Press [1950]) Henry Nash Smith has performed the most complete job thus far in confusing the problems of the desert and of climate change, along with land policies.

may properly be studied in the same fashion. Such studies as are suggested here require as a preliminary step the same type of comprehensive historical study that Roe gave to the buffalo. Even when it is conceded that soil is benefited by such animal activities, there is no agreement upon what degree of disturbance by animals is advantageous to long-term equilibrium.

In pursuing the ecological literature about the grassland another gap is conspicuous—the function of deep-rooted plants of the nonleguminous families. To be sure, there are many studies of roots, and noteworthy are those of J. E. Weaver and associates, but they are oriented from the standpoint of plant ecology, not of soil science. The literature of the explorers contains many references to the range and distribution of such plants, which stand as a challenge to the historically minded to investigate certain of them comprehensively in relation to soil as an object of study. An example that invites investigation is the man-root, a morning glory, *Ipomea leptophylla* (Torr.), found, according to Gray's *Manual of Botany* (1889), on the "plains of Nebraska to central Kansas, Texas and westward." It produced a root the weight of which was given as ranging from 10 to 100 pounds. Lieut. Abert described his experience with it in 1846 while waiting for high water of the Pawnee Fork to subside, in the general vicinity of Larned, Kansas, about 99° west longitude, in the hard land north of the Arkansas River.[12] A soldier spent several hours trying to dig up a specimen under Abert's direction, but the ground was so hard they finally gave up and broke it off. The stem, about half an inch in diameter, ran down about 12 inches, then enlarged suddenly to 21 inches in circumference, or about 6 inches in diameter, and extended about 2 feet deeper. Abert's comment indicated that this specimen was relatively small compared with others supposed to grow to the size of a man.

From the standpoint of soil as an object of study, what happens to soil when a root expands to 6 inches or more in diameter, displacing the soil to a depth of three feet or more? When the plant died, the root decayed, and the cavity was refilled—but how, and how rapidly, and with what effect on the soil? What was the actual floristic range and the density of distribution of this plant, and its average and maximum life expectancy? There were many other grassland forbs, with roots of smaller diameter, that penetrated the soil 10 feet or more. All these deep-rooted plants penetrated the lime accumulation zone. Abert commented that the Cheyenne Indians dug and ate the man-root. If they did it generally, they must have possessed more patience than Abert and his soldier, because the Indian had no iron tools with which to dig. Also, such digging substantially disturbed the soil.

Soil should be investigated as an object of study under aboriginal conditions as a prerequisite of scientific investigations carried out under existing conditions, or artificially controlled conditions. Such historical investigations should recognize all possible factors: aboriginal man; large animals; the smaller animals, especially the burrowing animals; insects that bury themselves in the soil; the deep-rooted plants.

Again, the *Ipomea leptophylla* may be used as an example in order to make the discussion more concrete, although prairie dogs, or pocket gophers, or ground squirrels might serve as well. Considering the extent of vegetational distribution quantitatively, this plant was engaged in a continuous soil tillage operation. New plants replaced old ones; new growth in one spot displaced the soil, while decay of old roots at another permitted the cavity formed in the soil to collapse. But from what directions: From top down, or did the sides cave in? Or both, on occasion? The vital issue is that the tillage was continuous, but without destroying the soil cover as mechanical tools tend to do, and it was to varying depths; possibly, where subsurface conditions permitted, the prevailing depths were 2–4 feet. Lesser roots penetrated much deeper. No mechanical tool has been devised for cultivation of the soil that can perform a comparable job, that can open up the soil body to a depth of 30 inches or more, and certainly none that can open up the soil to any considerable depth without destroying the vegetational cover. To what extent did these processes interrupt or modify theoretical profile-forming tendencies and the lime-accumulation zone? What happens when these factors are removed altogether by eradication programs or clear-field cultivation?

From this historical approach to the problems of the grassland the conclusion has been reached that erosion, in the much-advertised sense, is not necessarily the most important aspect of soil conservation. In any case, the critical aspects of soil conservation vary with particular spots. They depend upon time and space. But, in many respects, more fundamental than the several facets of surface erosion is more knowledge about soil in a comprehensive sense under aboriginal culture, and what happens to soil internally as a consequence of the transition from aboriginal occupation to utilization by modern society—in the transition

from natural tillage by wild animals grazing, by burrowing animals and insects, and by the influence of the native legumes and deep-rooted forbs to the twentieth-century mechanized regime.

VIII. Water Table

One contention of the present author, not yet given a full-scale demonstration, is that availability of well water for livestock and domestic purposes played a role in settlement survival during the pioneer period that may have been even more decisive than rainfall for grass and field crops. The drought decade of the 1930s focused attention upon water supplies for cities, and set geologists to work with a new vigor upon Pleistocene geology.[13] Among the results of such research was the conclusion that the water table generally was essentially stable, varying temporarily with climatic fluctuations and local circumstances. Soil science placed a new emphasis upon the water table, assigning to it a role as a soil-forming factor.[14] Thus, by a rapid succession of events, the original proposal, the study of the relation of well water to pioneer settlement in the grassland, has been given a more fundamental significance expanding into the far broader issue of the water table in relation to the whole problem of human occupance of the area. The subject is so large as to offer research opportunities for a number of students equipped for the task.

IX. Mesquite

Another aspect of the problem of man within the ecosystem may be illustrated by reference to an article "Man vs. Mesquite," in *Life* magazine, August 18, 1952. The caption under the map read: "Mesquite march during last 100 years has taken it from small riverside areas in which it grew in 1850 to the 75 million acres it now covers . . ." (p. 69). Inquiry concerning the authority for the map brought the answer from the editors that

> A century and a half ago, there was hardly any [mesquite] in the U. S., but during the next fifty years it was brought into this country from Mexico by Spanish ponies and by wandering herds of wild buffalo. So that, in 1850 (as shown by the map) scattered stands of mesquite were growing along the creeks and river beds of the southwest. This first generation mesquite, however, was exceedingly sparse . . . during the great cattle drives of the second half of the nineteenth century (roughly 1860 to 1880), the cattle intensified the mesquite in the areas along the watercourses, and extended it out onto the plains away from the creeks and streams, and since then mesquite has fortified its hold on the southwest to cover the area shown in the 1952 distribution on the map. . . .[15]

Another statement on the mesquite history is that in the Clements-Shelford textbook *Bio-ecology* (1939), where mesquite in the costal and mixed prairies was attributed to a disclimax induced by overgrazing which took the form of "a savanna of mesquite and cactus" (p. 279). And in still another place, V. E. Shelford declared:

> For example, the cattle business of the United States had its beginning in the gulf coast tallgrass prairie. This is an area almost universally mapped as mesquite—chaparral or savannah and regarded by many as having been that type before the white man came to the area. On the contrary, since cattle eat the mesquite beans and fail to digest them, they spread the seed widely and may be responsible for the entire savannah. It is well known that the mesquite has been spread from south central Texas into west central Oklahoma by this method.[16]

Some historical data may now be brought to bear upon the mesquite problem in order to establish some factual landmarks. The Stephen H. Long expedition of 1820 found mesquite in the Canadian River country, and Edwin James, who prepared the report of the expedition is credited with the first public notice of the mesquite tree.‡ Lieut. Abert found "an abundance of mesquite" in 1854, growing about 103° west longitude as a shrub about 5 feet in height in what is now northeastern New Mexico above the headwaters of the Canadian River.[17] The R. B. Marcy expedition up the Canadian River, in 1849, found mesquite just east of the Llano Estacado escarpment, the journal entry stating that "We found a great deal of the small mesquite . . . today."

Marcy's return route from Santa Fe, in 1849, turned southward down the Rio Grande to Doña Ana, thence eastward to the Pecos River down that stream to the crossing; then, skirting the escarpment of the Llano Estacado, he struck northeastward across Texas. The mesquite was brushlike in the country west of the Pecos, but increased to small tree size at that river, and eastward as the ascent was made into the high plain and in the Big Spring area it attained large tree size. From the latter point northeastward, Marcy's map indicated mesquite timber. The second day after Big Spring, the route led "over rolling and rather broken country, of good soil, and covered on each side with large mesquite trees." Near what he miscalled the Double Mountain Fork of the Brazos River, Marcy recorded: "We have been travelling through groves of mesquite timber, with a beautiful carpet of grama grass underneath, nearly all day." On the next day, on the south side of Double Mountain Fork, he continued over

‡ R. B. Marcy recognized this fact in his report on the Brazos River expedition of 1854, quoting from John Torrey, by whom the mesquite species collected by James was described and named *Prosopis glandulosa*.

as beautiful a country for eight miles as I ever beheld. It was a perfectly level grassy glade, and covered with a growth of large mesquite trees at uniform distances, standing with great regularity, and presenting more the appearance of an immense peach orchard than a wilderness. [Heading toward the Brazos River above the mouth of the Clear Fork] The mesquite wood and grass continued very abundant. . . .

Four days later, just before crossing the divide into the watershed of the West Fork of the Trinity River and west of the 98th meridian, mesquite and oak openings were reported, with occasional prairies. In summing up the estimate of his line of march as a route for the Pacific Railroad, Marcy reported 200 miles "over a gently undulating country, with prairies and timber," springs and streams, "in many places covered with large groves of mesquite timber, which makes the very best fuel," and later he made a more positive commitment to the existence of "an inexhaustible amount of mesquite timber, which, for its durability, is admirably adapted for use as sleepers, and for fuel."[18]

In 1852, Marcy explored the headwaters of the Red River. When west of the 101st meridian on the South Fork of that stream he wrote:

We find much more mesquite timber upon this branch of the river than upon the other. Indeed, I have never seen much of this wood above the thirty-sixth degree of north latitude; but south of this it appears to increase in quantity and size as far as the twenty-eighth degree. Upon the Canadian river I have observed a few small bushes; but the climate in that latitude appears too cold for it to flourish well.

In the same report, in his discussion of the Pacific Railroad by the southwest route of his exploration of 1849, Marcy wrote that after crossing the Brazos

the road skirts small affluents of that stream and the Colorado for two hundred miles. . . . Here and there prairies present themselves, but this section is for the most part covered with a growth of trees called mesquite, which stand at such intervals that they present much the appearance of an immense peach orchard. They are from five to ten inches in diameter, their stocks about ten feet in length, and for their durable properties are admirably adapted for railway ties, and would furnish an inexhaustible amount of the very best fuel. . . .[19]

In 1854 Marcy explored the headwaters of the Brazos and Big Wichita rivers. From Fort Belknap, heading west of north, they passed over "rolling country, covered with groves of mesquite trees." The next day they crossed tributaries of Trinity River, "all of which were wooded with mesquite, and occasionally a grove of post oak seen, with here and there a cotton-wood or willow tree along the banks." Later, "On leaving the Wichita, we travelled south towards the Brazos for six miles through mesquite groves. . . ." From a low mountain near the Brazos, Marcy described the scene: "Towards the east from this elevation nothing could be seen but one continuous mesquite flat, dotted here and there with small patches of open prairie, . . ." and on the next day: "The country we are now passing is gently undulating and covered with mesquite trees."

By the time Marcy made this expedition he was much impressed by the mesquite and wrote a rather comprehensive summary of the subject some three to four pages in length:

In the journeys I had made before upon the plains, I had observed the mesquite tree extending over vast tracts of country, and I had noticed some of its useful properties, such as its durability and its adaptation for fuel, but I was never so fully impressed with its many valuable qualities as during the past summer.

It covered a great portion of the country over which we travelled. . . .

It was at this point that Marcy acknowledged that Edwin James, of the Long expedition of 1820, had given mesquite the first public notice. In commentary upon the range of distribution, Marcy admitted limitations of information, but east of the Rocky Mountains he defined its limits as between 97° and 103° west longitude, and between 28° and 36° north latitude; but west of the Rio Grande the mesquite flourished best in the valley of the Gila River. In the plains, however, he remarked that the size diminished north of 33° and to mere bushes at its northern range limits of 36° north latitude.§ In its tree form, it ranged in size from 4 to 15 inches, and was not more than 20 feet in height, and furthermore was "much used for building in southern Texas and Mexico," being well preserved in the ruins of old buildings. And then Marcy recorded information critical to the ecological problem, reporting that mesquite often grew "upon the most elevated arid prairies, far from watercourses," but it would grow only upon good soil, and that settlers competed for mesquite land.[20]

A second account of the Marcy Brazos exploration is available in the book of W. B. Parker, a civilian. In many respects Parker's version is similar to Marcy's, but variation in presentation of the scenery affords some further enlargement of perspective. Between the Cross Timbers and the Little Wichita, on July 11, the entry read: "The country we had been passing over, since leaving the Cross Timbers, was a rolling prairie, very thin

§ Marcy did not leave any account of having explored north of that limit, so he was indicating in part at least only the limits of his firsthand knowledge.

in soil and timber very scarce. At this point we began to find the mesquite trees in great abundance." Their size was given as 4–15 inches in diameter and not more than 20 feet tall. These were the same specifications as those given by Marcy. In addition to its qualities as fuel (burning like hickory wood), Parker added, "and not the least is its durability for building purposes— . . . invaluable to the future settlers."

An entry five days later recorded "ascending in a northwest course, a rolling country, covered with buffalo grass and mesquite timber. . . ."‖ Three days later, approaching the Little Wichita, a belt of timber marked its course, "and in front the wide prairie with its yellow coating of buffalo grass, studded with the pale green mesquite, a beautiful combination for a landscape painting." Again, a few days later: "Our course was Northwest, and ascending gradually, we came upon a very extensive plain, covered with buffalo grass and mesquite timber." Later on, a course south from the junction of the three prongs of the Little Wichita, they "entered an extensive plain covered with thin coarse grass and stunted mesquite timber." Upon arriving at a spur of the Llano Estacado, they ascended it "to a broad level plain . . . covered with buffalo grass and mesquite trees, and extending as far as the eye could reach in a perfect level toward the dim cloud like mountains at the head of the Brazos." Arriving at the escarpment of the Llano Estacado August 3, they climbed to the top, and looking eastward from an elevation estimated at 600 feet above the country below:

> The view was the most extensive and glowing in the sunset, the most striking that we had enjoyed during the whole trip, combining the grandeur of immense space—the plain extending to the horizon on every side from our point of view—with the beauty of the contrast between the golden carpet of buffalo grass and the pale green of the mesquite tree dotting its surface.[21]

Admittedly, the foregoing survey does not cover anything like all the literature, and certainly there is no intent to exaggerate the extent of mesquite occurrence, but it makes abundantly clear the fallacies widely held about the mesquite problem, especially those in evangelical conservation circles. In order to bring this discussion to a focus, a few tentative conclusions are outlined, derived from the limited historical data cited from the reports of the Long and the Marcy explorations. First, in a floristic sense, the geographical range of distribution of mesquite (*Prosopis* spp.) is about the same in 1952 as at the opening of the nineteenth century, or 150 years ago. Possible extensions of floristic range appear to be a minor aspect of the problem. Second, in a vegetational sense, the quantity of mesquite at the midpoint of the nineteenth century was substantial, and was not limited to the banks of streams; upon occasion mesquite occupied broad plains and rolling hills in west and north central Texas as far west as the Llano Estacado. Repeatedly the Marcy descriptions of the country indicated extensive reaches of mesquite savannah, with occasional patches of open prairie. Such language appeared so often, and so explicitly, as to be both significant and important. Third, in an ecological sense, the focus of interest is the change in the behavior, or growth form, of the mesquite during the century 1852–1952. As an ecological fact, the nature of mesquite occupance in much of the region under review changed from a savannah to a tangled jungle, in places almost if not quite impenetrable. The outstanding ecological problem, then, is to find an explanation of the how and the why of this change in growth form of mesquite and its associates. An accurate historical study of what has happened, establishing in fuller detail the facts of floristic range limits, quantity of vegetation, and form of growth, prior to the time the Indians handed the land over to the whites, may put the ecologist and the range manager in a position to attack the question.

A fourth and fundamental conclusion is the full acceptance, as of long standing, of the mesquite occupance of the floristic range just indicated. Marcy gave the size of mesquite as ranging from shrubs to 15 inches in diameter. This in itself is proof of long establishment. Further evidence of the long duration of mesquite occupance in southern Texas, the portion inhabited by European culture, was the reference to mesquite timber found in the ruins of old buildings. In 1884, V. Harvard compiled a growth-age table for mesquite: a trunk diameter of 7–8 inches, 30 years; 8–10 inches, 50 years; 10–12 inches, 75 years; and over 12 inches, more than 100 years old.[22] According to such a calculation, a diminishing rate of increase of diameter with age must admit, for a 15-inch diameter, a life span of 150–200 years or more. As of 1952, that would carry mesquite occupance of northwest central Texas back in time 250–300 years—possibly more, on the basis of the Marcy evidence. This does not take into consideration the possibility that earlier trees may have grown, died, and disappeared prior to those he was describing.

So far as the buffalo and other wild animals operated as a factor in scattering mesquite, they

‖ Clearing up of confusion in the nomenclature of grasses would require a separate study.

wandered over the whole area for centuries before 1800. The accounts of the Coronado and De Soto expeditions record buffalo in the area about 1540–41.

So far as domestic cattle drives or domestic overgrazing were factors, according to the census of 1880, neither operated generally in the country west of the 100th meridian prior to about 1879. The cattle drives northward during the 1850s followed a path just west of the Arkansas western boundary, many crossing the Missouri River below Kansas City. The cattle business in the plains proper awaited the breaking of the Comanche-Kiowa Indian Barrier.[23]

The savannah form of vegetation was found in other parts of North America when Europeans took over the land from the Indians, and studies of it elsewhere may be profitable to establish perspective. N. S. Shaler, by profession a geologist, but by avocation a historian of the Kentucky country, long ago attributed the prairie condition of much of the area east of the Mississippi River to fire, occurring naturally, by accident, or as an instrument used deliberately by the Indians. Shaler did not make the mistake of assigning to fire the whole responsibility. In some areas, especially westward as the rainfall diminished, climate was held to be decisive. But in the Kentucky country, what Shaler described as essentially a savannah stage was a preliminary step in the process of reducing a dominantly forest area into a prairie. He associated the Indian practice with the eastward migration of the buffalo sometime after the year A.D. 1000 and suggested that had European intervention been delayed another 500 years, the prairie might have been extended to the Alleghany Mountains. Although Shaler's is a rather extreme view, Roe's study of the buffalo gives support to the factual portion of Shaler's general contention.[24] Shaler's dating of the arrival of the buffalo was established by excavations he had made in 1868 around the salt springs at Big Bone Lick, Boone County, Kentucky. In succession from about glacial times toward the present, bone deposits accumulated, the modern buffalo species occupying the top position—in time, later than the Mound Builders, who were not acquainted with the buffalo.

In the state of Mississippi as of the late 1850s, E. W. Hilgard wrote of the country as received from the hands of the Indians:

The herbaceous vegetation and undergrowth of the Longleaf Pine Region is hardly less characteristic than the timber. Whenever the regular burning of the woods, such as practiced by the Indians, has not been superseded by the irregular and wasteful practice of the later settlers, the pine forest is almost destitute of shrubby undergrowth, and during the growing season appears like a park, where long grass is often very beautifully interspersed with brilliantly tinted flowers (p. 349).

The same writer, at another place, continued the theme under the head of "Pasturage in the Pine Woods":

In their natural state, as received from the hands of the Indians, the Pine Woods were one great pasture—as, in thinly settled regions, they still are. Nor is it, generally, the ranging of cattle which has destroyed the pasturage in other regions, but simply the injudicious burning of the woods, at seasons when the fire would destroy not only the dry leaves, but also parch the *heart* and the *roots* of the grasses. It would seem that in a region comparatively poor in agricultural resources, the maintenance of pasturage should be considered a matter of national importance. The Swiss, being unable to cultivate profitably their mountain slopes, have converted them into pastures, these form the basis of their national wealth. Why this should not be so with the inhabitants of the Pine Woods, I have been unable to discover, it is certain, however, that the pasturage of that region, is disappearing before the fires at a fearful rate, and that those who heretofore have relied on the range, during all but a few weeks in winter, for the support of their cattle, will soon be compelled, as many are now, to raise feed for them on their poor soil, which, at present, will but just furnish comfortably the prime necessities of life for the population itself. The beautiful park-like slopes of the Pine Hills are being converted into smoking desert of pine trunks, on whose blackened soil the cattle seek more vainly every year, the few scattered, sickly blades of grass, whose roots the fire has not killed.

The preceding paragraph was descriptive of past and present. Hilgard then discussed policy and procedures in terms of management:

It is not the province of this Report to suggest municipal regulations by which the burning of the woods at improper seasons might be prevented, or at least, rendered of less general occurrence; the evil, however, is a crying one to the mind of every candid observer, and the destruction of national wealth caused by it is so enormous as to deserve no less attention certainly, than the improvement of soils. However convenient and effectual may be the burning of the dry grass in order to render the young growth accessible to cattle, that advantage is certainly purchased very dearly at the cost of its total destruction within a few years—a policy little better, in fact, than cutting down a fruit-tree for its fruit; which appears more especially irrational when we consider how easily the advantage could be reaped without incurring the enormous waste, by a regular system of burning at times when, as after the first autumnal rains, and more especially in early spring, the ground is too wet to allow of injury to the roots, while yet the grass and weeds may be burnt off low enough to serve all practical purposes, and to destroy, at the same time, the Black Jack and Post Oak undergrowth, which is equally fatal to the range, with the fire itself. For the latter purpose, the burning in early spring, when the sap is rising, would be the most favorable time.[25]

In a study¶ entitled "The Recent Intrusion of Forests in the Ozarks," Beilmann and Brenner dealt particularly with the eastern and northern portions and concluded that "Within historic times this vast region was a prairie, or at least park-like in that the trees were widely spaced and confined to the water-courses and drainage-ways." In explanation of the change from prairie to forest in this transitional region they included among the factors "the extremely important rôle of fire in the perpetuation of the grassland at the expense of the trees."[26]

In 1939, H. C. Hanson summarized much of the modern research literature on the effects of fire, especially upon trees. In general, he indicated that in transition country the effect of fire was to discourage trees and favor grass, yet he warned that this was not necessarily the case, as fire increased the sprouting of some woody plants. White pine was badly damaged or destroyed by fire, but long-leafed pine was resistant, fire contributing to the savannah form of vegetational structure under some conditions.[27] Braun-Blanquet, the leader of the Montpellier school of plant sociology, in Europe, took the position that "Fire is particularly destructive upon very thin, sterile soils and especially in the transitional region between forest and prairie, where both types of vegetation are struggling for control."[28]

The role of aboriginal man in influencing the mesquite problem has not been given an all-out investigation. Insect infestation should not be ignored.[29]** Any pretense at drawing conclusions now would be premature, but the preliminary analysis stated here should suggest possible investigations. Such experimental work as has been done indicates that the problem is complex, and no easy solutions are to be anticipated. The point of this discussion is to emphasize the fact that the historical perspective on the whole question is seriously deficient, and that there is need of a comprehensive historical re-examination of the mesquite problem on a scale comparable with Roe's *North American Buffalo*.

¶ Some adverse criticism may be made of the Beilmann-Brenner study, although the major conclusions would not be changed. First, a more critical examination is in order of some documentary material used. For example, modern scholarship does not accept the view that the Coronado expedition of 1540–41 reached the Ozark country, and therefore the accounts of that expedition have no place in the evidence supporting the prairie interpretation of the Ozarks. Second, no discussion is included of whether earthquake disturbance may have been a contributing factor through local topographical and drainage changes, or ground-water levels. To be considered especially would be the disturbance of December 1811–March 1812, rated by geologists as of an intensity equal to the San Francisco earthquake.

The disappearance of salt licks or comparable accumulations of salt occurred elsewhere, so the assumption of a climatic change is not necessarily essential to the Ozark phenomena. Local changes in drainage and ground water incident to white occupation need more careful investigation, not only here, but as a general problem.

** Bartlett reported that "The tree seems to suffer from the attacks of insects in a similar manner with the locust." Harvard pointed out that insects laid eggs in the mesquite seeds, which destroyed germination.

X. Sagebrush and Cactus

The sagebrush problem is similarly the subject of contradictory treatment in the printed documentary material. Likewise, the tendency is to attribute sagebrush to overgrazing, in spite of the fact that many of the most significant descriptive accounts of the earliest explorers and travelers run to the contrary.[30] An interesting illustration is the following from a letter, written July 2, 1854, by the Rev. W. F. Boyakin, en route from St. Joseph, Missouri, westward, to a correspondent in St. Joseph:

> the whole country is from the South Pass to this place, one boundless sandy plain, for miles every way, stretching as far as the eye can see, with nothing but the wild sage to break the monotony; over which roll oceans of sand uplifted by the roughest winds, fairly darkening the horizon from ten to four o'clock every day, making traveling truly disagreeable.[31]

The cactus problem falls into the same category of treatment as sagebrush in most range conservation literature. But studies by C. W. Cook, and by G. T. Turner and D. F. Costello, demonstrated that the cactus infestations were not the result of overgrazing, but were related to insect-climate-plant relations.[32] Overgrazing must bear justly the responsibility of a number of evils, but it has become a convenient scapegoat for a multitude of situations where the proper answer should be "Nobody knows."

XI. Tame Grass

As Americans, derived from English and continental European stock, were primarily a forest people, when they met the grassland of the interior of the continent, they misunderstood it in many ways. Forest man's concept of grass was conditioned largely by his experience where desirable pasture and hay grasses were not generally native soil cover, and had to be cultivated like any other field crop. That outlook upon grass persisted tenaciously after the grass country was actually being occupied. At least three variant misconceptions,

separately or in combination, are important to historical perspective: First, that prairie and plains grasses were inefficient in their utilization of soil and available moisture; second, the conviction that the prairie and plains grass cover always changed fundamentally in composition under domestic pasture; third, that prairie and plains grasses would not survive domestic pasturing. Forest man proposed and eventually undertook to introduce and cultivate in the west the tame grasses he had learned to depend upon in the forest country—timothy, orchard grass, bluegrass, etc., and the clovers—and to search for still better grasses.

Thus Edwin James wrote as follows:

> There can be little doubt that more valuable and productive grasses than the native species can with little trouble be introduced. This may easily be effected by burning the prairies at a proper season of the year, and sowing the seeds of any of the more hardy cultivated gramina. Some of the perennial plants common in the prairies will undoubtedly be found difficult to exterminate, their strong roots penetrating to a great depth and enveloping the rudiments of new shoots placed beyond the reach of fire on the surface. The soil of the more fertile plains is penetrated with such numbers of these as to present more resistance to the plough than the oldest cultivated pastures.[33]

In his "Notes on Nebraska" printed in 1852, Thomas Jefferson Sutherland, a Nebraska Boomer, agitating the opening to settlement of the Indian country, the present states of Kansas and Nebraska, advocated the planting of bluegrass, "On all of the lands of the eastern part of the Territory bluegrass in luxuriant growth may be produced; and there are spots of land scattered all over the plains of the west, possessing the requisite fertility of soil, for the growth of bluegrass, in any desired perfection."[34]

In 1883 Shaler published a paper on the "Improvement of the Native Pasture-Lands of the Far West." He advocated a search of other areas of the earth having similar characteristics, but also he made the following statement: "With the poorest grasses there are generally wide interspaces between the tussocks of high growing species. If these intervals could be filled with other forage-plants, the consequences would be a greater amount of food per acre. . . ."[35]

In Kansas, E. M. Shelton, professor of agriculture, 1874–90, at the Kansas State Agricultural and Mechanical College, was in most respects an unusual man, but he could not rid himself of the notion that tame grasses must be introduced to replace the native wild grasses. At the same institution, in 1887, W. A. Kellerman challenged the assumption that the composition of the native grass cover was undergoing a change, the tall grasses driving out the short grasses. He doubted whether the vegetation was changing.[36] The form of his remarks indicates that he was thinking especially of floristic range of distribution rather than quantitative density of the several species within the vegetational structure under fluctuations of climate.

Again, since the drought period of the 1930s much of the same debate, with suitable variants, has been carried on in connection with regrassing programs in the Western range country. For the most part, the decade of the 1940s has been favorable weatherwise for such operations, but prolonged drought may compel some revaluations. Once more, it might be appropriate to urge that comprehensive historical studies of the whole problem of grass introduction into the grassland are yet to be done.

The quotation already given from Edwin James' tame-grass proposal provides an excellent springboard for discussion. So far as a bare suggestion of the introduction of tame grasses was concerned, the first sentence quoted would have been sufficient, but the remainder of the paragraph provides the highly significant context, the clues to the conceptual equipment with which James viewed the problem.

The agronomist in James suggested how the seeding operation might be accomplished with the least effort—burning. But as an experienced forest man he recognized the possible difficulty of killing certain deep-rooted plants by the use of fire. Also as a forest man, who was evidently acquainted with plowing among roots of trees and brush, he was impressed by the numbers and formidable character of woody root growths of the plains country. As a technologist, James envisioned the difficulties to be met in attempting to turn such plains sod with the iron-shod wooden plow or the cast-iron plow of the period. The numbers of the roots and the "resistance to the plough" gave him pause. Furthermore, from an agronomic point of view, James was thinking of clear-tilled fields of grass, in which a simple-stand crop was to be grown—free of "weeds," of course. But as an ecologist James was deficient, in spite of the remarkably accurate and comprehensive character of his observations, in recognizing and recording all these facts about the strange country he was visiting for the first time. The thought did not occur to him, apparently, that the presence of these plants in such numbers, and the character of their woody roots and their deep penetration constituted a veritable ecological system, and that the wide spacing of the grass plants on the surface,

dividing space with the many species of forbs, was an integral part of that system. Neither did the thought occur to him, apparently, that to destroy any part of it would work a fundamental change in the whole system, soil and all, as deep, at least, as the deepest penetration of any root. This was a century before Tansley formulated the concept of the ecosystem, and for all of James' remarkable insight into so many things essential to this grassland system, so strange to his forest mind, he did not anticipate anything of the larger concept.

XII. Conclusion

As the central purpose of this paper is to point out the role of history in a research program,†† the discussion may properly return to the question of method. Casual or random excursions into historical material to find data that appear to fit a preconceived frame of reference, or to serve a particular purpose, are not only not sufficient, but such procedure is more likely than otherwise to lead to erroneous conclusions. Although the contention may appear paradoxical, to serve their purpose historical studies must have no purpose. In an immediate functional sense they are useless, and must be useless in the same sense that Max Planck said of science: "Scientific discovery and scientific knowledge have been achieved only by those who have gone in pursuit of them without any practical purpose whatsoever in view."[37] This dictum applied to science is equally valid as applied to history.

The documentary evidence for historical studies of the kind proposed here is so contradictory and so fragmentary that the strictest precautions and safeguards must be exercised in its use. It is possible to find evidence, especially when removed from context, to make a show of proof for almost any predetermined conclusion. If "quickie research" should arrive at a sound conclusion, it would be purely accidental. Sound and comprehensive studies are more likely to require a commitment to many years of systematic collection and analysis of data in full context. Furthermore, data must be tied explicitly to time and place. Each spot is unique in an absolute sense. No one can predict what time a historical project may require for completion. Although, upon occasion, a few months may suffice, the minimum requirement is more likely to be measured in years, or a long lifetime. In a perfectionist sense a historical project can never be finished. But, within the realm of the possible, historical work, as intellectual enterprise, can be so comprehensive and complete as to render difficult, without danger of immediate exposure, any flagrant misuse of evidence by propagandists. History need not be written by historians, but whoever writes it must assume the obligation of doing it by the most rigorous historical methods.

Two other conclusions are pertinent. Every vegetational map must be dated historically. By that statement is meant that any description of vegetation is valid only for a particular historical time, and what exists at that particular time is the product of the whole situation, which must include man, whether he be primitive or so-called civilized. The other conclusion is closely related, because the vegetation of any specified time and space is an aspect of the ecosystem. If upon no other grounds, the recognition of man as a factor in the system precludes any possible recognition of the concept of climax for vegetation, for animals, for soil, for man, or for the ecosystem as a whole. Also, the traditional concept of succession must be revised. Change is the overriding fact, but disturbance of any rigid, orderly sequence is incessant, and, obviously individual events, whether acts of nature or of man, as they impinge upon any particular spot, are unpredictable. A temporary tendency toward establishment of a "steady state" is certain of interruption by the intervention of unpredictable events, and, for emphasis, man must be specified explicitly because of his characteristic of worrying about the future and of devising ways and means for trying to manipulate nature. What is said about vegetational mapping applies similarly to animals, to soil, and to man himself. The state of the ecosystem at any particular moment is the product of the factors of the past that have shaped it; and the state existing at that specified moment is the parent material upon which, or through which, succeeding states are formed—an indeterminate system.[38]

The present paper is focused upon the past, which is peculiarly in the jurisdiction of history. But there are no Rothamsteds in the United States —experimental areas, with carefully kept records of land use over more than a century. It is time such fundamental experimental areas were being set up in order that the same statement cannot be made in 2053.

The ideal of intellectual enterprise, whether history or science, is to attain at one and the same time, both depth and perspective. To attempt either without the other can lead only to futility.

†† Since this was written a Unesco report has been issued submitting some significant recommendations relative to research programs for arid and desert regions: *Arid Zone Programme*: Report of the fourth session of the Advisory Committee on Arid Zone Research. Royal Society, London, 29 September–October 1952. Unesco/NS/103 Paris, 31 October 1952.

In many respects, twentieth-century specialization has reached that barrier, and in some areas more seriously than in others. The development of ecology, or the ecological point of view, is a healthy recognition of that fact and an earnest of a determination to do something about it.

Although the fashion has been set by major segments of the intellectual world to dismiss Aristotle and the ancient philosophers as outmoded, there can be no harm done, and possibly some good can be done, in reminding moderns of the great Aristotelian concept expressed in the phrase *in potentia*. Whatever line of descent from Aristotle one may choose to follow to the present, a positive philosophical outlook is still defensible, grounded in that concept. The potentiality of man to solve problems has not yet been exhausted, and the potentiality of the resources latent in the earth to be brought into the horizon of usefulness is still beyond the power of man to conceive. The key to the situation is not the earth, but the minds of men determined to realize their own potential in act.

References

1. MALIN, J. C. *Sci. Monthly,* **70,** 295 (1950); *Grassland Historical Studies.* Lawrence, Kan.: Author, Vol. 1, chap. 1 (1950); WHITTLESEY, D. *Ann. Assoc. Am. Geograph.,* **35,** 1–36 (1945).
2. ———. *Kansas Hist. Quart.,* **14,** 129, 265, 391 (1946); McCOY, I. *Ibid.,* **5,** 365, 366, 371, 372 (1936); WEDEL, W. R. *Trans. Kansas Acad. Sci.,* **50,** 1 (1947).
3. HILGARD, E. W. *USDA Weather Bureau Bull.* 3, Washington, D. C.: GPO (1892).
4. SUMMERHAYES, V. S. *J. Ecol.,* **29,** 14 (1941).
5. MURRAY, J. A. H., Ed. *A New English Dictionary on Historical Principles. . . .* Oxford: Univ. Press, 3, 240 (1897).
6. MARCY, R. B. *Explorations of the Red River of Louisiana in the year 1852. . . .* Sen. Ex. Doc. 54, 32nd Congress, 2nd session, Public Doc. 666. Washington, D. C.: Robert Armstrong, Public Printer, 99–100 (1853).
7. MALIN, J. C. *Grassland Historical Studies,* Vol. 1, 135–8 (1950).
8. ABERT, J. W. *Journal of Lt. J. W. Abert, from Bent's Fort to St. Louis.* Sen. Ex. Doc. 438, 29th Congress, 1st Session, Public Doc. 477. Washington, D. C.: Ritchie & Heiss, Senate Printers, 28 (1846).
9. MALIN, J. C. *Grassland Historical Studies,* Vol. 1, chap. 2 (1950).
10. ABERT, J. W. *Notes of Lt. J. W. Abert . . .* [*on*] *natural history. . . , during the Journey from Fort Leavenworth to Bent's Fort.* House Ex. Doc. 41, 30th Congress, 1st Session, Public Doc. 517. Washington, D. C.: Wendell & Van Benthuysen, 387, 392 (1848).
11. LONG, S. A. *An Account of an expedition from Pittsburgh to the Rocky Mountains, performed in the years 1819 and '20. . . .* From Notes of Maj. Long, Mr. Say, and other gentlemen of the Exploring Party. Edwin James, Comp. (Philadelphia, 1822–23). In R. G. Thwaites, (Ed.), *Early Western Travels, 1748–1846,* Vol. 14–17. Consult index under "Prairie Dog."
12. ABERT, J. W. *Notes of Lt. J. W. Abert. . . ,* 399.
13. FRYE, J. C., and LEONARD, A. B. *Pleistocene Geology of Kansas, State Geological Survey of Kansas,* Bull. 99. Lawrence: Univ. Kansas Press (1952).
14. JENNY, H. *Soil Sci.,* 61, 375 (1946); CROCKER, R. L. *Quart. Rev. Biol.,* 27, 144 (1952).
15. *Life.* Personal communication (Oct. 28, 1952).
16. SHELFORD, V. E. *Science,* **100,** 140 (1944).
17. ABERT, J. W. *Journal of Lt. J. W. Abert. . . ,* 28, 32–33.
18. MARCY, R. B. *Report of Exploration and Survey of Route from Fort Smith, Arkansas, to Santa Fe, New Mexico, made in 1849.* House Ex. Doc. 45, 31st Congress, 1st Session, Public Doc. 577. Washington, D. C.: Printer to the House, 41, 60, 64, 72, 74, 80, 82 (1850).
19. ———. 59, 144.
20. ———. *Message of the President of the United States, communicating a copy of the Report and Map of Captain Marcy of his Explorations of the Big Wichita and the Headwaters of the Brazos Rivers, 1854.* Sen. Doc. 60, 34th Congress, 1st Session, Public Doc. 821. Washington, D. C.: A. O. P. Nicholson, Senate Printer. 4, 10, 14, 25–26 (1856).
21. PARKER, W. B. *Notes taken during the Expedition through Unexplored Texas, in the summer and fall of 1854.* Philadelphia: Hayes & Zell, 104, 105, 119, 126, 143, 151, 162 (1856).
22. HARVARD, V. *Am. Naturalist,* **18,** 456 (1884).
23. RICHARDSON, R. N. *The Comanche Barrier to the South Plains. . . .* Glendale, Calif.: Arthur H. Clark Co. (1933).
24. SHALER, N. S. *Aspects of the Earth.* New York: Scribner's, 286–90, 295 (1889); *Nature and Man in America.* New York: Scribner's, 180–88 (1891).
25. HILGARD, E. W. *Report on the Geology and Agriculture of the State of Mississippi.* Jackson: Mississippi State Printer, 349, 361–2 (1860).
26. BEILMANN, A. P., and BRENNER, I. G. *Ann. Missouri Botan. Garden,* **38,** 261, 280 (1951).
27. HANSON, H. C. *Am. Midland Naturalist,* 21, 415 (1939); SAMPSON, A. W. *Range Management: Principles and Practices.* New York: Wiley, chap. 13 (1952).
28. BRAUN-BLANQUET, J. *Plant Sociology.* New York: McGraw-Hill, 278 (1932).
29. BARTLETT, J. R. *Personal narrative of Explorations and Incidents in Texas . . . during the years 1850, '51, '52, and '53.* New York: D. Appleton, 1, 75 (1854); HARVARD, V. *Op. cit.,* 458.
30. MALIN, J. C. *Grassland of North America: Prolegomena to its History,* Lawrence, Kan.: Author, 149–51 (1947).
31. BOYAKIN, W. F. *The Gazette,* St. Joseph, Mo. (Sept. 6, 1854).
32. COOK, C. W. *Ecology,* **23,** 209 (1942); TURNER, G. T., and COSTELLO, D. F. *Ibid.,* 419.
33. LONG, S. H. *Op. cit.,* **16,** 142–3.
34. SUTHERLAND, T. J. *The Weekly Tribune,* Liberty, Mo. (May 7, 1852).
35. SHALER, N. S. *Science,* **1,** 186 (1883); MALIN, J. C. *Essays on Historiography.* Lawrence, Kan.: Author, chap. 2 (1946).
36. MALIN, J. C. *Winter Wheat in the Golden Belt of Kansas.* Lawrence: Univ. Kansas Press, 80–82, 84, 89, 179 (1944).
37. *Sci. Monthly,* **72,** 222 (1951).
38. MALIN, J. C. *The Grassland of North America. . . ,* 278–9.

The Interplay of Social and Internal Factors in the History of Modern Medicine*

RICHARD HARRISON SHRYOCK

Dr. Shryock is William H. Welch Professor of History of Medicine at The Johns Hopkins University. He was born in Philadelphia and received his graduate education in history at the University of Pennsylvania. After some brief training at the U. S. Army Medical School in 1918, he developed a special interest in medical history in combination with the more general field of American history. He was professor of American history at the University of Pennsylvania until 1949, when he assumed his present post as Director of the Institute of History of Medicine at Johns Hopkins. He has served as Acting Director of the American Council of Learned Societies, as President of the American Association of University Professors, and is at present one of the secretaries of the American Philosophical Society.

THE history of any science may be best interpreted in terms of two major aspects: first, that of the internal trends in the science as such; and, second, that of the relations between the discipline and the social environment in which it evolved. This distinction is not absolute but is useful for most purposes, provided that the two categories are broken down into meaningful components. The internal aspect includes the assumptions and objectives of scientists, their approaches (questions asked of nature), methods employed (logical and technical), foci of interest, necessary sequences in discovery, evolving ideas, and so on. Also internal to science is the role of individual genius. Pertaining to the environment, in contrast, are the social, technologic, and philosophic backgrounds. This also involves professional and institutional circumstances, and independent developments in other sciences.[1]

It was usual, during the nineteenth century, to write the history of science largely in terms of the former (internal) aspect. Although some attention was accorded to professional circumstances and to philosophic backgrounds, little heed was given to the intricacies of the total social milieu. This oversight or indifference was carried so far, at least in the case of the medical sciences, as to imply that these developed within a social vacuum. In order to correct this tendency, much attention has been devoted in late years to the "social relations" or the "social history" of science;[2] but this effort has in turn been carried to extremes. One would gather, from some recent works, that the development of the sciences was little more than a function of the general culture of any given time and place.[3]

It is not the present purpose to linger with either of these extreme interpretations. The view here taken is, rather, that the history of science can be understood only in terms of a constant interplay between internal logic and environment.[4] The omission or even the relative neglect of either of these—however helpful for immediate analysis—will distort any final picture. One may hold, no doubt, that the internal story of a science is of primary concern because this is the most distinctive aspect of its history. It is this which makes biology, biology, and not just an indistinguishable thread in a larger pattern. Yet even this view may involve emphases not fully justified by the evidence. There seem to have been times when environmental factors were indeed more potent than internal drives in pointing science in a particular direction.[5]

The analysis of modern medical history which follows is intended to present (to bring out, as it were) the interplay in that particular field between environmental and internal developments. The sequence employed is more a topical than a chronologic one, and the treatment will be as comprehensive as brevity permits.

* This article will also be published in full in *Centaurus,* Vol. 3, No. 1–2.

One may recall, to begin with, the influence exerted on medicine by the European cultural environment as a whole. During the early modern era, pervasive changes—to which science was continuously related as both cause and effect—stimulated and transformed various aspects of "natural philosophy." Medicine[6] then shared with other sciences (as these are now recognized) the benefits which resulted from certain trends in European life—from trade expansion, the rise of the middle class, the revival of Greek science, new intellectual outlooks, and so on. One need not review all aspects of the sociology of knowledge, or the varied implications of new perspectives, in order to recall the advantages which society gradually extended to science during this epoch. Certain it is that, by 1750, scientific activities were carried on within a social and intellectual setting which was far more conducive to discovery than had been that of the Middle Ages. And it need hardly be added that scientists, taking advantage of this, had by that time achieved remarkable results in both basic and applied investigations.

Yet the rate of advance varied widely in different fields. If one accepts the use of quantitative methods as a measure of progress, for example, it is clear that dynamics had emerged on a level by 1650 which chemistry did not attain until after 1750, and which clinical medicine had hardly reached even by 1850. This meant that between these dates different sciences were operating on distinct levels of method and of achievement, despite the fact that all were immersed at any given time in a common cultural environment. Such contrasts could be ascribed (1) to differences in the relationship between the common environment and one science as compared to another, and (2) to differences in the respective natures of the various sciences as such.

In the case of medicine, the first theme noted—the particular relation with the surrounding culture—was of unusual significance. It will be recalled that, despite conditions relatively favorable for science by 1750, there was still much to be desired in this connection. It is true that universities were maintained with the help of church, state, and private endowments, and that professors in certain of these were expected to pursue original work. But there was little or no direct financial aid for research itself. The very establishment of scientific academies was evidence of the inadequacy of the older institutions so far as science was concerned. In England, at least, few outstanding scientists taught in the universities, and the recruiting of future investigators was anything but systematic.

No doubt the rather casual manner in which research was then supported was more effective than it could be today. The prevailing arrangements were fairly well adapted to the needs of the physical sciences, since most of the latter were still in a relatively simple stage of development. Research procedures were not complex, the necessary technical facilities were simple and inexpensive, and little specialized training was required of investigators. Under these circumstances professors could manage reasonably well without "outside aid," and self-trained amateurs could and did do outstanding work.

The relation of medicine to the cultural environment, however, was less satisfactory. Unlike the physical or the general biologic sciences, medicine dealt directly with the most vital interests of mankind—with birth and with death—and out of this situation arose a whole series of peculiar difficulties. Most obvious, first, was the manner in which the use of the human body (as basic subject matter) was hedged about by all sorts of moral taboos. Physical scientists could do as they pleased with test tubes and pendulums, but physicians must not experiment with living men except within very narrow limits. Popular opposition to the dissection of even dead bodies lingered into the nineteenth century, and some abhorrence of autopsies and of animal experimentation persists to this day.

Physicians could, of course, learn something about disease by a passive observation of the sick and by cautious experiments in treatment. But sound generalizations must be based on many cases, and the traditional "solo" form of medical practice did not enable a physician to see more than a small number of patients. What was needed was an institution in which many cases could be studied; that is, the hospital. Hospitals, however, had been founded chiefly for humane rather than for scientific ends, and it was not until the nineteenth century that many of them were so organized as to be available for systematic investigations.

If medical men surmounted the obstacles noted, moreover, they faced still another difficulty inherent in what Roger Bacon had once called the "nobility" of their materials. This was the fact that physicians were under constant pressure to get results quickly. This was not usually the case with physical scientists, because the latter's findings were rarely of vital concern to the public; hence, physicists, even though seeking "useful knowledge," could suspend judgment and proceed with due caution. But death would not wait, and so men desired that physicians reach conclusions without benefit of real verification. The insistent need for curing illness had been

present throughout the centuries, when the state of medical knowledge was such—we can now see—that it could not possibly meet this demand. Yet the attempt had to be made and, what is more, men ever wished to believe that it had at last been successful. Such wishful thinking led some physicians along the way of groping empiricism, while it tempted others to go to the other extreme of unverified speculation or sheer dogmatism in medical thought.

In addition to the difficulties imposed on medicine by humane considerations, there were further obstacles inherent in professional traditions. In the case of physical science, there was no large and ancient guild whose organization or vested interests might retard an effective pursuit of new science. It was far otherwise in medicine. Consider, for example, the situation in the United States during the nineteenth century. In this country, by guild tradition, there were rarely any full-time professors in medical faculties before 1900. Within the universities, professors of physical science could give all their time to teaching and investigation; but medical instructors were selected from among the best known—and therefore the busiest—practitioners. Such men could pursue original work in their spare moments, as could any self-patron; but the truth was that they had few moments to spare when lives were at stake. Cynics may add that professional income was also at stake here, but this was not the whole story. Even a wealthy practitioner who need never worry about "that damned guinea" found it wise to seek a large practice for the sake of prestige.[7]

In other words, the traditions of the medical guild antedated modern research and only slowly adapted themselves to it; hence, few physicians devoted much time to medical science, giving themselves rather to the related art of medical practice. Comte summed up the situation, in 1830, in observing that the prospects for medical science were as dim as they would have been in astronomy if all research in the latter had been left to *its* practitioners—that is, to the sea captains![8]

One may conclude that, although medicine shared in advantages enjoyed by science at large, it also was handicapped by unsatisfactory relationships with its social environment. Since this lack of rapport was more or less peculiar to medicine, much of the lag in this field between 1600 and 1800 may be traced to it. But the slow pace of medical progress may also be ascribed in part to the other major factor; that is, to the internal nature or logic of medical science as such.

In the first place, biologic phenomena were in a sense more complex than were the physical. This was apparently not fully realized in the seventeenth century when, encouraged by the success of dynamics and influenced by the concept of the "animal machine," there was much enthusiasm for an experimental and quantitative study of the bodies of men and animals. But although the first results were encouraging, as in the discovery of the circulation of the blood, the iatrophysical and iatrochemical schools became bogged down by 1700 in a morass of obscure phenomena. It was easier in dynamics than it was in physiology to isolate problems that could be solved in terms of the knowledge and techniques then available. Hence the zeal of 1650 for quantitative concepts and procedures in medicine, however sound and prophetic in principle, was of small avail at the time.[9]

One may pause here to inquire whether this outcome really involved anything more than just another case of adjusting medicine to the surrounding culture—in this case, to the other sciences. For if the human body *was* simply an animal machine, then medicine merely called for the application of physical science to this body. And, in that case, medical science could be expected to advance only in the wake of the physical.

That there was some truth in this will hardly be denied. Biophysics presupposes an adequate physics, biochemistry an adequate chemistry. But this truism was not so applicable to early modern medicine as it is to that of the present time. This is because much of the significant research of the earlier period was done in pathologic anatomy and related clinical problems, and these fields could make little use of either chemistry or physics. Indeed, they usually involved only simple observation, without benefit of the experimental and quantitative methods already taken for granted in the physical disciplines. The significance of this is apt to be overlooked, if it is assumed that experimentation is the only method employed in modern science for observing nature.[10]

In other words, a great part of medical research prior to 1850 was still necessarily in the descriptive stage, even as was that in other biologic sciences. Now botanic and zoologic taxonomy presented real difficulties, but these were relatively simple in comparison with the complexities associated with the taxonomic stage of medical developments. It is when one considers this phase of the story that the problems inherent in medicine as such become more apparent.

Medical thought was in a confused state during the eighteenth century. There was some enthusiasm for Baconian induction and for the empiricism of

the Greek clinical tradition. But the Greek heritage also involved a theoretic, generalized pathology, and learned physicians felt it necessary to defend this "rational" theory against the skepticism of the "mere empirics"—a controversy which had likewise been inherited from classical times.[11]

Actually, much medical lore had had an empirical origin. Pharmacopoeias were the cumulative products of centuries of trial-and-error gropings after remedies. The one clearly specific drug known—cinchona—had had a folk origin. So had the first positive measure in preventive medicine—inoculation against smallpox. Yet the "rationalists" were correct in assuming that progress in medicine—as in any science—must in the long run be based on principles. And what could these be?

Since the purpose of medicine was usually assumed to be the prevention or cure of disease—rather than biologic knowledge for its own sake—it was assumed that medical thought must be based on a theory of the nature of illness. This theory was provided by the prevailing Greek tradition. Illness was usually viewed as a condition in which the body fluids or humors (blood, bile, etc.) were impure or out of balance. Once accepted, this theory led logically to a therapy of bleeding, purging, and other depletion procedures, intended to eliminate impurities or to restore balance in the "general state of the system." Names had long been given to the more obvious "clinical pictures" (smallpox, great pox, etc.), but there was little interest in disease identification. Since it was the state of the body which the physician treated, and as this seemed much the same in various types of illness regardless of any names employed, why bother about exact diagnosis?[12]

This view is not in itself to be lightly dismissed, as was the wont of medical critics during the past century. It involved some shrewd insights or at least inspired guesses; indeed, it may present us with one of the basic alternatives in outlook to which pathology will from time to time return. But the point here is that the ancient, humoral pathology, as still accepted in the 1700s, was both vague and unconfirmed. Professional discussions of its validity, or of that of opposing theories, were reminiscent of scholastic disputes rather than of an effort toward verification in the manner already established in the physical sciences. No knowledge was then available by which the humoral theory could be either proved or disproved. Clinical evidence was cited in its favor; that is, treatments based on the hypothesis were often followed by recoveries. But this was obviously the logic of *post hoc, ergo propter hoc*. Such reasoning would hardly have been accepted *if* medicine had developed in a social vacuum, but here again one must recall the dense atmosphere of human hopes and fears within which physicians actually carried on.

Equally vague and unconfirmable were prevailing ideas about the causes of assumed humoral conditions in the diseased body. Most physicians, as late as 1850, had only confused notions on this score. They spoke, as had the Greeks, of unhygienic habits, of heredity, of poisons in airs and waters, of contagion, and of what now would be termed psychosomatic influences. A few were even convinced that infections could be traced to minute "insects" or animalculae. But these explanations were rarely verified in any exact manner.

Indeed, as long as one general state of the body was assumed to underlie all illness, there was no great interest in causal factors (etiology). For, since illness was viewed as basically of the same nature in all cases, physicians focused their attention on this condition. What had originally caused the bilious, the dropsical, or the feverish state of the system was not so important as was the question of how one dealt with such a condition once it had appeared. After all, it was for this curative function that physicians were desired in society. Here one encounters again a limiting social circumstance. Physicians did write at times on preventive hygiene, but laymen usually felt that this was a matter of folklore or common sense. By tradition—and this is still all too true—physicians were called in only to cure acute illness. And at that stage, etiology seemed to the humoralists to be largely an academic matter.

There was, however, another Greek tradition which taught a doctrine essentially different from that of humoralism. The so-called school of Cnidos had held that there was no one pathologic state common to all illness. Rather, were there many distinct diseases; from which it followed that there were many distinct causal factors—some or all of them specific for particular diseases. Means of prevention, cures, and prognoses were also likely to be of a specific nature.[13] From this viewpoint, the first purpose of physicians was to discover these different diseases; for one could hardly seek the cause and cure of a particular condition until this entity was itself identified.

From the time of Galen until the sixteenth century the humoralist tradition dominated medicine, and the Cnidian view was recessive. Just why a few physicians then revived emphasis on the latter is not clear. Increasing knowledge of non-Galenic, Greek medical literature may have had something to do with it. There were always clinical

phenomena, of course, which suggested differences in types of illness; and it is conceivable that a renewed attention to these differences resulted from a slow but pervasive improvement of observation in general. More definitely, it has been suggested that the discovery of a remedy which was helpful against *only one* type of illness implied that this type ("clinical picture") must be distinct from all others. The only striking instance here was the discovery that cinchona bark was a specific for a certain type of fever (malaria), but this one instance made a deep impression on physicians during the seventeenth century.[14]

Whatever the explanation, the doctrine of specificity was clearly revived and emphasized during the latter part of that century. The most sweeping presentation was that of the English clinician Sydenham, who held that diseases were as real and diverse as were species of plants and animals. Each disease entity had its own cause, its own natural history, and even—if these only could be found—its own cures.[15] The optimism implicit in this outlook has not always been fully appreciated, but it must thereafter have had an increasingly stimulating impact upon medical thought. Instead of continued dependence on the shopworn and static doctrines of humoralism—which had all the answers—those who accepted the concept of specificity could envisage new and promising discoveries all along an advancing front in medicine.

This is not to claim that it was easy at first to identify diseases, to say nothing of finding specific cures for them. It was difficult even to determine what criteria of identification could be employed. Sydenham and his successors defined disease entities largely by symptoms—a procedure which had always been vaguely followed in giving names to different patterns of illness. But when attempts were made to do this in a more systematic and exact manner, the effort bogged down in the multiplicity of symptoms and their combinations. The nosology texts of the later 1700s listed and classified almost two thousand so-called diseases, but these lists involved little more than names for that number of symptom combinations. So confusing did this situation become that some medical leaders of 1800 maintained, or returned to, the view that there was only one pathologic state in all forms of sickness.[16]

Fortunately, the energy imparted by the concept of specificity finally carried medicine through the nosologic maze. The route followed was opened up by the study of human anatomy. This field, cultivated in classical Alexandria and revived during the later Middle Ages, was brought to a flourishing state during the sixteenth and seventeenth centuries. Representing basic research, anatomic studies had been motivated in part by artistic interests or simply by intellectual curiosity. But they eventually came to the rescue of a field long confused by its "practical" focus on the nature and treatment of illness.

As research in normal anatomy progressed, it led by an almost inevitable logic into a discovery of morbid (pathologic) anatomy as well. The view that there was a relationship between disease and structural changes within the body was first expressed in Alexandria, but was thereafter lost to sight during the long dominance of generalized humoralism. Perhaps the possibilities were never entirely forgotten in obvious instances; for example, it was suggested even in medieval autopsies that crude obstructions might occasion sickness. But normal anatomy had first to be revealed before its implications for morbid anatomy could be demonstrated. From the sixteenth century on, great anatomists increasingly called attention to pathologic phenomena encountered in the course of their dissections.[17]

One might now think that such observations would have immediately suggested a relationship with disease patterns. Actually, however, it required more than two centuries to put two and two together in this fashion. (It is a truism that scientific concepts which now appear simple enough were originally most difficult to formulate.) In this case the delay in relating morbid findings to disease may probably be ascribed to two circumstances, the one internal and the other external to general medicine. Within medical science, it was necessary to know something of the function as well as of the form of organs in order to relate structural changes to the abnormal functioning observed in illness![18] Such a knowledge of physiology also evolved logically from earlier studies in anatomy, but this was a gradual process.[19] Not until the later eighteenth century, at best, was physiology adequate for the purpose here noted. The first physician systematically to correlate morbid findings with disease patterns was the Italian Morgagni (1761); but even then this approach was ignored for decades by physicians in general.

One may say that, after Morgagni's time, the knowledge of morbid anatomy on the one hand and of physiology on the other was potentially adequate for revealing a relationship between structure and disease. But physicians, still thinking in terms of a merely humoral, generalized pathology, had to be prodded in some way in order to recognize the possibilities of this new orientation.

The immediate stimulus, in this case, seems to have come from the surgeons. As Temkin has pointed out, surgeons—in the nature of the case—had always dealt with structural conditions. It is true that they were usually concerned with such external situations as fractures or superficial excisions, since a pathology of humors did not call for interference within major body cavities. But all that was needed in order to provide a complete localized, structural pathology was to project surgical attitudes regarding obvious injuries to internal lesions as well.[20]

Now this transfer of surgical attitudes to physicians was delayed by social circumstances; namely, by the separation of these two guilds. Surgeons were long viewed as professionally inferior, and physicians were therefore not inclined to look to them for guidance. Fortunately, professional barriers began to be lowered during the late eighteenth century, and some leading physicians became in consequence familiar with surgical outlooks at that time. There is contemporary testimony by internists that it was this familiarity which finally led them to think in terms of a structural pathology. Such a reörientation was most apparent in the so-called Paris school after 1800, which was largely responsible for establishing the localized, structural pathology as the dominant doctrine over the ensuing century.[21]

It was realized, after this time, that a correlation of ante-mortem symptoms with post-mortem, structural findings could reveal disease patterns which were more clear-cut and distinctive than were those defined by symptoms alone. A great impetus was thereby given, not only to autopsy studies, but also to the improvement of clinical investigations. As has often been said, physicians prior to 1800 *observed* their patients; thereafter, they began to *examine* them. The introduction of improved research methods in clinical medicine subsequently owed much to the cultural environment, as when mathematics made statistics available and when technology produced achromatic microscopes. But such equipment would have been useless in medicine without the revolution in its outlook.[22] Actually, certain potentially useful instruments (thermometers, pulse watches) had long been available, but had not been employed by physicians because they had no interest in exact clinical observations. Not until they began to *look* for correlations between subtle, bedside phenomena and lesions did they desire such instruments. And when they did so desire them, the first ones employed—such as the stethoscope—owed nothing to current physics or technology.

Between 1800 and 1850, progress was made by the new procedures in the identification of many diseases as these are still recognized. In the place of the old humoral theories, or even of vague symptomatic notions like "inflammation of the chest," there appeared such relatively specific concepts as "pneumonia" and "bronchitis." And instead of confused symptomatic notions of various "fevers" (intermittent, continuous, remittent), there emerged relatively clear-cut concepts of typhus, typhoid, malaria, and the like.[23]

In consequence, medical research was ready, by about 1850, to undertake the next step, which Sydenham had long before envisaged; that is, to seek out specific causal factors and cures of these now-identified diseases. Here, again, research was aided by methods or instruments that could not have been employed prior to identification. Thus the microscope could not have been used in a search for specific pathogenic organisms until the diseases to which these were related were clearly recognized. Pasteur could never have found organisms which were causal factors, had he thought in terms of such vague conditions as "biliousness," or "inflammation of the chest."

Up to this point, medical opinion had varied on the almost metaphysical question of the ultimate nature of diseases.[24] Were these, as Sydenham held, objective entities—even as plant or animal species? Were they something which invaded men's bodies from the outside—a notion reminiscent of ancient, demoniac lore? There was considerable resistance to this "ontologic" concept, even among pathologists of the later nineteenth century, who tended to think of disease as simply a form of bodily response to stimuli. Instead of viewing this as involving the bodily "system" as a whole, however, they now thought of response in particular parts—in organs, tissues, or cells, depending on the historic stage of research involved.

When specific, pathogenic microorganisms were discovered after 1875, however, bacteriologists at first viewed these as solely responsible for related infections. And as long as typhoid bacilli were thought of as *the* cause of typhoid fever, it was easy to think of this disease as an objective entity—incarnate in the bacilli, so to speak, and loose in the community. But subsequent developments in immunology and other fields reduced the role of pathogenic organisms to that of merely one factor among many, and the concept of disease as bodily response has once again become dominant. These trends pertained largely to thought within medicine, but they were also conditioned—delayed or accelerated—by the changing environment in

which medical scientists found themselves after 1800. Hence, one may turn again, at this point, to the interaction between internal drives and the social setting of the nineteenth century.

If space permitted, much could be said about the more subtle influences exerted by society on medicine during this later era. Prevailing philosophic outlooks—British empiricism, German idealism, and French positivism—all had some bearing on the course of medical research, despite the fact that this was the period when science consciously sought to cut itself loose from philosophic backgrounds.

More obvious than the influence of philosophy was that exerted by changing social and economic conditions. Certain of these changes were favorable. Medicine shared with other sciences the various advantages ushered in by the industrial revolution—increasing wealth, urban growth, improvements in printing, transportation, and technology in general. These were the developments which made possible the organization of science as we now know it; with reference, for example, to libraries, societies, and journals. Various social factors, meantime, converged to produce the modern universities, in which research and training for research were happily combined. Medicine profited from all this, as it did likewise from the improved facilities and equipment which technology placed at its disposal.

Two nineteenth-century social trends were of peculiar benefit to medicine—the growth of cities, and the cultivation of humanitarianism. These combined to produce and improve the large, general hospitals. Although not originally intended for the purpose, this type of institution turned out to be a *sine qua non* for the very clinical and pathologic research which was so essential at the time. We would have had the hospitals without modern medicine, but no modern medicine without the hospitals.

On the other hand, some aspects of social evolution were less favorable to medicine. Consider, for example, the social reactions of 1825–75 to the expanding hospital program of clinical and pathologic research. As already implied, it is difficult to see how basic progress in medicine could ever have been achieved except along this line. Yet the program was centered for more than fifty years on the identification of diseases, rather than on their prevention or cure. Research men were so preoccupied with this "pure" research, which had no immediate prospect of utility, that they apparently lost interest in therapy. Moreover, their more critical temper led them to discard the older remedies, at a time when there was as yet little with which to replace them. A spirit of "medical nihilism" pervaded the best centers.

Insofar as this nihilism became known to the public, it was not calculated to inspire confidence in medicine. There is indeed evidence that this period, which we can now see was of the greatest promise in medicine, was one in which the public had the least confidence in regular practice. One of the indications of this was the proliferation of rival medical sects, such as homeopathy, "the botanic system" (Thomsonianism), and so on. These sects—like later osteopathy and chiropractic—preserved the old theses of a generalized pathologic state or of some single scheme of treatment, long after these oversimple formulas had been repudiated in regular medicine. Then they promised the cures which candid regular physicians no longer believed possible, and so appealed to a public which was often in dire need of such assurances.[25]

More serious than popular doubts, moreover, was the danger that neither philanthropists nor governments were likely to support a field having little apparent utility. Modern science had early been hailed as a means to acquiring "useful" knowledge; and if this was indeed its major purpose, why assist it when it failed to serve that end?

The answer to this query was not a simple one. Actually, little direct private or governmental aid was extended to pure research in medicine or biology prior to 1860. But in relatively aristocratic countries not yet dominated by industrialism, science continued to benefit from the deference long accorded to learning as such. During the Enlightment of the eighteenth century, such deference had increased and had focused, so to speak, on the universities. This can be best observed in the prestige enjoyed by German or other Continental professors, and in the support extended to them by their respective governments. It even became a matter of pride with some men that their research had no relation to "mere utility."

In relatively democratic countries, where businessmen became increasingly influential, the middle classes continued to encourage the pursuit of useful knowledge. Conversely, they had little desire to support basic research. This attitude could be observed to some degree in England, but found its extreme expression in the United States; where a "practical" people saw little reason for supporting the "idle curiosity" of pathologists or other pure scientists. It is hardly an exaggeration to say that, for most Americans, the word science

connoted simply applied science or technology.[26]

It is suggestive that in those countries where science was highly regarded for its own sake, there were notable achievements during the nineteenth century. This can be well observed in medicine, where the pre-eminence of French and German pathologists was widely recognized. Even more striking was the manner in which the French and Germans dominated medical bacteriology, as this field emerged after 1875. At the other extreme, again, was the experience of the United States, where—despite individual exceptions—the record in basic medical research was a minor one. Had the matter been left to this country, it is unlikely that modern medicine as we understand it would ever have evolved. At best, the process would have taken a much longer time. In this connection, as has been pointed out, American culture was related to that of Europe in much the same manner as Roman civilization had once been to that of Greece.[27]

The fact that technology made rapid progress in the United States might conceivably have led to parallel advances in basic science here. Certainly there are circumstances in which one can observe technology stimulating science, as well as vice versa.[28] This was true even in medicine; for example, when a knowledge of brewing and fermentation played a role in Pasteur's investigations. But whatever was true in special cases, the American story certainly indicated that technology *may* be successfully cultivated on a large scale without much benefit to basic research. It even suggests that an extreme devotion to applied science may interfere with, or at least divert attention from, basic science. Americans were quite successful at times in advancing medical practice, as in the introduction of anesthesia and in other contributions to surgery. Yet these achievements did little, prior to 1900, to stimulate investigations in medicine as a whole.

American attitudes were not likely to change until medicine—or any other science—demonstrated its immediate utility. Fortunately, this was just what happened in medicine during the later nineteenth century. This is an oft-told tale: of how pathology led into bacteriology, and the latter into rational sanitation and immunology. So far as infectious diseases were concerned, prevention took on new possibilities and public hygiene was transformed. Even curative medicine came to life, with the aid of chemistry and pharmacology, in chemotherapy. Meantime, pathology and physiology opened up the fields of endocrinology and metabolism, so that prevention, controls, or cures became available for the deficiency as well as for the infectious diseases.

Last but not least, surgery was transformed; not by anesthesia or aseptic procedures, as is often assumed, but primarily by the advent of the new pathology. As long as the old humoralism prevailed, surgery could never be more than a skilled trade auxiliary to medicine—for, after all, one could not operate on the humors. But once disease was conceived in reference to localized structures like the appendix, then the offending parts could be removed by operations. And when this was realized, surgery shifted from the periphery to the center of medical practice. The discovery of anesthesia and of aseptic procedures ensued, in an effort to make the most of newly discovered opportunities.[29] Here, again, it was the revolution in medical *thought* which initiated progress: better instruments and procedures followed in the wake of this reorientation.

Hardly had all these promising developments appeared before American policies toward medical research and practice began to change. Complex as the entire story is, one need only recall here the manner in which—after about 1890—licensing restrictions were tightened, sectarianism declined, and people began to seek admission to hospitals rather than to stay out of them. Medical research, meantime, played a Cinderella role. Completely neglected by both philanthropy and government before that time, it became the chief beneficiary of the great foundations during ensuing decades. And it is obvious enough that government also moved into this picture in recent decades.[30] This latter development, in turn, raised new problems regarding the relation of government to medicine as well as to the other sciences.

In all this, it was not American attitudes that had changed: it was rather medicine that had been transformed. Americans still demanded the useful, but they were now convinced that even the "purest" medical research might become helpful at any time. And, in consequence, society came to the aid of medical studies in a manner that promises much for the future.

Implicit in public confidence was a realization that the interplay between medicine and society had been profoundly modified during recent generations. In all that has been recalled here about the relations between medicine and its environment, the influences noted so far have been those exerted by society upon medicine. Nothing has been said, up to this point, about the possibility

of a reverse influence—that of medicine upon society.

The reason is obvious. There was little tangible evidence prior to about 1875 that medicine had had much impact upon society. In certain respects, physicians had undoubtedly been of service to their communities. They had inspired confidence, encouraged personal hygiene, and guided empirical sanitation. They had learned how to prevent smallpox, to control malaria, and to ameliorate other conditions. Surgeons, meantime, had been most helpful in emergencies. But over against these services must be set the damage done by generations of well-intentioned but too heroic practitioners. No one will ever know how many good people lost their lives on the altars of bloodletting and purging. On the whole, medicine had probably done more good than ill prior to 1875, but little measurable influence upon society emerges from such a balancing of values.

It is clear, however, that the whole situation has changed over the past seventy-five years. Medicine has become one of the major factors in unprecedented declines in death rates and in associated increases in life expectancy.[31] These trends, of course, have had their effect upon the composition of the population, notably in the rising percentage of elderly people and in the resulting increases in certain chronic and degenerative diseases. And this, in turn, not only presents further problems in medicine, but also imposes new obligations upon society—from hospital construction to social security.

In a different connection, medicine has confronted society with still another basic difficulty—that of medical costs. For the very advances that made medical care more desirable also made it more expensive; and out of this situation have arisen the public pressures for health insurance.

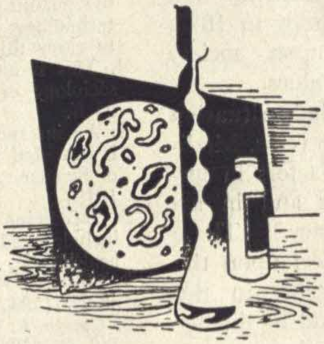
It is apparent, then, that the relations between medicine and society now involve—for the first time in history—a fully developed, two-way process. Each influences the other in many and significant ways. The drives within medical science itself, meantime, continue to operate as one of the most dynamic factors in the whole situation. In some respects, medical thought at the present time seems to be returning to some of the very concepts—such as that of a generalized pathology common to various diseases—which were repudiated during the 1800s.[32] But that is another story. The interplay between medicine and society continues as in times past, but the components and the results of the process are changing rapidly.

Footnotes and References

1. The distinction between internal and external factors is applicable to the sociology as well as to the history of science. See Barber, B. *Science and the Social Order,* 28 ff. (1952).
2. For the European literature see, for example, Merton, R. The Sociology of Knowledge. *Isis,* **27** (Nov. 1937), 493 ff. For more recent writings, there are a number of bibliographies in English; e.g., Leikind, M. C. *The Social Impact of Science.* Washington, D. C.: GPO (1945); and those in *Impact,* a Unesco periodical. Most useful is Barber and Merton. Brief Bibliography for the Sociology of Science, *Proc. Am. Acad. Arts Sci.,* **80,** 140 ff. (May 1952).
3. It is, of course, justifiable to present the history of scientific developments primarily in relation to the surrounding culture, as in Merle Curti's able work *The Growth of American Thought* (1943). But there is always some danger of misinterpretation unless the limitations of this frame of reference are clearly stated; see e.g., the author's review of this study in *Am. Historical Rev.,* **49,** 732 ff. (July 1944). In emphasizing the pursuit of "useful" knowledge and the impact of technology on basic science, Marxian writings tend to carry this "cultural" interpretation to extremes.
4. This is also the view of those concerned with the sociology of science; see especially Barber, *op. cit.,* 25 ff.
5. An instance of this, in the case of thermodynamics, is provided in Lilley, S. Social Aspects of the History of Science. *Arch. intern. hist. sci.,* (6), 376 ff. (Jan. 1949).
6. "Medicine" is used throughout in a broad sense, to include medical practice and institutions, the public health, etc., as well as the medical sciences.
7. Shryock, R. H. *Development of Modern Medicine,* 38 ff. (1947).
8. Comte, A. *Cours de Philosophie Positive,* III (1908); 148 ff. (1830).
9. Shryock, R. H. *Op. cit.,* 17 ff.
10. Barber, for example, although fully aware of the importance of simple observation in biologic science, declares that "The dynamic of modern science inheres in the proper interweaving of conceptualization and experiment" (*Science and the Social Order,* 19). Instances will be cited of the "dynamic" of interweaving conceptualization with simple observation as well.
11. Galen's comments are given in Brock, A. J. *Greek Medicine,* 130 ff. (1929).
12. Knud Faber has noted the expression of this view which is found in the concluding statements of the Hippocratic text on *Prognostics;* see his Thomas Sydenham, der englische Hippocrates u. die Krankheitsbegriffe der Renaissance. *Münch. med Wochschr.,* (1), 29 (1932).
13. Garrison, F. H. *History of Medicine,* 99 (1929).
14. Faber, K. *Op. cit.,* 29.
15. Rush, B., Ed. *Works of Thomas Sydenham . . . ,* xxiv ff. (1809).
16. For example, Benjamin Rush, of Philadelphia, announced about 1800, as a new theory, that there was only one pathologic state. He was then hailed by many as having brought order out of the chaos of the nosologies. A long poem to this effect is preserved in the Rush *MSS.* in the Library Company of that city.
17. Long, E. R. *History of Pathology,* 77 ff. (1928).
18. Sigerist, H. F. *Man and Medicine,* 120 (1932).
19. Castiglioni, A. *The Renaissance of Medicine in Italy,* 49 ff. (1934).
20. Temkin, O. The Role of Surgery in the Rise of Modern Medical Thought. *Bull. Hist. Med.,* **25,** 248 ff. (May–June 1951).
21. Other social influences, of course, were brought to bear on the Paris school; such as those exerted by

empirical philosophers and by mathematicians. There is a large literature on this story. See, for example, Fosseyeaux, M. *Paris Médicale en 1830*, 97 ff. (1930); and Rosen, G. The Philosophy of Ideology and the Emergence of Modern Medicine in France. *Bull. Hist. Med.*, **20**, 328 ff. (July 1946).

22. Or, in more formal language, without a shift in the "conceptual scheme" (Barber, *op. cit.*, 19). But note that it was simple observation rather than experimentation which was here being directed by a new "conceptualization."
23. The evolution of these concepts can be traced by comparing old "bills of mortality" with the later lists of "causes of death."
24. Pagel, W. The Speculative Basis of Modern Pathology. . . . *Bull. Hist. Medicine*, **18**, 1 ff. (June 1945).
25. Shryock, R. H. Quackery and Sectarianism in American Medicine. *The Scalpel* (May 1949).
26. ———. American Indifference to Basic Science During the Nineteenth Century. *Arch. intern. hist. sci.*, (5), 50 ff. (Oct. 1948).
27. DeTocqueville, in analyzing this situation in 1835, thought that Americans would have developed basic science *if* they had lacked European aid. But he cited Chinese civilization as having failed to do this under those circumstances. *Democracy in America*, II, 518 (1904).
28. Lilley. *op. cit.*, 376 ff.
29. Sigerist, H. E. Surgery at the Time of the Introduction of Antisepsis. *J. Missouri State Med. Assoc.*, 169 ff. (May 1935).
30. Shryock, R. H. *American Medical Research: Past and Present*. 88 ff. (1947).
31. Winslow, C.-E. A. Communicable Diseases, Control of *Encyclopedia of the Social Sciences*, Vol. IV, 77 (1931).
32. Grey, G. W. Cortisone and ACTH. *Sci. American*, **182**, 35 (Mar. 1950).



SMO ON THE AIR

STATION	SPONSOR	TIME
	Monday	
WOI-FM, Ames, Iowa	Iowa State College of Agriculture and Mechanic Arts (Articles of Interest)	7:45 P.M.
	Tuesday	
WEVD, New York City	Wendell W. Rázim (Science for the People)	9:00 P.M.
	Wednesday	
CKPC, Brantford, Ont.	The Telephone City Broadcast Limited (Modern Science)	9:45 P.M.

The Editor of THE SCIENTIFIC MONTHLY will be glad to cooperate with university or other educational stations interested in securing scientific material suitable for broadcasting.

Mathematics and the Educational Octopus*

STEWART SCOTT CAIRNS

Dr. Cairns has been professor of mathematics and head of the department at the University of Illinois since 1948. For the two previous years he was chairman of the department of mathematics at Syracuse University. He has published a number of articles in mathematical research journals and has served as consultant to a number of governmental agencies. He has taken an active interest in the problem of scientifically trained manpower and in the past year has become involved in efforts to strengthen scientific training in the public schools.

THE perennial debate over educational policies and principles has in the past few years gone into an acute phase. Speakers and writers have attacked one another's views, and occasionally one another, with varying degrees of vigor. The dispute has sometimes waxed acrimonious and has frequently drifted into irrelevancies. In view of the acknowledged basic importance of the subject and in view of its intimate connection with future scientific developments, the writer believes that the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE should devote substantial attention to it. Accordingly, he proposed last April that at the December 1952 meetings there be an open and well-organized discussion of current educational practices and their relation to the sciences. Correspondence with a number of interested persons revealed, however, (1) that the atmosphere was unfavorable† to a reasonably objective debating of the issues, and (2) that there was insufficient time during the few intervening months to achieve the necessary conditions for a symposium appropriate to so important and complex a question.

In the hope that a suitable session for December 1953 may prove possible, the writer recommends that Section Q (Education) of the AAAS join with a few other sections in sponsoring a conference devoted to the subject of scientific training in the elementary and secondary schools. He further recommends:

1. That such conference be concerned with questions of content, teaching methods, administration, and policy control in the public schools, especially at the secondary level.

2. That it be planned by a special committee, representing various AAAS sections, on present tendencies in secondary education and teacher training.

3. That the planning committee endeavor to formulate some general recommendations and resolutions to be offered to the AAAS as a whole for consideration.

4. That such recommendations, resolutions, and other appropriate material be published in advance of the meeting or, if that prove not feasible, be distributed at the time thereof.

The present article is restricted primarily to mathematical training and is thus largely confined to one special phase of the battle against anti-intellectualism in the schools. The plight of the humanities is in a way even more serious than that of the sciences, for the latter currently have the mixed blessing of an artificial stimulus from defense efforts in favor of adequate scientific training of students. It is largely in the fields of literature, art, music, and the other humanities, however, that one would expect those with intellectual tastes, whether scientifically minded or not, to find occupation for the leisure time afforded by technological improvements and by those medical advances that continue to increase the average age of the population. Accordingly, let the scientists not neglect their humanist colleagues in the common struggle for the improvement of the public school program.

* This article is based on an address delivered December 30, 1952, before the Mathematical Association of America at Washington University, St. Louis, during the AAAS Annual Meeting.

† At the time of his address in St. Louis, the writer stated his belief that his proposal had been referred to Section Q of the AAAS and had there met with an unfavorable reception. This was a regrettable error, based on a misinterpretation of certain items of correspondence. The proposal had not even come to the attention of the officers of Section Q.

The Aims of Education and the Needs of Society

Prudence or modesty would lead most of us at first sight to shun these large and controversial questions and perhaps instead turn to our own scientific research problems, where we at least have the satisfaction of knowing with certainty when a solution has been reached; but the interests of mankind, the welfare of our children, and the prospects for further scientific advance should lead us to overcome our reluctance, especially when we consider that in dodging these questions we are, in effect, answering them. Decisions will of necessity be made somehow, by someone, concerning the educational program of our schools. We have only ourselves to blame if, by default, we permit vital policies to be determined on the basis of improper criteria in the hands of persons who, however well-meaning, have a biased and only partial comprehension of the issues at stake.

It is too much to expect that clear-cut, well-defined educational aims will be widely adopted for the guidance of our public school administrators. The guiding principles now most prominently advocated constitute a jumble of policies, subsumed under the general title of "life adjustment" education. Backed by powerful forces, in a manner to be described, "life adjustment" is gaining greater and greater prevalence in our country, so that it behooves us all to examine it carefully and decide on the extent to which it should be permitted to pervade the educational system.

Authoritative information on this movement is to be found in pamphlets issued by the Office of Education of the Federal Security Agency. One such pamphlet[1] says, "Life adjustment education is designed to equip all American youth to live democratically with satisfaction to themselves and profit to society as home members, workers and citizens." Although, in the form here stated, life adjustment is a great and noble aim for the public schools, grave trouble inevitably arises in its interpretation and implementation. Clearly, for the scholar and for the scientist, living with satisfaction to oneself and with profit to society will carry very different implications from those conveyed to a man devoid of intellectual interests. Along with other critics of modern educational practices, the writer recognizes that the narrower, better-defined academic aims of two generations ago are inadequate for our existing society, and that vocational training, social adjustment, and training for citizenship are necessary to modern mass education. These aspects of schooling should, however, merely supplement, never replace, grammar, literature, history, mathematics, and other essential elements of our intellectual heritage. The latter are subjects which, if neglected in the public schools, will be acquired by only a favored few, and the lives of the great bulk of our youth will be "adjusted" to a very low cultural level indeed. Is *this* our concept of democratic education? Rather, let the public schools assume the leadership in an intellectual revival. In a release dated September 20, 1950, the American Institute of Public Opinion reported that only 21 per cent of United States adults read books. Across our northern border, in Canada, the figure is 40 per cent. It is 43 per cent for Norway, and 55 per cent for England. In the light of these data, do we really wish merely to "adjust" our children to their environment, or shall we, using "education" in its true sense, lead them into something better?

The next large issue to be touched upon is that of the educational needs of society and of individuals. How vast is the need of the nation and the world for the truly educated? How widespread and how deep are the thwarted, perhaps unconscious, intellectual thirsts that now go unsatisfied for the sake of wholesale educational experimentation? One might suppose that mathematics, generally looked upon askance by the layman, would be a rare object indeed for such intellectual desires. Yet, counteracting this impression, consider the success of such books as Hogben's *Mathematics for the Million*. Kasner, with his *Mathematics and the Imagination*, a recent selection of the Book-Find Club, has fascinated many a layman, and other such examples could readily be cited. Let us hasten to agree with one objection to the foregoing argument, namely, that the entertaining style of the successful popularizer bears little resemblance to traditional classroom presentations. Many a future mathematician has been bored to extinction, and many a potential amateur repelled by the dullness of arithmetical and algebraic drill in the schools. Mathematicians have, however, played a leading role in efforts over the past fifty years to encourage the teaching of mathematics so as to appeal to the imagination and awaken the interest of students. Further success in these efforts, in which professional educators have cooperated, will depend on training public school teachers so that they will acquire a genuine appreciation for mathematics and so that their understanding will be developed well beyond the level at which they are expected to teach. Under present conditions many of them dislike and fear the subject and unconsciously transmit their feelings to their students.

Let us now pass on to one particular, measurable

phase of our national needs—the critical shortage of scientifically trained manpower. Throughout this article, the presentation of numerical data is minimized, because the retailing of figures is somewhat boring, because an unbiased canvassing of the available statistics would be a mammoth undertaking, and because the writer does not wish to assume the vulnerable position of those who select, from the wealth of statistical data, an array of persuasive facts in support of their personal views. Attention is called, however, to *Higher Education* (**9**, [7], [Dec. 1952]), issued by the Federal Security Agency, Office of Education. In an article on "Engineering Education in the United States," Henry H. Armsby concludes, on the basis of figures from the Bureau of Labor Statistics, that "there seems no immediate prospect of meeting the current acute shortage of engineers." The practical exigencies of this situation and the closely related shortage of scientists in all fields may prove to be the entering—or the re-entering—wedge for restoring some of the essentials of mathematics to their proper place in the schools. For, as Mr. Armsby points out, "engineering is based on mathematics and the physical sciences, and an interest in and aptitude for these fields is an important indicator of success in the profession. However, many studies have indicated that the pattern of high school subjects bears little, if any, relationship to success in an engineering college."

Even in peaceful times, the demands of modern society for engineers and scientists would be very heavy. As matters now stand, we need hardly be reminded that our national security hangs in the balance, and that, in the desperate struggle to preserve it, mathematics and science are crucial weapons. Our educational shortcomings are thus revealed not only in the light of a national disgrace but also as a dangerous source of weakness on the international front.

Attention is called to a statement, "Policy on Training and Utilization of Scientific and Engineering Manpower," by John R. Steelman, acting director of the Office of Defense Mobilization.[2] After a clear discussion of the problem, a set of recommendations is made (*a*) to employers of scientists and engineers, (*b*) to professional engineers, and (*c*) to educational institutions. The engineers are urged, among other things,

> To cooperate with educational institutions in studying the adequacy of existing curricula in the sciences and in engineering, in developing better teaching methods and in achieving maximum utilization of teaching facilities in scientific and engineering fields.

The educational institutions are urged

> To make special provision for students who possess the necessary aptitude for engineering work but who lack the courses in science and mathematics prerequisite for admission to the engineering college either because of the inadequacy of high school offerings or because their interest in an engineering career was developed too late in the high school program.
>
> To strengthen high school curricula in order that more high school graduates will be eligible for entrance into engineering colleges and to establish closer working arrangements between colleges and universities and high schools to the end that both high school students and the faculty will became more aware of the opportunities in the engineering field.

Responsibilities are then assigned to a number of agencies, including the Federal Security Agency, which is directed to

> Encourage and assist engineering colleges to make special provision for students who possess the necessary aptitude for engineering work but who lack the courses in science and mathematics prerequisite to admission either because of inadequacy of high school offerings or because their interest in an engineering career was developed too late in the high school program.
>
> Assist secondary schools in developing more adequate curricula and better teaching methods in order to provide students possessing the requisite aptitudes and interests with the fundamental education necessary for college work in science and engineering.

Growth of the Octopus

There is danger that a description of the octopus will seem to imply a wholesale attack on all colleges of education, on their entire faculties, and on all who participate in running the public schools. This is far from the intention. To avoid so false an impression, let us first cite J. G. Fowlkes, of the School of Education, University of Wisconsin. At a farewell dinner for Willard Spalding, dean of the College of Education of the University of Illinois, Dean Fowlkes is quoted[3] as having upheld arts and sciences training as the basis of teacher education, saying "It is nothing short of tragic that so many professors of education took their undergraduate work with a major in education . . . [took] a master's in education, and a doctorate in education. . . . My concern is great for the lack of sound general education of the elementary teachers in this country." As the guests departed, one educator is said to have remarked to another that they might as well have had Bestor[4] or Fuller[5] as speaker.

Another important gratifying event has been the valuable cooperation of the College of Education at Illinois with mathematicians and engineers in certain forward-looking projects in secondary school mathematics. These will be mentioned later in somewhat greater detail.

Least of all would the writer wish to appear to

be casting aspersions on public school teachers. They are our natural allies in the struggle for higher standards, and many of them have, not too vociferously, expressed a desire that they be permitted to teach something more substantial and to apply higher standards of performance than at present.

But we are neglecting the octopus. This creature grew as a byproduct of our national adherence to the principle that public education should be equally available to everyone. During the past few decades, we have seen widespread and well-motivated efforts to adapt curricula to the varied needs of all students. At the same time, school enrollments have experienced a tremendous growth, far out of proportion to the general increase in population. Since 1870, our secondary population has grown from 80,000 to 6,000,000—that is, by a factor of 75; and our colleges from 60,000 to 2,300,000, a factor of 38. Meanwhile the population of the country has merely tripled. This tremendous relative and absolute school expansion has created an almost overwhelming educational problem. Little wonder that the efforts to deal with it have fallen far short of perfection!

Economic and social developments, rather than a thirst for learning, have largely accounted for the phenomenon of a rapidly increasing fraction of the population wishing to continue school attendance to a more and more advanced age. The predominantly academic interest which dominated earlier secondary schools has offered little appeal to most of the inflowing students, and, accordingly, new curricula have been developed in an effort to satisfy the requirements of those with all degrees of ability and all types of interest. In such a period of change, it is natural that one extreme group of educators should advocate a close adherence to traditional academic studies, and that an opposing faction should desire to scrap the entire curriculum of the past and start from scratch. Neither extreme has quite won a complete victory, but we have been led much too far toward the goal of the latter.

Colleges of education have played a leading part in the recent development of our public schools. Given the enormous task of coping with unprecedented hordes of students, it was natural that such colleges should enjoy a vast expansion in both size and influence. Their products have flooded the educational system as teachers, principals, superintendents, and occupants of powerful posts in state offices of education. The certification requirements and conditions for advancement that have been established tend to strengthen the control of schools of education over the training of teachers, obliging the latter to include in their programs an amount of course work in educational methodology and allied subjects that seems excessive in quantity and deficient in substance. After these teachers embark, with a minimum background in subject matter, on their careers, they are encouraged, for the sake of professional progress, to devote some of their vacation time to additional study, with a continued slighting of subject matter in favor of educational workshops and more courses in methodology, psychology, and so on. There is little to marvel at here, for our elementary and secondary school system has so developed that true scholarly attainments are not particularly conducive to advancement. The premium is on the comparatively nonintellectual phases of school life, which, although important, make relatively light mental demands on either teacher or student.

The educationists have achieved power not merely through local and statewide influence, but also through nationally centralized activities. The U. S. Office of Education, in the Federal Security Agency, has proved an influential mouthpiece for the advocates of the life adjustment program. One pamphlet issued by that office[6] refers in the following terms to a report, *Cardinal Principles of Secondary Education,* by the national Commission on the Reorganization of Secondary Education.

> General, vague, and highly theoretical objectives such as formal discipline were rejected for such down-to-earth criteria as social utility, student interest, and provision for individual differences.
>
> Besides emphasizing a social and practical basis for the high-school curriculum, ***this report seemed to receive wider acceptance because it was published and distributed by the U. S. Office of Education.*** [Italics supplied.] It was the first national report to reach the rank and file of the classroom teachers. Moreover, the seven basic objectives of health, command of fundamental processes, worthy home membership, vocational education, civic competence, worthy use of leisure, and the development of ethical character, while general in nature, were sufficiently real to be well understood by the teacher whose task it was to work toward these objectives in the classroom. They were soundly based on such practical considerations as the needs of society, the nature and capacities of youth, and the findings of educational research.

For a more emphatic illustration of organizing power, we quote from *Life Adjustment Education in the American Culture,*‡ which reports a Work Conference on Life Adjustment Education, held in Washington, D. C., in October 1951:

‡ Federal Security Agency, Office of Education. This pamphlet is referred to hereafter as "L. A." The quotation is from p. 66.

It was agreed that: The State has a leadership function, not a policing function; the State colleges can serve as centers of stimulation; the success of a State-wide program of life adjustment education depends upon a well rounded task force at the State level which includes representation from the high school principals, the State Department of Education, teacher education institutions, and interested laymen.

It was also agreed that the States should exchange information regarding their action programs. The secretary of the national Commission on Life Adjustment Education was requested to prepare a list of no more than two representatives from each State in which systematic efforts are being carried on to improve secondary educational programs.

The Life Adjustment Movement

Let us turn next to a brief examination of this life adjustment movement, behind which such power is concentrated. The program goes also under the names of "core curriculum," "general studies," and so on. The term core curriculum, which is only vaguely defined, refers to a conglomeration of endeavors cutting across subject-matter lines and related to "real life problems." It is partly described as follows in *"Core Curriculum Development. Problems and Practices.*§

Some schools went still further in eliminating subject-matter lines. They believed that the school should do something about the problems which are persistent in the lives of adolescents as members of a democratic society. These problems are common to all youth and draw upon many different subjects for their solution. Working on them, it was thought, would develop the personal and social competence of youth. Also, the democratic processes of pupil-teacher planning and cooperative group work which would be used, should develop the habit of reflective thinking and skill in solving problems. Classes were organized on a block-of-time basis with one teacher in charge throughout the two or more periods (p. 5).

Perhaps the concept of this curriculum can best be clarified by quoting one of the several examples of activities to be found in C. C. (p. 20).

Construct and interpret a personal health record form. On the form, place such items as: height, weight, illnesses, accidents, habits of good hygiene (bathing, brushing teeth, washing hair, etc.), problems of health and appearance and plans for solving the problems. Use the form to keep a health record.

Subject fields: Agriculture, Arts, Business Education, Distributive Education, Health and Physical Education, Home Economics, Language Arts, Mathematics, Music, Science, Social Studies.

The other examples are of similar character. If you are feeling particularly strong, you might care to read them, and, as you do so, ponder on the grocery-store concept of mathematics that is being propagated. The authority most quoted in this pamphlet, judging from the index, is Harold Alberty, of Ohio State University. In a speech in Illinois last spring, he included the following topics under core curriculum: orientation to schools; home and family living; community life; contemporary cultures; contemporary America among nations; political, social, and economic ideologies; personal value systems; world religions; communications; resource development; human relations; physical and mental health; group and individual planning; science and technology; vocational organization; hobbies; education; war and peace; public opinion. What should be taught under these ambitious headings is nowhere spelled out. Coupled with this impressive mass of topics is the principle of pupil-planning, in accordance with which the class, by discussion and vote, decides, or helps decide, what to "study" next.

Although a strong aroma of the ridiculous pervades the above programs, let us not scoff at their objectives. Efforts to promote the moral and social development of students are of primary importance. Many behavior problems among capable students could, however, be cured by providing them with studies to challenge their abilities in an atmosphere where there is an immediate incentive to scholastic success. The effort to retain everyone in school beyond the age of legal compulsion may even lead to increases in juvenile delinquency, instead of to suitable life adjustment, as a result of the unrestrained and infectious activities of students artificially induced to remain in the public schools. From the viewpoint of the present article, there is more cause for concern in the power of core curriculum to displace essential studies, as illustrated in the following quotation from C. C. (p. 5):

Such classes, of course, had to replace subjects that were already in the curriculum, subjects that were required of all. English and social studies were the subjects usually chosen, with science, mathematics, art, music, or health added in some instances. These were basic or core subjects. The new program then became known as the "core curriculum" or sometimes as "general education." It had a distinct type of organization in the total school program; its content and methods differed widely from traditional courses.

Note that core curriculum replaces the very heart of the traditional academic studies. To quote from a later passage (p. 19), "Occasionally English or social studies is combined with mathematics or science; infrequently science and mathematics are the basis of core. . . . Core knows no subject matter lines and the problem to be solved may draw from any areas of the curriculum that can contribute to its solution."

§ Federal Security Agency, Office of Education (1952). This pamphlet will be referred to as "C. C."

To emphasize further the expulsive force of core curriculum, we remark that Professor Alberty has advocated beginning "core" in the junior high school with two thirds of the school day devoted to it, and continuing it through high school *and the first two years of college,* gradually reducing the allotted time to half a day. Meanwhile, incidental to such activities as the "health record" project quoted above, the students are expected casually to pick up the displaced studies. Even where core curriculum has not predominated, the diversification of the high school program has been accompanied by a growing freedom for the students to choose what they will study and what they will omit. Naturally, as the more exacting courses have become optional, they have lost favor and patronage. Advisers of students tend to warn them against the more difficult studies, lest they run the danger of frustration and failure. Warning lights hang particularly over mathematics and Latin, when the latter remains on the program at all. Because of the bad psychological effects of poor grades on *some* students, the standards for *all* students have been lowered. This is done partly in the name of enhancing the "holding power" of the school, which means making it so attractive in one way or another that all students will attend even after legal compulsion ceases. In general, drill is tedious, so it is dropped. Minimum demands are made on the memory. Why we should shun the development of memory as a tool is a mystery difficult to fathom! Students are supposed to learn to reason without knowing facts, but why is it that they should not be obliged to master such useful tidbits as the multiplication tables? In English classes, the parts of speech are either not taught at all or are supposed to be picked up in some incidental manner—perhaps after ignorance of them has hampered students in foreign language courses. Mathematical instruction has been generally depressed to the level of the mediocre, and essential topics have been progressively omitted. Powerful figures in the educational field repeatedly emphasize the acquisition by students of necessary but trivial arithmetical competencies to the exclusion of those fundamentals of algebra, geometry, and trigonometry required for the most rudimentary efforts in the field of science. The life adjusters use the lopping-off technique of Procrustes. The weakening of mathematics, along with the general suppression of other classical academic subjects, has been partly justified by the false argument that the studies in question are good only for the college-bound minority. Hence, by a gross misinterpretation of democracy, these subjects are crowded out, to the detriment of everyone. Indeed, the damage to those who do not go to college is particularly severe, since the others will at least get later opportunities to fill the more serious gaps, but the non-college-bound majority are denied even a proper high school education.

College entrance requirements are among the obstacles that have hampered the experimentation of the life adjusters. Some colleges have high admission standards; others, especially state institutions, have almost nominal requirements. The "adjusted" students can go to the latter. If a student in a high school dominated by advocates of "core" wishes to prepare for college entrance examinations, he swims against the stream. On all sides are schoolmates who, in order to become college students, are required to do little but live long enough, and in front of the class is a teacher who has not much time or incentive to help him out. It is rare indeed to find a teenager, however intelligent, who will work assiduously toward a distant goal while his companions are contentedly loafing their way through high school and into college, with no penalty involved save perhaps a mediocre progress report in which they are at once complimented and slapped on the wrist by being told they are working below their abilities.

Even the weakest of college entrance requirements are targets for the life adjusters. We quote from a pamphlet[7] issued through the Office of the State Superintendent of Public Instruction in Illinois.

> If a considerable block of courses must be retained in the high school to provide for the preparation of students who hope to go to college, the opportunity to re-examine the total high school curriculum and to replan the program in terms of the needs of all high school youth is thereby curtailed.

We also quote again from L. A.:

> It was agreed that college entrance requirements are often an obstacle to curriculum change, especially in small schools. Described were the Michigan agreement (which eliminates any required pattern of high school subjects for college entrance) and the proposals made in Illinois for determining college entrance (on the basis of a few basic tests and the recommendations of high school teachers). Some States reported that colleges accepted all high school graduates, but doubt was expressed that all graduates were actually equipped for college entrance (p. 69).

This last sentence belongs in the department of understatement.

It is interesting also to glance briefly at the so-called Eight-Year Study, used by many educationists as a justification for experimentation. Although it purported to be a controlled statistical study, Vol. I of the report, *The Story of the Eight-*

Year Study,[8] states: "Everyone invited to serve on the Commission was known to be concerned with the revision of the work of the secondary school and eager to find some way to remove the obstacle of rigid college prescription." Little wonder that this report as a whole is a gold mine for those seeking samples of statistical fallacies!

The encroachments of life adjustment on academic work do not stop with the high schools, or even with the college admission standards. The tentacles of the ever-growing octopus now stretch dangerously toward the colleges and the graduate schools, with state-supported institutions as the most vulnerable. This tendency is revealed not only in Alberty's desire for 50 per cent of the first two college years but also in the following passage (L. A., pp. 63–64):

> In order to prepare those who teach teachers to teach, we recommend to graduate schools
>
> 1. That a candidate for a doctor's degree be counseled as to his vocational adjustment, the kind of job he expects to get, and the kind of people with whom he will work.
> 2. That a college and university teaching minor for a doctoral degree include such subjects as mental hygiene, guidance, and supervised teaching.
> 3. That faculty members of graduate schools be equipped to handle general education courses.
> 4. That specialists in one field in the graduate school work with and understand the work of specialists in other fields.
> 5. That the graduate school provide real life situations for prospective college teachers under supervision.
> 6. That graduate students be encouraged to help plan courses and to evaluate their own work and the work of the instructor.

In this connection, let it be noted that administrative posts in institutions of higher learning are frequently filled of late by those with education degrees.

The Struggle with the Octopus

Numerous and vigorous, but sporadic and largely uncoordinated, have been the protests against the substandard educational fare offered to our children. In many communities, parents have made more or less futile efforts to bring about a strengthening of the curricula, so that their children would learn at least some of the fundamentals of grammar, mathematics, and other subjects once considered an obvious part of the program. Scientists and scholars have raised their voices eloquently and convincingly but, thus far, to little avail. These are, we trust, only the preliminary skirmishes before the battle proper.

It would be a task carrying us far beyond the bounds of the present effort to undertake a balanced discussion or even a listing of the principal events in the educational controversy. The writer's participation in the battle dates back less than a year, when a sequence of incidents, outlined below, on the University of Illinois campus led him actively into the fray.

Although no attempt is here made to give a general treatment of the modern educational controversy, the highlights at the University of Illinois, which is by way of becoming a spearhead for the attack, will be mentioned. The opening guns were fired by Harry Fuller, professor of botany, in a Phi Beta Kappa address to the Illinois chapter on May 10, 1950. The published version, "The Emperor's New Clothes, or *Prius Dementat*,"[5] appeared in THE SCIENTIFIC MONTHLY for January 1951. Some months later, Dean Spalding, of the College of Education, responded in a vituperative address‖ on the Illinois campus, during which Fuller was characterized as "The Bewildered Botanist." The next address in the spasmodic Illinois series was "Aimlessness in Education, or *Ex Nihilo, Nihil Fit*,"[4] by Arthur E. Bestor, of the History Department. The discussion at the end of Bestor's speech turned partly on arithmetical training, apropos of the desire on the part of certain educational extremists to remove the three R's as an essential part of every normal student's program. The writer was particularly irked by the apparent argument that, because a certain percentage of school children are incapable of mastering the multiplication tables, the more capable students, as well as the incapable fraction, should not be required to master them. Having, by this argument and the cumulative effects of other incidents, been drawn actively into the struggle, the writer joined a few of his colleagues from the College of Liberal Arts and Sciences on March 6, 1952, in making presentations at an open meeting of the School Problems Commission of the State of Illinois, which is charged by the General Assembly "to consider and study all germane factors in an effort to determine the improvements necessary to raise the educational standards of the public schools to a desirable level." Although the pros and cons of these various events have been extensively debated in THE SCIENTIFIC MONTHLY and in Illinois newspapers, there is no evidence of significant progress, and it appears that much more ambitious measures will be necessary.

The protests of the various critics of current educational theories are countered in a variety of ways. Local organizations of parents are stigmatized as pressure groups and as either enemies of

‖ See Note 15 at the end of SCIENTIFIC MONTHLY article by Bestor.[4]

the schools or as dupes of such enemies. They are crackpots, malcontents, and reactionaries with a horse-and-buggy philosophy, or else they are motivated solely by a desire to lower taxes, regardless of the effects on the schools. They are also called un-American and enemies of our democracy—which, of course, is being valiantly sustained by the octopus (L. A., pp. 9–12). The possible existence of sinister forces desiring the destruction of our schools lends plausibility to this kind of answer. Some of these charges were advanced, with varying degrees of emphasis and directness, by no less a group than the National Commission for the Defense of Democracy through Education, reporting to its parent group, the National Education Association, at the July 1950 meeting of the latter in St. Louis. As for the scholars and scientists, they are met with the voice of authority. They are, perhaps, reputable specialists in their respective fields of research, but only the specialist in education is authorized to deliver himself on educational subjects. The mathematician, although he may have spent his life as a teacher, is rarely to be consulted on questions in the teaching of mathematics, with regard either to subject matter or to manner of presentation. It is the educationist, however deficient in knowledge of mathematics, who must be the primary arbiter of both questions.

In reply to criticisms of educational practices, one encounters not only the arguments already mentioned, but also a variety of statements, of differing degrees of relevance, tending to distract from the main issues. Thus we are sometimes told, quite truthfully, that, as far back as man can remember, college faculties have been critical of the deficiencies of students coming to them from the secondary schools, the apparent implication being that, because this is a current, though acute, phase of an immemorial dispute, it can be dismissed with a shrug.

Efforts to suppress criticism have been reported from a number of reliable sources, though there has been no noteworthy attempt to discourage us at Illinois from speaking our minds. We have, however, been criticized by colleagues for carrying the argument outside the confines of the university, on the ground that the dispute might prove detrimental to the institution and might undermine confidence in public education. It is indeed generally politic to settle intramural arguments with a minimum of publicity. This is far from being an intramural problem, however; it is a grave national issue, in which industry, the government, and society as a whole all have a vital concern. We could not, if we would, settle it within the confines of any university or all universities, for the tentacles of the octopus are all-embracing.

Symptoms of Progress

In this section, I shall restrict myself to a few symptoms with which I have, in somewhat haphazard fashion, come into contact. The topic is too large to permit an adequate survey on the present occasion. At several successive annual meetings of the Illinois Section of the Mathematical Association, there were items on the program concerned with secondary school mathematics. Before the 1952 meetings, a special committee prepared for a panel discussion and worked up a set of resolutions, one of which (Resolution II) commented on the present poor mathematical preparation of entering college students and put the section on record as approving a general strengthening of mathematics offerings at all precollege levels. Resolution III provided for "a committee of five members of the Illinois Section of the Mathematical Association of America and/or the Illinois Council of Teachers of Mathematics distributed as follows: two classroom teachers of college mathematics, two classroom teachers of secondary school mathematics, and one classroom teacher of elementary school mathematics, which committee shall be known as the Committee for the Strengthening of the Teaching of Mathematics. . . ." The committee was charged with a specified set of appropriate duties. It hopes to begin its work by embarking on a realistic survey of the mathematics actually being taught in the schools of the state.

The College of Engineering of the University of Illinois, handicapped by the poor mathematical preparation of its incoming freshman, took steps leading to the appointment of a committee to study and report on the situation. This committee was composed of engineers, mathematicians, and representatives of the College of Education. As one result of its labors, a pamphlet was produced, *Mathematical Needs of Prospective Students in the University of Illinois College of Engineering*, which is expected to have a strong effect in increasing the number of entering freshmen who can immediately take analytic geometry and can take college physics concurrently with calculus the next semester. The booklet has been widely distributed and has awakened considerable interest in high schools and in other colleges. Incidentally, it has aroused the hopes of some secondary school teachers that they may get something more substantial to teach than at present. Following the production of the pamphlet, there has been a preliminary development of tests to measure the

relevant competencies attained by secondary school students. An experimental high school program implementing the recommendations of the pamphlet has been prepared by a committee of similar composition to the one mentioned above and has been put into at least partial operation at University High School. These things are mentioned to illustrate the possibilities of collaborative effort among mathematicians, educators, engineers, and others.

Earlier in this paper, an article in *Higher Education* was quoted, to the effect that the pattern of high school subjects bears little relationship to success in an engineering college. At the same time, the critical shortage of engineers has led the Engineering Manpower Commission, working on a wide front and operating through high school advisers, to stimulate enrollment in engineering colleges. Given an irrelevant high school program, it is extremely difficult to guess which students should be encouraged toward engineering, with the result that we can expect an unnecessarily large number of misfits. We can only hope that these misfits will discover the mistake before too much of their own lives and the efforts of their teachers has been wasted. With military service just around the corner, a boy can hardly afford to embark on a college curriculum for which he is ill-suited. Aptitude tests are, to be sure, of some assistance in determining which students should be advised to consider scientific careers. A high score on such a test should, however, be regarded with mixed feelings, for it is partly an indication of what *might* have been. It suggests what progress in learning the student could have made, had he only been confronted with appropriate opportunities in an atmosphere conducive to intellectual effort. On the other hand, when it comes to discovering potential scientific talent, a sound mathematical course may well be more effective, and is certainly of far higher value to all concerned than any aptitude tests that the ingenuity of man can devise. The colleges are now generally devoting a year or more of mathematical course work to studies that belong in the high schools. In the case of engineering, where the essential training is largely of a sequential nature, this has the effect of seriously delaying the entire program of studies. It is to be hoped that the Engineering Manpower Commission will be influential in stimulating the strengthening of mathematical offerings in the secondary schools. A counterpart of the Engineering Manpower Commission, the recently organized Scientific Manpower Commission, will presumably bend its efforts in the same direction insofar as training and education in mathematics are concerned.

Proposals for Further Progress

Although the items outlined here are mere symptoms of progress, they may furnish a basis for possible activities in other regions, with such improvements as experience may dictate and ingenuity devise. In general, the writer believes in the feasibility and efficacy of collaborative efforts among college, secondary and elementary teachers, and such professional educators as prove cooperative. The sections of the Mathematical Association of America, the state councils of teachers of mathematics, and various other organizations can render valuable service. In particular, all possible resistance should be offered against the drive to weaken college entrance requirements. We should, on the contrary, strive to build up these requirements toward that point where the colleges will no longer devote a substantial part of their effort to the teaching of high school subjects. As another measure, certain to prove unpopular with those who want freedom to manipulate the entire secondary school program, the writer advocates legal guarantees that every student be provided with specified educational opportunities in the public schools, the specifications to be adopted after careful consultation with all interested groups. The consultation and the groups consulted must not, however, be dominated by professional educators, although the opinions of the latter should be considered along with the rest. Once standards are adopted, they must be implemented, perhaps by testing procedures like the New York Regents Examinations, perhaps with the aid of tests devised by Educational Testing Service, or possibly in some other way.

As a longer-range project, the writer recommends a large-scale study, perhaps sponsored by one of the foundations or by a national agency, directed toward analyzing the general problems of

1. The mathematical competencies considered desirable by colleges of liberal arts, engineering colleges, industrial organizations, and government agencies on the part of high school graduates, both those bound for college and those terminating their formal education with high school.¶
2. The extent to which the schools of the country are meeting these needs.
3. Measures required to bring about any changes necessary to meet such needs.
4. Determination of the actual and desirable lines of demarcation between "high school" and "college" mathematics.

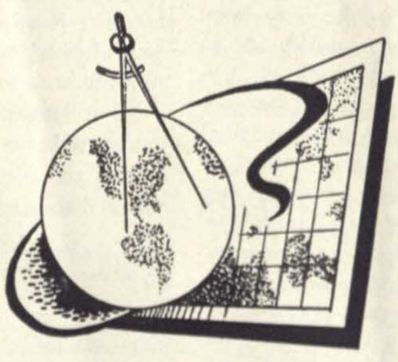
¶ Such work has been done in the past by highly competent groups. It should be carefully reviewed and brought up to date.

Such a project would involve the services of a group of mathematicians and other scientists, statisticians of the highest caliber, professional educators sympathetic to the investigation, representatives of the general public, and representatives of the various groups listed under (1) above. Part of the work would consist in analyzing the validity of statistical arguments employed to justify some of the present practices, and part would consist in planning and conducting surveys to furnish an adequate basis for conclusions and recommendations.

Finally, the writer wishes to express general support for a set of resolutions proposed by his colleague Arthur Bestor to the American Historical Association, and designed to promote a cooperative undertaking by the learned societies in all suitable disciplines. His proposal begins with a preamble expressing alarm at anti-intellectualist tendencies and calling for "cooperation among all the learned societies of the country, acting through an independent, interdisciplinary Commission of their own creation," in activities "designed to uphold and strengthen sound, systematic, disciplined intellectual training in the public schools." A first resolution, in several parts, affirms a belief in certain educational principles. A second resolution "calls upon its [the AHA's] sister learned societies in every field to join with it in creating a Permanent Scientific and Scholarly Commission on Secondary Education," adequately financed and charged with a list of appropriate specified functions. On December 29, 1952, the American Historical Association referred the proposal to a committee for further study.

References

1. *Vitalizing Secondary Education,* subtitled *Education for Life Adjustment.* Washington, D. C.: GPO.
2. The Federal Register, Title 32-A-National Defense, App., Chap. 1, Office of Defense Mobilization, Defense Manpower Policy 8.
3. Champaign-Urbana *Courier* (Nov. 18, 1952).
4. Bestor, A. J. *Sci. Monthly,* **75,** 109 (1952).
5. Fuller, H. J. *Ibid.,* 72, 32 (1951).
6. *Vitalizing Secondary Education,* 15–16.
7. Illinois Secondary School Curriculum Program. Bull. No. 9, *New College Admission Requirements Recommended,* 5.
8. Aiken, W. M. *The Story of the Eight-Year Study,* Vol. I. New York: Harper, 2 (1942).



SCIENCE ON THE MARCH

RECESSION OF EASTON AND DEMING GLACIERS

Mount Baker, a glacier-crowned volcanic peak, is located in northwestern Washington, about 15 miles south of the international boundary (Fig. 1). Washington's third highest summit, it rises to an elevation of 10,775 feet above sea level and carries at least nine large glaciers, all but one beginning at and radiating outward from the summit. The Easton and Deming glaciers were selected for study primarily because of their close proximity to the Koma Kulshan ranger station where the writer was employed by the United States Forest Service during the 1952 fire season. These glaciers are accessible by 12 miles of graveled road from the town of Concrete to the ranger station, by 10–12 miles of good trail from the station to Easton Glacier, and by 2 miles of cross-country travel from there to Deming Glacier.

Considerable information is available on Easton Glacier, and photographs taken in 1917, 1925, 1931, 1947, and 1952 strikingly show the recession undergone by the glacier. Less information is available on Deming Glacier, but photographs taken in 1931, 1947, and 1952, and information taken from earlier topographic maps provide a fairly accurate record of its recession.

Glacial Features and Nature of the Recession. Easton and Deming glaciers coalesce and form a continuous expanse of ice covering the entire south and southwest sides of the mountain, with but a single rock outcrop near the top marring the glistening whiteness. Only at the lower margin, where ice tongues extend short distances into the valleys, with bare bedrock ridges lying between, is the continuous ice mass separated into 2 distinct glaciers.

An excellent vantage point from which all of Deming Glacier can be seen is the open ridge overlooking the glacier on the south (Fig. 2). By looking in a northeasterly direction to the source of the two glaciers at the crest of Mount Baker, one can see the clean cover of perpetual snow on the upper part; where crevassed, the annual snow increment is exposed in layered bands of varying thickness. Snow and ice accumulating on the summit plateau overflow and descend steeply over the Roman Wall to the upper slopes of the glaciers.

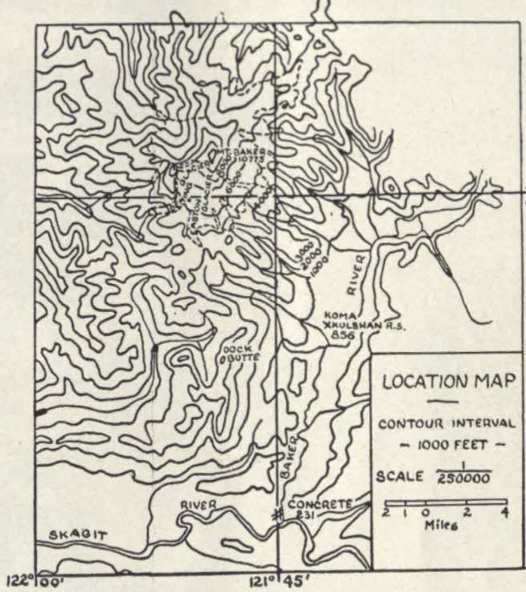


Fig. 1. Topographic map of Mount Baker, surveyed in 1907–08, edition of 1915, U. S. Geological Survey.

Below the wall the crevasses appear, deep wide lateral flexures with ice or snow cliffs on the upper sides.

The ice tongue protruding from the Deming ice front flows into the deep cirque lying at the south base of the Black Buttes. The walls of the cirque are very steep and high, with considerable overhang, and snow avalanching down them provides much additional nourishment to the glacier. In a riverlike manner the glacier swings in a full quarter-circle, flowing in a southerly direction and leaving the cirque by way of a narrow defile descending into a deep canyon. Through the defile the glacier is broken by a spectacular icefall. Here the surface snow is displaced and broken into large blocks, with the blue glacial ice generously exposed. At the base of the icefall where the ice is smoother, Deming Glacier turns and flows in a southwesterly direction. The terminal zone of the glacier is beyond this last turn.

Within the terminal zone Deming Glacier is rapidly wasting, as evidenced by much debris hiding the ice and by the chaotic appearance of the

FIG. 2. Deming Glacier icefall on July 9, 1942. A large meltwater stream flows on bedrock along left side, with the Black Buttes on the skyline and Mount Baker at extreme upper right. Note high ice cliffs just right of center.

surface (Fig. 3). Intersecting ridges and hummocks interspersed with closed depressions, all having considerable relief, form the surface of the glacier. That the surface debris is resting on the ice, closely conforming to surface irregularities, is demonstrated by the fact that where the debris has slumped glacial ice is revealed. Beyond the ice front the debris has been "let down," so to speak, by the melting of the ice beneath it, until now it conforms with the lesser relief of the underlying valley floor. A very milky meltwater stream issues from the base of the ice tongue and enters a narrow V-shaped stream channel about a quarter of a

FIG. 3. Terminal zone of Deming Glacier showing meltwater stream issuing from the base of the ice. The 1907 position of the ice was at or near the place where the stream leaves the barren glacial trough and turns to the left beyond the line of trees.

mile beyond the present terminus. As nearly as can be determined from a distance, the stream has cut its channel about 100 feet into the bouldery valley fill without exposing bedrock.

Except at the terminal zone of both glaciers, meltwater streams are notably absent on the surface. Evidently surface streams are unable to flow even short distances without interruption by deep lateral crevasses. At the base of the Black Buttes, where ice cliffs are numerous, a few large streams emerge, cascading abruptly over rock precipices to disappear again beneath the ice. A large stream emerges from the ice base and flows between the southern wall of the buttes and the ice mass, disappearing beneath the ice after flowing a short distance on bedrock.

The 1907 Mount Baker quadrangle map places the terminus of Deming Glacier fully four fifths mile beyond the present-day terminus, near the place where the meltwater stream leaves the barren glacial trough and enters the old-growth forest. The 1907 terminus was at an elevation of approximately 3800 feet, as compared with a present elevation of about 4600 feet. Figure 4 shows the relative position of the ice about 1931, when the terminus stood 2500–3000 feet behind the 1907 front. The Hamilton and Mount Baker quadrangles (Fig. 5), edition of 1952, show the present-day terminus about four fifths mile behind the 1907 position at the edge of the forest.

Three ice tongues or lobes protrude from the lower margin of Easton Glacier. From their general relation to the surrounding topography, the lower ends of these ice tongues are many hundreds of feet short of the advanced position shown on the 1907 map. Although the two eastern lobes were not visited, they were viewed from a distance, and their earlier positions can be estimated with rea-

FIG. 4. Aerial photograph, about 1931, showing Mount Baker from the southwest. Deming Glacier occupies trough at center and flows toward observer. The 1907 position of the ice was at extreme lower end of glacial trough at edge of forest. Note relatively clean surface of ice at terminus and compare it with debris-laden ice in Figure 3. (Photo by U. S. Forest Service.)

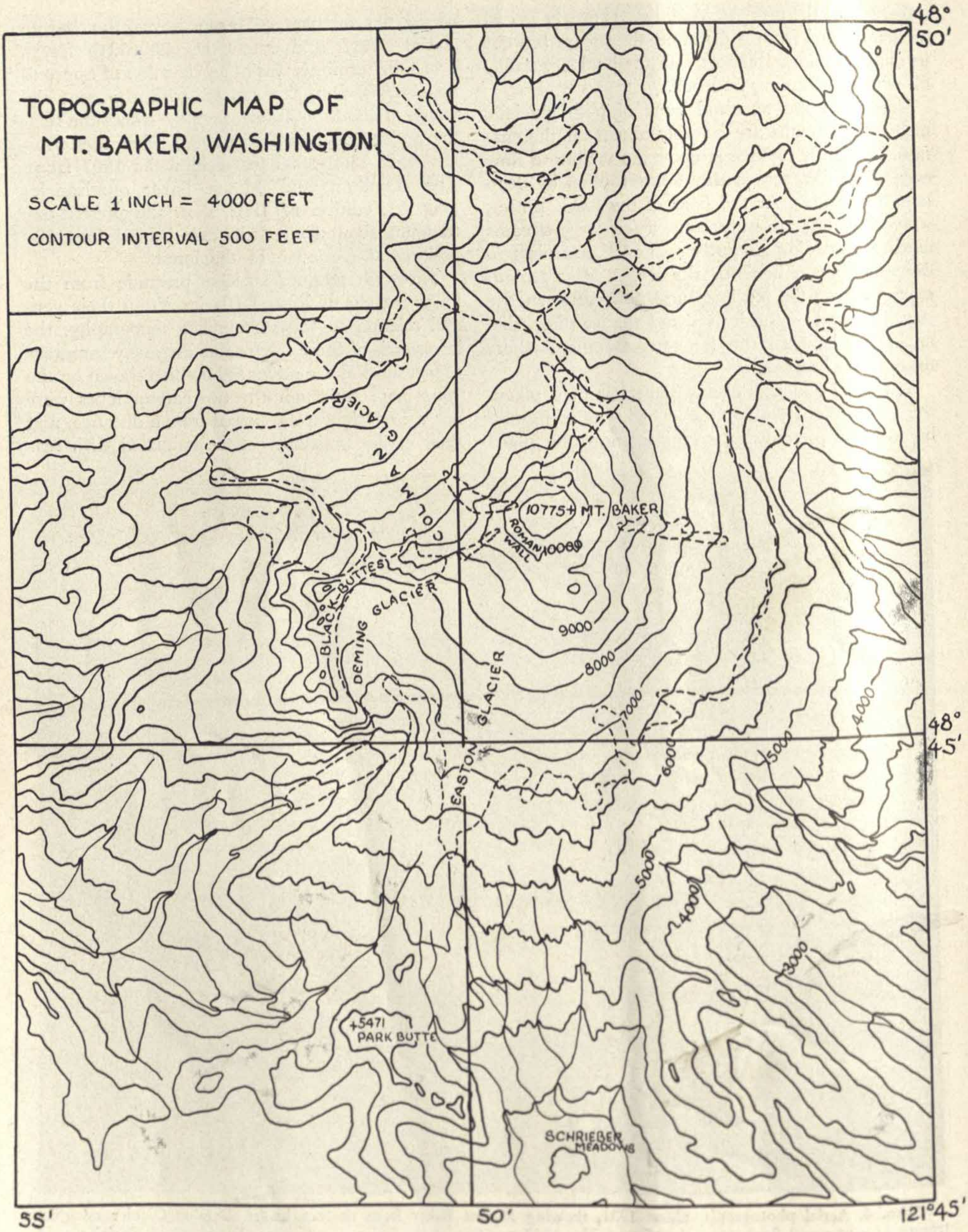


FIG. 5. Topographic map of Mount Baker, redrawn from Hamilton and Mount Baker quadrangles, edition of 1952, U. S. Geological Survey.

FIG. 6. Huge morainal embankments were deposited by Easton Glacier when the ice occupied the barren glacial trough. Mount Baker in the background.

FIG. 7. Easton Glacier and Mount Baker, showing advanced position of the ice in 1917. Judging from the size of the glacier at right margin of picture, with the ice at least 250 feet thick at this point, as determined by estimate and by later pictures, the terminus is considerably less than 1000 feet from the 1907 position. When compared with later pictures, both the retreat and downward wasting of the ice are very obvious. Orange-painted arrow marking 1934 terminus is on rock outcrop just to left of meltwater stream shown at center. The right margin of the glacier almost exactly coincides with the location of the marker. (Photo by George Ely.)

sonable accuracy from the map and from early photographs. The detail and the nature of the recession of Easton Glacier are best shown by the longest, or western, ice tongue, since this tongue has left a fairly complete record of its recession.

As indicated by the great quantities of rock debris forming huge lateral embankments (Fig. 6), the recession of the west tongue must have been balanced by its forward movement for many years. The lateral ridges extend unbroken from behind the present ice front a distance of about 1½ miles, curving gradually inward near the apex and descending to a low end moraine, which is breached at the center by the meltwater stream. The end moraine marks the approximate position of the ice tongue in 1907. The crests of the lateral moraines are sharp and narrow, overhanging and slumping very readily on the inner, or trough, side. Huge boulders continually break loose from the trough sides of the laterals and fall to the floor of the glacial trough. Plumes of whitish dust rising from the trough mark the paths of many small rock slides started by these tumbling boulders. On

FIG. 8. Mount Baker from Park Butte, about 1925, showing Deming Glacier and the Black Buttes at left, and Easton Glacier on right. (Photo by George Ely.)

Fig. 9. Aerial photograph, about 1931, showing Mount Baker from the south. Deming Glacier and Black Buttes are at left, and Easton Glacier is at center, occupying upper end of barren glacial trough. Dotted lines show relative positions of the ice in 1907, 1917, and 1925. "X" marks the place where the Mountaineers painted the orange arrow showing the 1934 position of the ice. Note debris-covered terminus of Easton Glacier. (Photo by U. S. Forest Service.)

the ridge side, however, the bouldery debris is firmly held in place by soil, alpine plants, and trees.

The 1907 map of the Mount Baker quadrangle shows the snout of the west lobe of Easton Glacier about 1 mile northwest of Schrieber Meadows at an elevation of about 4100 feet. The writer visited several old-timers in the vicinity who vouched for the fact that the glacier occupied this advanced position near the turn of the present century. The Hamilton quadrangle map, made by multiplex methods from 1947 aerial photographs, shows the terminus at an elevation of about 5450 feet, fully 1⅓ miles behind the 1907 position at the lower end of the glacial trough.

The earliest photograph shows Easton Glacier in 1917 (Fig. 7). At that time the snout ended only a short distance—perhaps not more than 400–600 feet—behind the 1907 position. Since then, however, the entire ice tongue extending across the base of the view has completely disappeared, and the amount of thinning at the head of the trough has been appreciable.

The condition of the ice in 1925 after consider-

Fig. 10. Showing thin, debris-covered terminus of Easton Glacier on August 11, 1952.

able recession had occurred is shown in Figure 8. The terminus stood at an elevation of about 4800 feet and had receded almost half the distance from the 1907 position to the present-day front at the upper end of the glacial trough. Figure 9, an aerial photograph taken about 1931, shows Easton Glacier after it had receded more than ¾ mile from the 1907 front.

Panoramic photographs, taken in September 1935 by the United States Forest Service from Dock Butte fire lookout house, place the terminus at an elevation of about 5200 feet, or the same elevation as the lookout house. This elevation can be established with considerable accuracy, since a line connecting points of elevation equal to the height of Dock Butte almost exactly coincides with the snout of the glacier at the lowest point.

The present-day terminus is several hundred feet behind the 1947 position. Near the terminus the ice bows outward and descends steeply for 300 feet, then gradually bows inward, thinning and narrowing into a very short tongue extending only a few tens of feet before it is completely dissipated by melting (Fig. 10). This short ice tongue has thinned to such an extent that it has almost completely disappeared. Large chunks of the tongue remain, but they are detached from the main mass and are melting very rapidly. Much sand, silt, and bouldery debris cover the isolated masses of ice, and small meltwater streams are prevalent on the ice surface. On the east side a narrow section of ice still is attached to the main glacier, but it is very thin. Thinning is hastening the rate of recession, as evidenced by the condition near the terminus where large sections stagnate and simply waste away. The Hamilton quadrangle map and the photographs from which it was made show a much longer ice tongue extending beyond the Easton ice front. Assuming the farthest or lowest isolated ice chunk to be at or near the 1947 front, the writer on August 11, 1952, measured by pacing and found the distance from the present ice front to the lowest chunk to be 550–600 feet. Recession from 1947 to the present is about 600 feet.

Records kept by the Mountaineers, of Seattle, are shown in Table 1.

TABLE 1*

MEASUREMENTS OF RECESSION OF EASTON GLACIER

	Recession (Feet)
1934–35	190
1935–36	170
1936–37	116
1937–40	429
6-year average	905

* From 1934 until 1937 the Mountaineers measured the annual recession of Easton Glacier on Mount Baker. However, during the years 1938 and 1939 no measurements were taken. Therefore, the figures acquired this year must be made into a 3-year average.
The terminus of the Easton Glacier forms two thin tongues of ice, of which the eastern one is the longer. Because of the small amount of ice in these tongues recession may be quite rapid during the next few years. The Easton Glacier was measured October 13, 1940, by Fred Becky and Paul and Ed Kennedy.

On August 20, 1952, the writer and Ben Carey, of Mount Vernon, Washington, measured the total recession from the orange-painted marker made by the Mountaineers at the 1934 terminus and found it to be 2210 feet. These figures show that in the past eighteen years the ice has been retreating at an average rate of about 123 feet a year.

WILLIAM A. LONG

Filer High School
Filer, Idaho

ANIMISTIC THINKING AMONG COLLEGE AND UNIVERSITY STUDENTS

THERE is evidence that a large percentage of children, up to at least twelve years of age, believe that many objects, classified by biologists as inanimate, are alive. The data concerning animism in children come from groups as diverse as the Swiss,[1] white American groups,[2] and American Indian groups.[3, 4] These investigations indicate also that children who believe objects to be living frequently attribute to them various forms of consciousness.[3, 5] In other words, the child attributes biological and psychological characteristics to entities which the scientist classifies as inorganic matter.

Piaget,[1] who is largely responsible for the modern interest in animistic thinking in the child, proposed that in civilized man it disappears at about age twelve or shortly thereafter. Adults, he claimed, believe that only plants and animals are living. But it does not appear that Piaget has questioned adults on this point, nor, until recently, have other investigators directed their inquiries to adult subjects.

In 1949 Mallinger and I[6] reported the results of applying the techniques that have been used with children to 36 persons aged seventy and over. We found that 75 per cent of these subjects attributed life to some objects other than plants and animals. Although it was suggested that the high percentage of older persons exhibiting animism might be due to mental deterioration accompanying age, it was kept in mind that no data on adults in early maturity were available.

Recently I have undertaken the questioning of various groups of younger adults. I propose eventually to interrogate persons of average intelligence and education, but pilot studies were conducted with college and university students. The results of the investigations have proved so surprising, not only to me but also to several groups of psychologists to whom they have been reported, that it seems advisable to present a brief preliminary report.

In studies of children, the method was that of an individual interview.[7] With college and university students individual questioning did not seem necessary, and a group method was used.

The instructions were as follows:

> I am going to ask you questions about some common objects. You may think some of the questions very easy; some may be rather hard. I am asking these questions of many kinds of people. Both the easy and the hard questions are asked of you because I want to know how persons of all sorts answer the very same questions. Please answer each question seriously, though some questions may seem very simple.

The subjects were then shown, or reference was made to, several objects. With regard to each object, the subjects were to write down whether it was living or not living, and were required to state the reason for the answer. The answer regarding one object was written before the next object was presented.

The first group to be questioned was a class in child psychology which had not studied or discussed animism. The students were enrolled in a large private university in New York City, and were primarily graduate students of education. Of the 67 students, 54 were graduate students, 15 of whom already held a master's degree; 45 had had teaching experience. The objects about which they were questioned were an unlighted match held before them, the same match lighted, an electric clock on the wall of the classroom, the sun, the wind, a five-cent piece, a pearl, gasoline, and the ocean.

Forty-five per cent of these subjects stated that one or more of these objects were living. The objects most often called living were the lighted match, the sun, and the ocean. In the case of each of these, approximately one third of the group gave animistic answers.

The questions were next asked of a class of 71 students in introductory psychology at one of the city colleges of New York. Most of the students were sophomores. The objects employed were identical with those presented to the graduate students of education, except that the nickel and the pearl were dropped and the term "clouds" was added. Thirty-seven per cent of these students gave one or more animistic answers.

Subsequently, a similar set of questions was given to a mixed group of 34 graduate and undergraduate students enrolled in child psychology in the summer session of a Southwestern university. The terms employed with this class were the sun, clouds, sea, lightning, wind, stars, and earth. The percentage attributing life to one or more of these items was 48.

In the light of these findings, it seemed worth while to make a serious attempt to find college students who were not animistic. The best available candidates seemed to be 68 sophomores in the city college previously referred to who were just completing the third semester of an integrated science course. Most of the work of the current semester had been biological, with a strong emphasis upon the properties of protoplasm and the distinctive characteristics of living things. They had had no other science course in college. The list of terms presented to the introductory psychology students was repeated with this group. On the basis of the results obtained in this instance, my belief in the possible efficacy of instruction in biology was somewhat restored. Only ½ per cent of these students attributed life to any of the objects about which they were questioned.

On several occasions, the presentation of the results described here has caused my auditors to be rather incredulous. They insist that, other than the science students, the subjects must not really have meant what they said. It was suggested that they were being poetic, or philosophical, or whimsical. The effective answer has been to read the detailed animistic responses and to ask whether the answers can correctly be considered poetry, philosophy, whimsey, or science. Space permits the printing of only a few examples, illustrating the naïve character of most replies. The following are typical:

The lighted match: "Living, because it has flames which indicate life." "Living because it is

burning brightly, giving forth something." "Dying—I saw it being burned."

The sun: "Living because it gives forth energy. Gives us power, warmth, light, and energy. Makes things—living things—thrive and exist." "Living because it gives off heat." "Yes! Living! Without breath, but living, scientifically living, changing."

The ocean: "Living because it is constantly maintaining life. Movement is characteristic of it, and life is brought forth by it." "Living. It has moods and is temperamental just like many human beings." "Living—it moves and makes noise and is powerful and changing. Sometimes calm, sometimes stormy. We cannot control it." "Living, continually in motion, changing, etc."

These answers are equivalent to those earlier recorded for children, except that some of the adult answers reflect a larger vocabulary. The reasons offered by the adults for their answers belong to the classes which Piaget has called Stage II and Stage III types; that is, an object is said to live because it moves or is active in some way (Stage II) or because it is self-moved (Stage III).

But did these students merely use the word "living" as equivalent to "active" or "moving," or did they, as children do, attribute other traits to these "living" objects? A few questions were asked to attempt to get at this matter. With the graduate students of education, after the questions previously reviewed had been answered, they were immediately asked the following additional questions:

"Many ships are lost at the bottom of the sea. We cannot find them. Do you think the sea itself knows where they are?"

"This pearl was once in a shell in the sea. When the water moved, could the pearl feel the movement of the water?"

Of the students who had given one or more animistic answers to the earlier questions, one third attributed consciousness to the sea or to the pearl or to both. The answers regarding the sea were as follows:

"Yes, the chemicals in the sea come in contact with sunken vessels and are aware." "Yes, the sea does know the location of the lost ships because they are in the bottom of the sea." "Yes, if it [the sea] is living, it ought to." "Yes, the sea rubs over the lost ships and knows them to be there." "No, the sea doesn't care to know. There are too many of them. The sea *could* know if it wanted to."

In regard to the pearl, the following replies were obtained:

"Probably as much as a very young fetus might feel the effect of water in the mother's womb." "Yes, the pearl was part of a living thing." "Yes, through the living oyster." "Yes, because it is changed by friction."

The introductory psychology students also were asked the question about the sunken ships. In addition, the following question was asked: "The tides are caused by the pull of the moon upon the ocean. Do you think the ocean can feel the pull of the moon which causes high tides?"

Of the 12 college students who had said the sea was living, five attributed consciousness to the sea in reply to one of or both these questions. Their responses were similar to those already quoted.

Obviously much more extensive investigations of the attribution of consciousness to objects lacking a nervous system are called for, but the data just presented demonstrate clearly that such anthropomorphism is not limited to children.

One would not be surpised if ideas such as those just described were found in a primitive or a backward group (I have data showing that in some little-educated groups the percentage animating the sun and other natural objects is close to 100). But, in my experience, few psychologists except to find such ideas among teachers and college sophomores. It should be borne in mind that teachers and sophomores often have had no more specific instruction than primitive peoples concerning the distinction between the animate and the inanimate and concerning the dependence of consciousness upon a nervous system. Apparently, in the absence of specific instruction, "educated" persons in modern societies possess many conceptions of the world that are identical with those of the child and of the uneducated. A fuller discussion of the importance of this fact must await a later occasion. Meanwhile it is hoped that others will avail themselves of opportunities to verify, expand, and refine the observations reported here.

WAYNE DENNIS

Department of Psychology
Brooklyn College

References

1. PIAGET, J. *The Child's Conception of the World.* New York: Harcourt, Brace (1929).
2. RUSSELL, R. W. *J. Genet. Psychol.*, 56, 353 (1940).
3. DENNIS, W. *J. Abnormal Social Psychol.*, 38, 21 (1943).
4. DENNIS, W., and RUSSELL, R. W. *Child Develop.*, 11, 181 (1940).
5. RUSSELL, R. W. *J. Genet. Psychol.*, 57, 83 (1940).
6. DENNIS, W., and MALLINGER, B. *J. Gerontol.*, 4, 218 (1949).
7. RUSSELL, R. W., and DENNIS, W. *J. Genet. Psychol.*, 55, 389 (1939).

A POSSIBLE IMPLICATION OF THE DUNE TOPOGRAPHY OF THE SOUTHERN COLORADO PIEDMONT*

AMONG the more outstanding topographic features in the foothills of the Colorado Front Range are the excellently developed pediments. Those lying to the west of Fountain Creek (Fig. 1) have been well described by B. A. Tator in recent volumes of the *Bulletin of the Geological Society of America* (Vols. 60, 62).

It has been my privilege to work with Tator in his research on the mountain-front pediments over a considerable number of years, and I am essentially in agreement with his conclusions as to how they were formed. They are fairly extensive, remarkably flat, bedrock surfaces (Fig. 2), which truncate the upturned sediments with seeming disregard for the varying resistance to erosion one expects from rocks with differing lithologic characteristics. The slope of the surfaces compares favorably with the gradients of the associated streams—slopes up to 200 feet per mile not being uncommon. Usually the pediments are covered with a few feet of alluvium, which in bouldery character and granitic composition also corresponds fairly well with the material carried by the streams of the area today.

Tator explains the pediments as being the erosional product of intermittent streams in a semiarid region. Very briefly, the process calls for a shifting of the stream channel as a result of self-plugging by debris left by the heavily laden and turbulent flow during and after a storm, and sidewall retreat produced by weathering and attendant downslope mass movement. The formation of surfaces at more than one general level is explained as the result of climatic change that prolonged stream flow and increased downcutting during a subhumid cycle between two periods in which semiaridity provided optimum conditions for pedimentation.

During the past few years our attention was attracted to, and research was started on, similar surfaces that lie to the east of Fountain Creek. I have undertaken the major portion of the research there and originally I had the idea of turning out a supplement to Tator's work. It was our belief at first that the mountain-front pediments and those east of Fountain Creek were probably formed by similar processes; therefore, my work was to be primarily concerned with delimiting the surfaces and determining their sequence of development. I have found the work much more involved than that, however. There are, of course, similarities between the surfaces of the two areas, but also there are what currently seem to be important differences.

Surfaces East of Fountain Creek. Even a cursory study of the excellent new $7\frac{1}{2}$ minute quadrangle maps (Pike View, Falcon, Falcon N.E., Colorado Springs, Elsmere, Corrall Bluffs, Fountain, Fountain N.E., Fountain S.E., Buttes) of the area east of Fountain Creek will reveal some facts about the pediments. In the first place, they are much better preserved than the mountain-front pediments and so, at least, appear to be more extensive. Second, they can readily be divided into two groups. One group, which is obviously younger, slopes to the southwest and is apparently controlled by the present drainage line of Fountain Creek. The older group slopes to the southeast and is graded to drainage lines that lie at some distance to the east of the modern course of Fountain Creek, which carries the mountain-born drainage to the south.

Early field work revealed other differences and at least one similarity between the pediments of the two areas. The younger slopes to the east differ from the mountain-front pediments in the nature of their alluvial cover and in their direction of slope. The alluvial cover of these southwestward slopes is a sandy gravel ranging in size up to pebbles, as compared to the bouldery gravels of the southeastward sloping mountain-front pediments. They are like the younger mountain-front pediments in that they are both graded to Fountain Creek. The older surfaces east of Fountain Creek may actually be remnants of the older mountain-front pediments. Their slope is in the same southeasterly direction, and their alluvial cover also contains boulders of ancient crystalline rock. The most significant difference between these older eastern surfaces and their mountain-front counterparts is the dune topography that modifies large expanses. The dunes were first detected on aerial photographs and later verified in the field (Fig. 3). So far as is known there are no dunes west of Fountain Creek.

Dunes. The full extent of the dune topography has not yet been determined (Fig. 1); however, it is known to reach from the areas described by Gilbert and by Fisher (east and north of Pueblo) north to the Black Forest (which lies on the divide between the South Platte and the Arkansas), the

* Based on a paper presented in the joint session of AAAS Section E and the Geological Society of America during the Annual AAAS Meeting, St. Louis, Missouri, December 26–31, 1952.

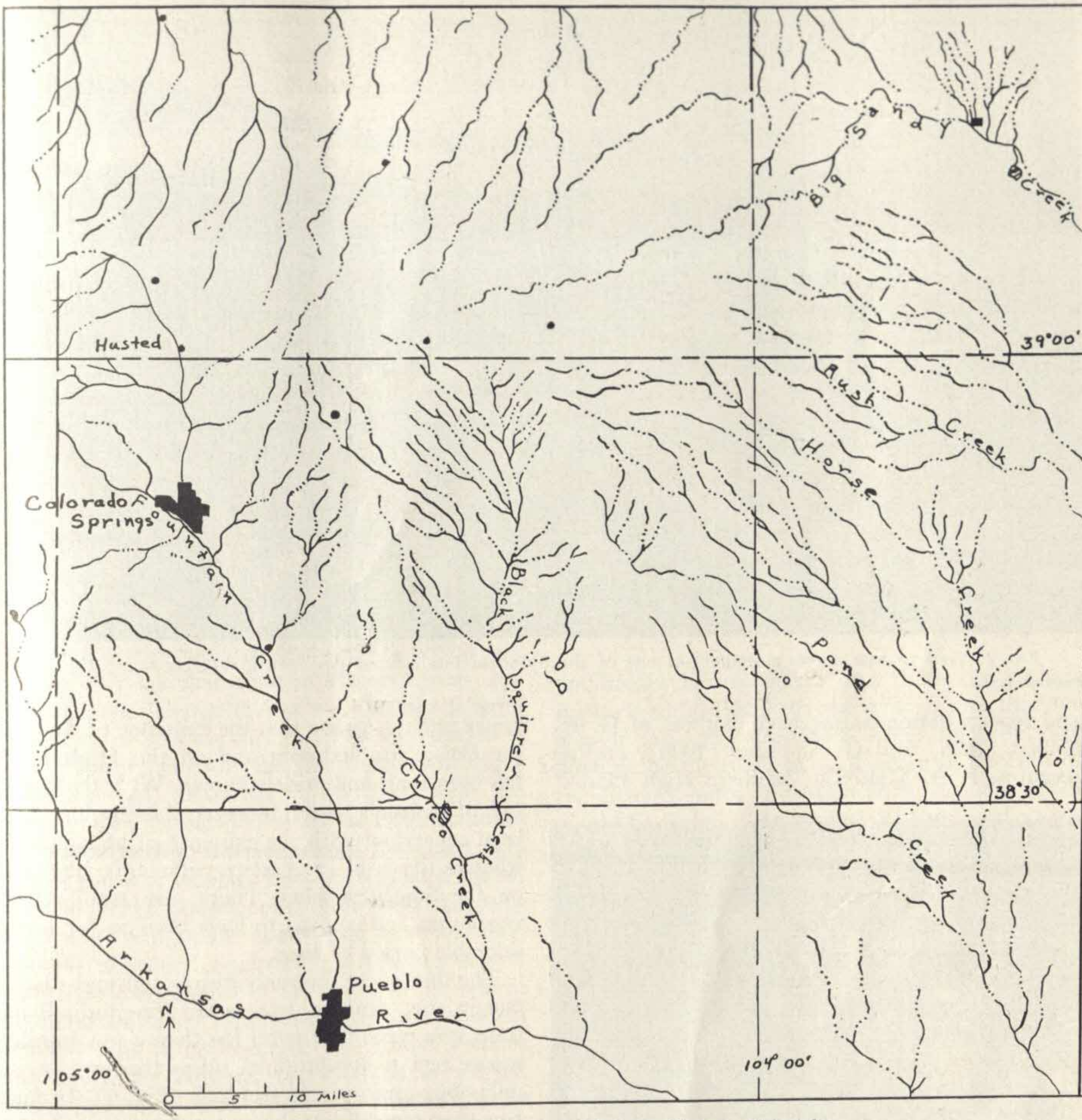


FIG. 1. Drainage map of eastern El Paso County, Colorado.

southern boundary of which it skirts westward to the vicinity of Husted. It is well developed in the northeast environs of Colorado Springs, whence the western limit swings south and east, coinciding to a marked degree with the divide between the eastward-sloping older surfaces and the younger ones that slope to the west. These tentative boundaries enclose an area of more than 1400 square miles, for the most part characterized by extensive flat surfaces the topography of which is nearly featureless, except as it is modified by the younger dune topography and the main drainage courses.

That there are no dunes west of Fountain Creek, even though there are surface remnants there as old as some of those covered by sand to the east, is probably best explained by the fact that the mountain mass undoubtedly received sufficient moisture even during more arid times immediately to the east to support vegetation similar to what it has today. In addition, the prevailing winds were apparently from the northwest, as shown by the orientation of the transverse dunes (Fig. 3), and so they could not have delivered sand to the mountain front from the desertlike piedmont to the east, where dunes were actively forming.

The recently published map of eolian deposits,

FIG. 2. View to east across a segment of one of the older mountain-front pediments, showing truncation of upturned beds.

and reports dating as far back as those of G. K. Gilbert (1896) and C. A. Fisher (1906), and as recent as F. A. Melton's "Southern High Plains" paper (1940), prove that the existence of dunes in the Colorado Piedmont and on the High Plains has been long and widely known. With the exception of Melton's paper, however, these reports have been concerned with the existence of active dunes, whereas those on the eastern pediments, like those on the Southern High Plains, are stabilized by vegetation and appear to have been so for a considerable period of time.

FIG. 3. Vertical view of area approximately 9 square miles, showing well-developed ancient transverse dunes trending about N 45° E. Very light areas are modern blowouts on lee side of old dunes, indicating a radical change in direction of effective wind. (Used by permission of U. S. Geological Survey.)

The dunes are unevenly distributed and diversified in size, ranging from small longitudinal and transverse types that cover less than a few hundred square feet to longitudinal ridges that are several miles long and over 30 feet high (Fig. 4). In numbers they range from dune complexes, which completely blanket areas of many square miles, to areas of comparable dimensions that are almost completely devoid of dunes.

The variation in particle size in the sands of different dune areas seems to be related to the sources of the materials. From the few samples that have been studied, it appears that there are two major sources for the sand. On the surfaces underlain by the Pierre shale, the sand is rather well sorted and fine-grained, as shown by the curve on the right side of Figure 5, whereas the dunes found on the surfaces underlain by the Fox Hills and Laramie formations, and particularly by the Dawson, are composed of poorly sorted sand with

particles up to fine gravel in size, as shown by the other curves in Figure 5. This pronounced difference is thought to have resulted from the fact that the fine-grained, well-sorted sands were derived for the most part from sand-filled, intermittent stream courses, whereas the relatively coarse-grained and poorly sorted material was to a larger extent derived locally from the underlying residual mantle. This would be especially true of the rather extensive areas in the north, which are underlain by the poorly cemented Dawson arkose.

Relation of the Dunes to Pedimentation. Although much additional work needs to be done before the exact relationship of the dunes to the pediments can be established, every dune thus far examined is resting on the pediment or on the relatively thin mantle of alluvium that usually covers the pediment. It seems quite probable, therefore, that whatever circumstances were responsible for the formation of the dunes undoubtedly also played an important role in the history of the surfaces. The abundance of dunes on at least portions of all the southeastward sloping surfaces, in contrast with their complete absence on similar but younger surfaces that slope to the southwest, implies a climatic change to desert conditions between two stages of surface cutting. This implication is further strengthened by the fact that, with only a few exceptions, the dunes of the area today are stabilized by a vegetative cover that supports large-scale cattle raising, whereas within the dunes very little in the way of plant remains has been found.

If we accept Tator's conclusions that the pediments were cut under conditions of semiaridity, and that dissection occurred during more humid times, then the presence of the dunes in the eastern area takes on added significance and perhaps offers added weight to his concept. The dunes seem to have developed between two periods of surface cutting and thus would indicate a climatic change of sufficient magnitude to have interrupted any pediment development in progress at the time of the change. If the pediments were cut during the optimum semiarid conditions, and if a climatic shift toward more humid conditions was accompanied by dissection of the pediments, then, logically, a change from semiaridity to aridity would have resulted in excessive alluviation on the pediments, or even the development of sand dunes.

The validity of Tator's concept depends in large part on the demonstrated fact that the alluvial cover on the pediments is rarely thicker than the depth of scour of the streams that formed them; however, he reports some instances of alluviation in excess of 100 feet on the older mountain-front pediments. He recognizes that there is a planate bedrock surface beneath the gravels and that the gravels are water-deposited. Their thickness may possibly be the result of a decrease in gradient of the heavily laden mountain-born streams, which would be forced to drop much of their load on the pediment. However, a re-examination of these older mountain-front surfaces designed to determine their sequence and to correlate them with the dune-modified eastern surfaces, suggests another explanation of the varying thickness of the alluvium.

Increasing aridity in the area in general would not necessarily mean a decrease in the transporting

FIG. 4. Vertical view of an area approximately 9 square miles, showing the complex nature of dune types and distribution. (Used by permission of U. S. Geological Survey.)

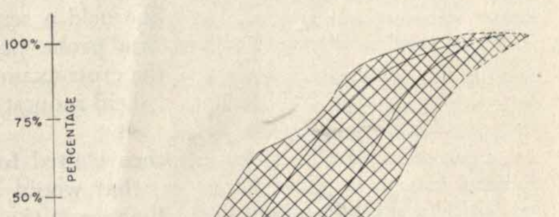


FIG. 5. Cumulative curves plotted to show wide range in sorting and size of material found in the dunes.

ability of the permanent mountain streams, which rise in the snowfields high above the piedmont. On the other hand, it could very well mean a decrease in the incidence of storms along the mountain front and in the piedmont. Since these storms are almost the sole source of water for the intermittent streams that move the mountain-born alluvium across the pediment, any prolonged diminution in volume should result in an accumulation of alluvium on the pediment. Thus, the increase of aridity as implied by the dunes to the east of the mountain front might well have stopped pedimentation and initiated alluviation on the newly formed pediments along the mountain front.

KEITH M. HUSSEY
Department of Geology, Iowa State College

Bibliography

GILBERT, G. K. *Undeground Waters of the Arkansas Valley in Eastern Colorado,* U. S. Geol. Surv., 17th Ann. Rept., Pt. II, 578–80 (1896).

FISHER, C. A. *Nepesta Folio,* 135, 3, U. S. Geol. Survey (1906).

MELTON, F. A. *J. Geol.,* **48,** (2), 113, Figs. 1–31 (1940).

TATOR, B. A. *Geol. Soc. Am. Bull.,* **60,** 1771 (1949); **62,** 255 (1952).

PHILOSOPHER OF AMHERST

She scrutinized existence,
  she turned life inside out;
hers was an incredulity
  that even doubted doubt.

She drew no swift conclusions
  but with Socratic mind
held counsel with a butterfly
  and interviewed a wind.

She held a seminar for dawn
  to probe time's mysteries;
she cross-examined katydids,
  held inquest for the bees,

then waited for the hemlock
  that would at last disclose
the secrets she could not extract
  from butterfly or rose.

MAE WINKLER GOODMAN
Cleveland, Ohio

BOOK REVIEWS

WHEELS WITHIN WHEELS

Flying Saucers. Donald H. Menzel. Cambridge, Mass.: Harvard University Press, 1953. 319 pp. $4.75.

EVERY reader should hail this excellent evaluation of "flying saucer" tales as a distinct service both to science and to the lay public. It was painstakingly prepared for thought-provoking but easy reading. It does not assume any background of scientific knowledge on the part of the reader, and will even help him acquire an ability to weigh evidence and reach scientific conclusions. Perhaps its most useful passages are those explaining scientific methods of approaching unusual physical problems, together with others designed to give every man the ability to safeguard himself against being deluded by fiction masquerading as truth.

Pretenders to scientific knowledge seem to assail the gullible at every turn, sometimes merely to get a tall story across, but often to gain business advantages over a competitor. As the Harvard astrophysicist points out, "Pure-food and narcotic acts protect us from potentially dangerous medicines, foods, or drugs. Yet exploitation of the minds of the American public, feeding them fiction in the guise of fact under the protection of a free press, or frightening people with fanciful ghosts—these, too, are potentially dangerous." Those who cannot spend years of study in theoretical physics, astronomy, or biochemistry do have a means of protecting themselves against the modern deluge of humbuggery. They can scrutinize the source and authority of the evidence. They can test details, look for false premises, gaps, and illogical conclusions.

"With a little thought," says Dr. Menzel, "we can detect fallacious logic, even when it is disguised with scientific vocabulary. Basically, the reasoning of much pseudo science is like the example of the table that turned into a dog: A dog has four legs; the table has four legs; therefore the table is a dog." Or another argument once popularly accepted: "Animals have legs and muscles. Animals can move. The earth has no legs or muscles. Therefore it must be fixed in space."

Just how would the "scientific detective" go to work on flying saucer stories? How can one explain in terms of reality tales that begin, "I saw—I heard"? For example, Dr. Menzel cites the unusual story originating on the campus of the University of Denver, about "men" from space. A Mr. George Koehler introduced a Mr. Newton, supposedly an authority on flying saucers, to the instructor of a class in general science. Newton told a fantastic story (the heart of a later book by Frank Scully entitled *Behind the Flying Saucers*). Widely reported in the press, a wave of incredulous acceptance swept over much of the country. The speaker seemed sincere and had a certain air of technical authority. He told of government attempts to keep secret the arrival of saucer-borne creatures from another planet; miniature men scarcely 44 inches tall, beardless, dressed in 1890-style clothes, of a fiber that could not be identified; wearing shoes of a material resembling human skin. One ship had supposedly crashed near Aztec, New Mexico, another near Durango, Colorado. Dr. Menzel characterized the reported technical description and operation of the saucers as mere "scientific double talk, of no significance at all except to give the impression of authority."

Although some educators have criticized the university for allowing such a lecture to be given, especially by a speaker who attempted to keep his identity a secret, it seems to have served the useful purpose the instructor hoped for. It gave students an opportunity to test basic rules of scientific discipline, to determine for themselves how much credence such testimony was worth. Eight classes valued the lecture at zero in terms of acceptable authentic information, but gave it a very high score as entertainment.

Professor Menzel devotes the major part of his book to denouncing what he calls the "cult of the flying saucers." Saucers are real, he says, in the sense that people have seen something. But they did not see what they thought they saw. Saucers have no connection with atomic experiments; they are not devices invented by Russia; nor are they interplanetary space ships.

On the positive side, he lists true saucer sightings in recent years as

> reflections from material objects; distant planes, jet aircraft, vapor trails, miscellaneous balloons, newspapers, kites, birds, peculiar clouds, spider webs, insects, feathers, etc. Searchlights playing on thin layers of cloud or mist account for many. Venus, Jupiter, various stars, bright fireballs, and even the moon shining through broken clouds have been identified frequently as flying saucers.

Some 20 per cent of sightings that do not fit in these categories are placed in the rags and tags of meteorological optics": mirages, reflections in mist, refractions and reflections by ice crystals. Some phenomena are probably related to auroras, others to unusual forms of shooting stars. The book covers just about all the reports in modern times and follows the trail of unusual visions back through history to early Biblical days. The visions of Ezekiel and Jeremiah, tales of the Flying Dutchman, the books of Charles Fort, Dr. Gee, Lubbock lights, Project Saucer, Specter of the Brocken, temperature inversion, unknown lights of Japan, and the Galloping Ghosts of Nansie Shoto—all receive attention.

A tremendous amount of work went into the preparation of this book. Dr. Menzel should receive the grati-

tude of scientists and laymen alike for his ability and his willingness to shed light on a perplexing, even frightening subject, one that is only remotely allied to his real interest—astronomy.

Of course there are flying saucers—as real as flying dragons, wheels, chariots of fire, flaming cherubims, angels, and rainbows, too! We learn slowly that things are not always what they seem. The search for truth must go on continually, else we could only despair with Sir Alfred Lyall:

"I think till I'm weary of thinking,"
Said the sad-eyed Hindu King,
"And I see but shadows around me,
Illusion in everything."

HERBERT B. NICHOLS
U. S. Geological Survey

HARVEY'S CLASSIC

The Genera of South African Flowering Plants. Botanical Survey Memoir No. 25. E. Percy Phillips. Pretoria: Division of Botany and Plant Pathology, Department of Agriculture, Union of South Africa, 1952. iv + 923 pp. £2.

IN 1868 the second edition of William Harvey's classic *Genera of South African Plants* appeared. The Botanical Survey Committee of the Union of South Africa entrusted to Dr. Phillips the difficult task of preparing an up-to-date manual that would be a successor to Harvey's *Genera.* This work appeared in 1926, spanning fifty-eight years of great activity and ever-widening knowledge of the new lands that were to be Natal, Transvaal, and South West Africa. Botanical research and discovery in these territories and the Cape Province revealed many new genera and the necessity for revision. Dr. Phillips' second edition successfully links the past twenty-five years of botanical investigations into one comprehensive volume "within the means of the ordinary student of our native flora." Dr. Phillips' colleagues may well greet this work as a sterling contribution to South African biological science; students of the South African flora will find it an indispensable manual. It marks the industry, skill, and learning of one who has devoted all of a professionally active life, and the greater part of an officially retired period, to the perfection of his labor.

The 1926 edition was largely a compilation of 1645 genera gathered from the available literature; this was an increase of 459 genera over William Harvey's list of 1868. The 1951 edition has been enriched by an additional 142 described genera. With the exception of the group of genera related to *Mesembryanthemum,* all the genera cited have been personally examined and described by the author, to which he has also added new generic records and annotations. He follows the system of de Dalla Torre and Harms, which is the arrangement followed in the National Herbarium; and where authorities differ in the placing of genera, the author has properly entered appropriate comments. The first edition contains a synopsis of Engler's system of classification; this has been omitted from the second edition.

The key to the families has been increased by 13 pages, which is partially accounted for by slightly larger type, but there are many changes aimed primarily at simplification. In preparing the keys to both families and genera, emphasis has been placed upon characters visible to the naked eye wherever possible. As in the 1926 edition, the keys are artificial, and stress has been given to characters exhibited only in the South African plants. Thus the manual is specifically designed for the needs and encouragement of South African students. This is clearly brought out in the introduction, which consists of three pages of instructions addressed to "Any student who has a keen desire to become acquainted with the flora of his district. . . ." It includes concise information on the correct procedures for collecting and maintaining a herbarium, the use of the keys, and the method for examining both fresh and dried plant material. (So few of our masters' works begin on such a practical and elementary level.) The introduction is followed by 44 pages of "Key to the Families" and 806 pages of text, which are generously printed with clear type and wide margins, adding much to the appearance. A bibliography and index of 66 pages complete the work.

Three especially valuable features of this edition are the references to the original publication of the genus, the citation of the type species where possible, and the excellent bibliography of 48 pages. The bibliography is arranged alphabetically by genera, so that the needs of the moment can be quickly met without the distraction of extraneous material. It also includes in its alphabetical arrangement three nongeneric items that would be quickly revealed through use but could easily be missed by the casual reader; these are the related subjects on nomenclature, taxonomy, and floras. Under "Floras" alone, some 56 works are cited, including publications of 1951. The bibliographic analysis completed in connection with this manual is in itself impressive.

It would be presumptuous to criticize a contribution that so sincerely and effectively develops the author's intent. The area covered is large by any measure and exhibits an extraordinarily high endemism distributed over varied and extreme habitats. There are, unfortunately, no references to geographic categories, but many tantalizing and exotic place names that could not possibly appear on a simple map. Nevertheless, one humble suggestion is offered on behalf of outlanders: the inclusion of a map indicating the more important subdivisions of the area treated, which could perhaps serve as end papers in the volume. This manual is certain to become an integral part of any working library of botanists and students of world floras.

GEORGE A. LLANO
Arctic, Desert, Tropic Information Center
Air University, Maxwell AFB, Alabama

PHILOSOPHICAL ANALYSIS

Ontology. James K. Feibleman. Baltimore: Johns Hopkins Press; London: Geoffrey Cumberlege, Oxford University Press, 1951. xix + 807 pp. $9.75.

IN THIS treatise Professor Feibleman presents what he terms "axiological realism" (*A–R* system) as a finite ontology. There are, according to his account, three possible philosophical positions:

1. Idealism: the universe of essence is alone real;

2. Nominalism: the universe of existence is alone real: as (*a*) subjective idealism: the knowing subject alone is real; or (*b*) materialism: the object of knowledge alone is real; and

3. Realism: both the universe of essence and the universe of existence are equally real.

"Reality" means, ontologically, "having independent being" and, epistemologically, "having identification as the object of that which is true." "Being" (essence) means "the power to affect or be affected."

In the opening section an attempt is made to apply this classification to the history of philosophy, with in most instances resulting distortion of the systems referred to. It is true, indeed, that the justification for the system is not sought in history but in the analysis offered. Nevertheless, the author states that he hopes he has included what is significant in each school, including, of course, contemporary movements.

The system subdivides into (1) the universe of essence, (2) the universe of existence, (3) the subordinate universe of destiny.

The universe of essence is the order of possibility. But these possibilities do not ever have to become actualized to prove that they are real. "Being [essence] is a power, but is indifferent to the exercise of that power, since it is the power itself."

The universe of existence is the actual, the spatiotemporal. "Existence may be defined as whatever affects or is affected. Existence thus includes the definition of essence, but only in the active sense." It depends on essence for its existence.

"Destiny is the direction of existence toward essence." It is not a separate universe, but an interrelation between the other two.

These are presented in an elaborate and very detailed development of all the aspects: postulates, categories, types of structure, levels of being, degrees of integration. Since it attempts to be all-inclusive in scope, there are naturally included classifications of all the sciences and disciplines hierarchically arranged.

A systematic summary (pp. 663–684) is presented as a postulate set. These include for the universe of essence, 117 postulates; for existence, 160 postulates; for destiny, 123; and in addition, for the special case of epistemology, 111; for the "all-presumptive calculus," 88—a grand total of 599 postulates. In connection with this the author states: "The postulates of a system, while the chief elements, are not the whole system. In this treatise not all the elements of the *A–R* system are given, and those that are given are not presented systematically. . . . The claim of the *A–R* system is to be all of the truth available at this time, i.e. with the theories and facts now known. It will be supplemented and perhaps largely replaced as knowledge increases. Thus it claims to be the discovery of the truth but is fallible."

Nevertheless, without sacrificing the wide range of topics and material included, the book could profitably be greatly condensed. There is much repetition where the restatement in no manner advances the argument. In fact, as far as any substantial contribution to philosophical analysis is concerned, that could adequately and more effectively be presented in a brief essay.

FRANCIS P. CLARKE

Department of Philosophy
University of Pennsylvania

BRIEFLY REVIEWED

The World of Eli Whitney. Jeannette Mirsky and Allan Nevins. New York: Macmillan, 1952. xvi + 346 pp. Illus. $5.75.

THE title gives the reader promise of more than a biography. The tale centers, of course, about a Connecticut Yankee, but its setting interrelates cotton plantations of the South with the arms factories of Mill Rock, near New Haven, Connecticut. One expects the biography of a man but finds the text uncovering the evolution of an idea.

Born ten years before Bunker Hill, bereft of his mother at the age of five, the eldest of four children, Eli Whitney, Jr., early developed resourcefulness and self-direction. Schooling was incidental until at about the age of eighteen he expressed a desire to go to college. His preparation in academies and his self-support by intermittent teaching eventually earned his father's reluctant consent. During the final years at Yale that consent was confirmed by his father's financial help.

After graduation he accepted service as a tutor in a South Carolina plantation family. Actually, he never entered that service. Instead, he invented the cotton gin during a half-year sojourn in the home of General Nathanael Greene's widow near Savannah, Georgia.

For a period of years following, his time was given to alternate "tooling" for the gin's production, litigation over patent rights, and disheartening negotiations for working capital. This third need was the spur that presently prompted application to the newly formed federal government for a contract to manufacture arms for its army. In the struggle to meet the time

limits of that contract the principle of "parts production" for a sort of assembly-line pattern was evolved. In the practice of parts production he began the system of fabrication by "tools." In this he depended on his machines for the accuracy of the operation rather than on the skill of the workmen and so was able to employ unskilled labor for servicing the machines. In all of this one sees the elements of what later became mass production, so significant in the rise of such industries as those associated with the names of Colt, Singer, McCormick, and Ford.

Whitney's success in arms production for the government required contracts of long duration; political shysters often pulled him from his mills to lobby for his program of production. In these frequent sojourns at the seat of the national government he became well known to many of the nation's leaders, including the presidents, of that day.

Romance came late for the inventor. He did not marry until after the death of Widow Catherine Greene-Miller—perhaps because between General Greene's wife and Whitney there was "rooted a delicious friendship—that had flowered for twenty years." His own silence in regard to this aspect of his life leaves the reader only conjectures. At the age of fifty-two, he married Henriette Edwards, grand-daughter of Jonathan Edwards. She was twenty years younger than her husband. His eight remaining years were cheered by the birth of his three children and a happy domestic atmosphere.

The authors, the second of whom is well known and widely read, have very skillfully contrived to bring much of the account to the reader in the words of the letters from which the facts are drawn. In the words of the authors, their subject was a major contributor to the industrial progress of his country: "His genius, his skill, his persistence became puissant forces by which, in the developing Republic, unity defeated disunity, uniformity replaced diversity, technical expertness supplanted a haphazard rule of thumb, and Plenty was empowered to conquer Want."

B. Clifford Hendricks
Longview, Washington

The Medieval Science of Weights (Scientia de Ponderibus). Treatises ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore, and Blasius of Parma. Ernest A. Moody and Marshall Clagett, Eds. Madison: University of Wisconsin Press, 1952. 438 pp. Illus. $5.00. (Lithoprinted.)

THOSE who discuss the historical background of modern science and technology will often vault, mentally, from the achievements of Aristotle and Archimedes, of an ancient world, to the times of Galileo, Descartes, and Bernoulli. The accomplishments of the thousand years of medieval history are brushed aside as immaterial. Those, however, who know the medieval period best have discovered for themselves that the final four centuries of that period were not unimportant either to technology or to creative thought.

The book under review deals in a special and limited way with certain accomplishments of those four centuries, in western Europe, in relation to that part of the physical sciences known as statics. As a background for the presentation, there is swept in for the reader the effect of the rapidly flowering Arabic scholarship in perpetuating and carrying forward the older Greek ideas. Through the Moorish universities of Spain the knowledge of the newer Arabic mathematics, the Arabic number system, and a more dynamic approach to the problems of force and weight reached Christian scholastic teachers in the Latin west. The result was fruitful. In the early thirteenth century Jordanus de Nemore, of France, was a central figure in extending Arabic conceptions into the field of mechanics.

To establish clearly the accomplishments of the medieval scientific writers, the authors use the first 279 pages to present eight treatises on weights and related topics, ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore, and Blasius of Parma. All are based upon Latin-written parchment manuscripts of medieval times. All have been carefully edited and are accompanied by adequate English translations. The first four of these treatises were available to, and the other four were produced by, scholastic scholars of medieval Europe. They may be considered, then, to give the background for, and the contributions made by these scholars to, the science of statics. To be found among the contributions are the general lever principle, that of the inclined plane, the composition of forces, and virtual displacements, with some suggestion of the use of the basal principle of work. All these have commonly been regarded as the accomplishments of later centuries.

The compilers, who are also the editors and translators, are quite modest when they write, "It is hoped that these texts and their translations will be of value to students of the history of science, and serve in some way to remedy the pitiful shortage of modern editions, and particularly of English translations, of medieval scientific writings in this field."

Keith Gordon Irwin
School of Science and Arts
Colorado A. and M. College

ERRATUM

The first sentence in the third paragraph of Bentley Glass' review of *Understanding Heredity,* by Richard B. Goldschmidt, on p. 189 of the March, 1953, issue should read as follows:

"Those who know Professor Goldschmidt's eminence in the field of physiological genetics, and his "radical" views of the constitution of the genetic material and the nature of the mutational changes that participate in evolutionary processes, will be surprised to see that these radical ideas are scarcely alluded to. . . ."

Due apologies are made for an embarrassing printer's error.

THE SCIENTIFIC MONTHLY

MAY 1953

A Chronology of Postglacial Time in Eastern North America*

RICHARD J. LOUGEE

In 1928, following his discovery of evidence indicating the catastrophic draining of Glacial Lake Hitchcock and the sudden birth of the postglacial Connecticut River, the author of this article was commissioned by Isaiah Bowman to begin chronological investigations of the Ice Age for the American Geographical Society. A summary of his surveys is presented here for the first time.

RAISED marine shorelines extend to elevations hundreds of feet above sea level around the borders of glaciated North America, but it is not generally known that they preserve a record of uplift of the earth's crust which amounts to a history of postglacial time. This paper summarizes a larger study, and without citing the detailed evidence, presents an outline of postglacial history in eastern North America furnished by records of upwarping.†

Upwarped shorelines were originally horizontal, and their tilted attitude today gives a clue to the amount and type of land movement that has affected them. They offer, in fact, almost the only means by which prehistoric changes of level can be accurately measured. In the region of former Glacial Lake Agassiz, the Glacial Great Lakes, New England, Labrador, and Arctic Canada, where there are raised marine or raised lacustrine shorelines, or both, the shorelines rise progressively higher in directions converging on the former ice cap center in Hudson Bay, indicating that crustal upwarp has been associated with removal of ice weight.

Mechanism of Crustal Upwarp

Despite the apparent domelike pattern made by upwarped shorelines around the former ice center, a correct representation of postglacial upwarp is inverse to the form of a dome. If the earth's surface is considered in its relation to the shorelines when they were horizontal, prior to upwarp, it is found that much of New England that slopes seaward today then declined in a direction inland, and the crust must have been in a downwarped or basined condition toward the ice center. Upwarping has been a process of elimi-

* Based on a paper presented at the XVII International Geographical Congress, Washington, D. C., August 11, 1952.

† "Crustal warping" is defined as rock-bending in contrast to "faulting" or "folding."

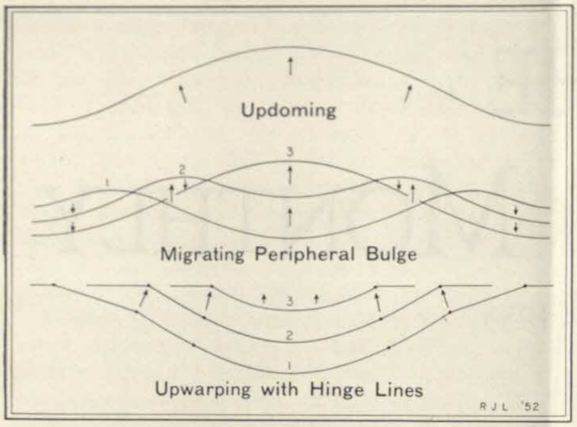


FIG. 1. Concepts of postglacial uplift.

nating this crustal basin. Within the immense area so affected, New England lies just inside the margin. Rise of the crust at the ice center has amounted to thousands of feet during and following deglaciation, and is in progress today around Hudson Bay, where the basined condition is not yet eliminated.

Authorities who interpret the tilted profiles of uplifted shorelines to indicate a direct rather than an inverted pattern of land movement describe the uplift as "updoming;"[1] others, conceiving the crust to be highly flexible, believe that a migrating "peripheral bulge" was succeeded by crustal sinking around the borders of the shrinking ice cap (Fig. 1). Neither of these views has satisfactorily explained the facts. The logic of updoming has mistaken apparent uplift for real; and no shorelines are known which decline in elevation toward the former ice center to substantiate a sunken bulge. Appearances of recent sinking on glaciated and unglaciated coasts alike are readily explained by postglacial worldwide rise of sea level. This so-called eustatic change of level resulting from the melting of glaciers has received scant attention in glacial problems, or the evidence for it has too often been ascribed to negative isostatic movements. If there has been crustal sinking to compensate for continental upwarping, associated with subcrustal undertow of magmas, it has left no signs on the continents, and must have taken place in the ocean basins, which have felt the weight of returning glacial meltwater.

It is not generally realized that raised glacial shorelines are emergent parts of the lower stages of sea level, contemporary with upward growth of coral islands (Fig. 2). Only in glaciated regions have Ice Age shorelines been upwarped by amounts exceeding the worldwide rise of sea level.

Unlike the complex mechanism that has been invoked to explain uplift by updoming and peripheral bulging, crustal upwarp from a basined condition was relatively simple. Upwarping affected not only the ice margins, but the whole glaciated area under the ice cap. Its mechanism resembled that of an old and brittle rubber ball responding to removal of pressure of a finger, except that the earth's crust, unlike rubber, has great rigidity, which accumulating stresses caused by removal of ice had repeatedly to overcome, allowing the rocky shell to rise with a jerky intermittent movement of separate crust blocks, like a broken mosaic. Progressive elimination of the basin was facilitated by a pattern of "hinge lines," or crustal cracks, encircling the basin, and perhaps combined with others radiating within it, which delimited regions under varying degrees of depression. Hinge lines are invisible, for they only bend or crack the crust and do not fault or dislocate it unless in some places local faulting is an added factor. They may be detected where shoreline surveys show changes of tilt in water plane profiles. The Algonquin Hinge Line, which was first discovered in surveys of the Glacial Great Lakes, has been traced 2000 miles (3219 km) across the North American continent from the region of Glacial Lake Agassiz to New England and New Brunswick.[2]

As successive uplifting movements took place,

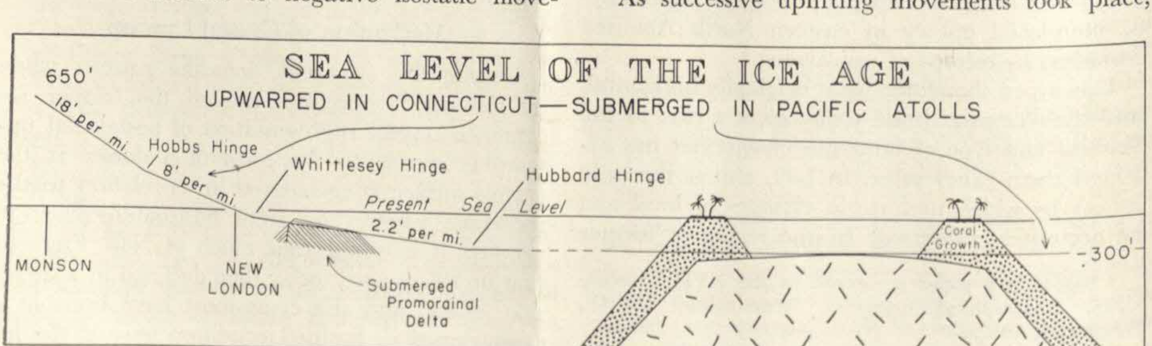


FIG. 2. Relationship of upwarped parts of glacial sea level (Leverett marine stage) to the submerged parts.

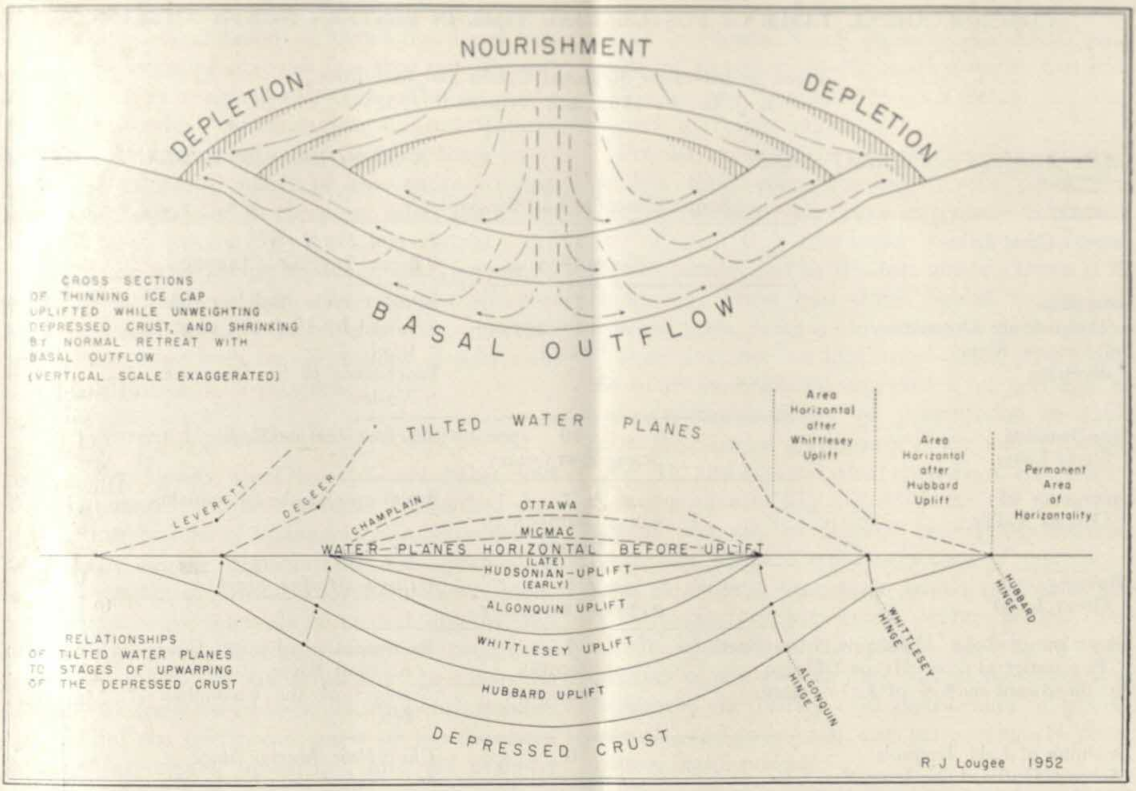


FIG. 3. Comparative diagrams of ice cap mechanics and mechanics of upwarping.

they were restricted to areas circumscribed by hinge lines of smaller circumference. Thus a clearly recognizable iceward shifting of the boundary of upwarp took place from outer to inner hinge lines, and vast regions early uplifted were not involved in later movements.

Two types of land movement took place during uplift. *Differential uplift* was characteristic of the peripheral parts of the basined region and showed its effects in "tilting" of the ice-marginal shorelines. At the same time *uniform uplift* took place in regions farther iceward or under the ice cap. Localities in southern New England on opposite sides of the Hobbs Hinge Line permit study of the two simultaneous movements and their effects on ice-marginal water levels.

Glacial Thinning in Relation to Upwarp

Postglacial upwarp was a response to thinning of the ice cap, for decrease in load on the land induced the upwarping. Contrary to a popular concept known as "downwastage," this shrinking in thickness was not due to melting down the top of the ice cap. If existing continental glaciers may be taken as reliable examples, deglaciation of the last North American ice cap was restricted to a "zone of depletion" (or ablation), probably extending not more than some scores of miles inward upon the high ice surface, which was two or three thousand miles across. This resulted in "retreat," or melting back, of the ice margin, exceeding the rate of forward movement of the ice.[3] Normal retreat of the ice margin induced a general reshaping and thinning of the whole ice cap in response to gravity (Fig. 3).

There has been a tendency among glacialists to overlook the element of time in ice cap mechanics, and to ignore gravity. Ice is a viscous substance, and gravity made stagnation impossible in the last ice cap. Bottom ice in contact with the earth's crust is at melting temperature, and highly plastic. Yet it never liquefies, as shown by the fact that in winter fine clay settles on the floors of glacial lakes, because no disturbing currents from any source enter the lakes at that season. Heat supplied to the base of an ice cap by terrestrial radiation promotes flowage and regelation before dissipating by conduction upward through the ice. Waning ice caps like those of Antarctica and Greenland are becoming thinner while snow continues to

CHRONOLOGICAL TABLE OF POSTGLACIAL TIME IN EASTERN NORTH AMERICA

Based on Successive Regional Uplifts and Stabilities
(R. J. Lougee 1932, and revisions to 1952)

	LAKE AGASSIZ and GREAT LAKES	Stages Since the Last Glaciation	*NEW YORK–NEW ENGLAND–ACADIA*	
	Present Great Lakes	12 PRESENT TIME	*Present Sea Level* Titicut Indians (Taunton)	Stability south of the Algonquin Hinge Line. 100 + ft rise of sea level to Present Time. Up-Growth of Salt Marshes.
	Postglacial Temperature Optimum Bell-Ottawa River diversion	11 LATE HUDSONIAN UPLIFT	Boston Weir Builders followed by 16 ft rise of sea level Emergence of St. Lawrence Valley	
	Post-Nipissing Great Lakes	10 MICMAC STABILITY	*Micmac Marine Stage*	
	Emergence of Ottawa Valley	9 EARLY HUDSONIAN UPLIFT	Isolation of Lake Champlain	
	Nipissing Great Lakes	8 OTTAWA STABILITY	*Ottawa Marine Stage*	
	Diversion of Lake Algonquin from North Bay outlet to Port Huron-Chicago Northeastward outflow of Lake Agassiz	7 ALGONQUIN UPLIFT	Rejuvenation of upper Connecticut River. Regressing stages of the Champlain Sea	
	Draining of Lake Iroquois Mohawk Outlet, Lake Iroquois Kirkfield Outlet, Early Lake Algonquin Campbell Stage, Lake Agassiz	6 CHAMPLAIN STABILITY	*Champlain Marine Stage* Ice retreat in Lower St. Lawrence Valley Coveville and Fort Ann Stages of Lake Vermont	
	Great Lakes diversion to Mohawk-Hudson Valley Birth of Niagara Falls Splitting of late-Whittlesey, Wayne, Warren and Lundy beaches Older beaches tilted north of Whittlesey Hinge Line	5 WHITTLESEY UPLIFT	Draining of Lake Albany; birth of ice-fed Hudson River Coastal regression beyond present shore: Massachusetts to New Brunswick Lake Upham, 2000 varves Ice retreat, Schuylerville, N. Y., to Middlebury, Vt.	
	Earliest Glacial Great Lakes: Maumee to Whittlesey. Lake Chicago. Retreat from Defiance Moraine to Port Huron-Alden Moraine Herman Stage, Lake Agassiz	4 DE GEER STABILITY	Birth of Connecticut River Lake Hitchcock, 4000 varves Lake Albany sea level lake *DeGeer Marine Stage* Ice retreat from Hartford, Conn., to Woodsville, N. H.	
Great Lakes Basins Ice-Covered	Ice retreat to Defiance Moraine Tilting north of Hubbard Hinge Line	3 HUBBARD UPLIFT	Coastal regression in Southern New England Cutting of Hudson Channel Continental shelf emergent	
	Milnor Stage, Lake Agassiz Small lakes in northern Ohio Ice retreat from terminal moraines; maximum crustal depression	2 LEVERETT STABILITY	*Leverett Marine Stage* Lake Hackensack, 2000 varves Ice retreat to Taunton-Monson-Middletown	
	Lake Arthur. Shelbyville-Bloomington Moraines	1 LAST GLACIAL CLIMAX	Lake Passaic Ronkonkoma-Harbor Hill Moraines	

FIG. 4. Historical table of postglacial time: Correlations of the Middle West and the Atlantic Seaboard.

pile on at the center, but this thinning, in spite of continued nourishment, is proof that basal outward flow is taking place so fast that nourishment does not offset marginal depletion. The ice cap is literally "bleeding" to extinction. Crustal upwarping must be considered in the light of these interrelated factors, for uplift of the entire ice cap several thousand feet in elevation offset the effects of thinning, prolonged the deglaciation, and changed the floor profile on which basal ice moved outward and upward from the crustal basin. Readvances and moraine-building during times of uplift may have been manifestations of subglacial crustal adjustments in progress.

Intermittent Uplift, the Basis of Chronology

In Lake Agassiz, the Glacial Great Lakes, and New England it has been reliably established that upwarping, instead of taking place in a continuous movement during deglaciation, was spasmodic.[4] Five intervals of crustal movement were interspersed with five intervals of crustal stability. Because they cover the whole range of postglacial time the uplifts and stabilities may be made the framework for a chronology.[5] During each stability the glacial sea overspread parts of the depressed regions, but crustal upwarping and the draining of glacial lakes kept changing the patterns of land and water. Through the use of carbon-14, marine stages in eastern North America eventually may be found to correlate with marine stages in British Columbia and Alaska, or in such distant glaciated regions as Fennoscandia.

Present sea level is the marine equivalent of present time, corresponding to the concluding stages of the fifth uplift now in progress in Hudson Bay. Present time concludes the record of postglacial history, just as the last glacial climax is its beginning. These two units, added to the ten tectonic units, make a chronology of twelve stages of time since the last glaciation—each stage capable of transcontinental correlation, of portrayal in paleogeographic maps, of presentation in table form for convenient insertion of carbon-14 datings (Fig. 4), and of convenient analysis in the profiles of water planes (Fig. 5). Hitherto the dating of organic deposits has lacked significance because there was no accurate chronology of postglacial time. A tectonic time scale which subdivides the records of fossil-preserving water bodies, and which enumerates the earth movements and changes of sea level as they shifted shorelines and influenced the history and distribution of plants, animals, and early man, can give carbon-14 dates a meaningful position.

Where shorelines that abutted on the ice cap

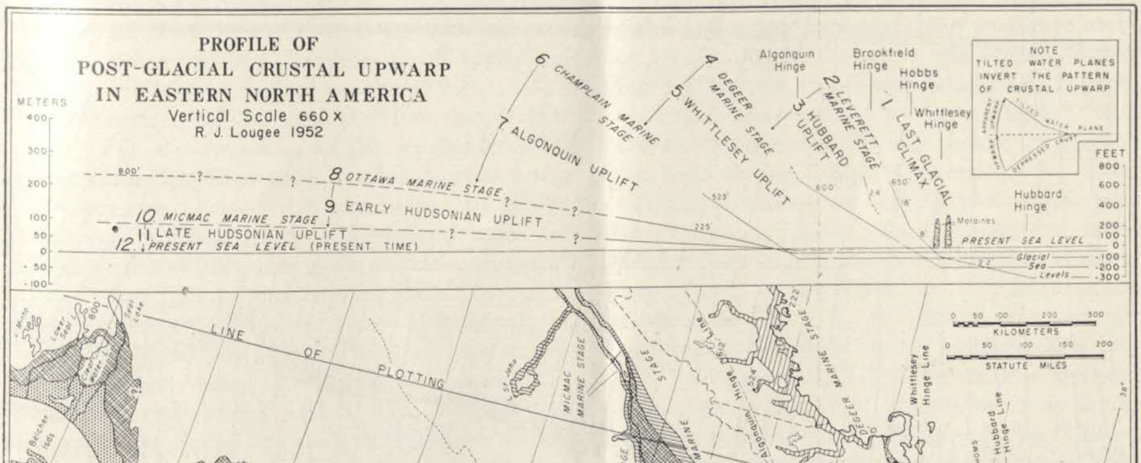


FIG. 5. Profile and map of postglacial upwarp between southern New England and Hudson Bay. For better portrayal, the Micmac water plane is drawn higher than its proper height along the line of plotting.

were altered by contemporary land movements, the ice border at the beginning and end of each of the first three uplifts may be accurately mapped. Thus, despite limited development of moraines in New England, ice borders at several important stages are traceable from the Atlantic coast to the Glacial Great Lakes and the basin of Lake Agassiz.‡ Where a marine water plane can be followed or projected to a hinge line marking a termination of warping, the lower position of glacial sea level at that stage may be calculated, as shown in Figure 5.

Measurement of how much vertical upwarp has taken place at the former ice center is theoretically possible by totaling the differential movements of the separate uplifts. In mose cases, however, differential movement extended some distance under the ice, so that part of the record is lost, and the total amount measurable from the region of the terminal moraines to the Hudsonian ice center, amounting to 2500 feet (762 m) is but a minimum figure at best. Additional depression in excess of 500 feet (152 m) still exists, accounting for the continued submergence of Hudson Bay.

If the modulus of crustal upwarp is indicated in the basin of ancient Lake Bonneville in Utah, where an uplift of 168 feet (51 m) took place in response to evaporation of 1000 feet (304 m) of water,* then the depression that centered on Hudson Bay, amounting to a minimum of 3000 feet (914 m) as measured in the present surveys, may indicate a former ice thickness of about 20,250 feet (6.115 km),§ or 3.8 miles, near the center of the last ice sheet.

Outline of the Twelve Stages of Postglacial Time

1. *Last Glacial Climax* (Fig. 6). At its climax the last ice sheet built a massive moraine which can be traced from the Atlantic to the Pacific, or two moraines, as in southern New England or Ohio. Moraine elevations as much as 333 feet (101 m) on Long Island indicate a thickness of

‡ It is the opinion of the writer that there is no justification for a tripartite division of time based on till deposits (Tazewell, Cary, Mankato) currently employed in the Middle West. Continuous till sheets may be mapped over large areas, but in the present state of their analysis they do not provide definite and measurable factors for delimitation of time.

§ The equation based on columns of water is 168: 1000 = 3000: 18,000.

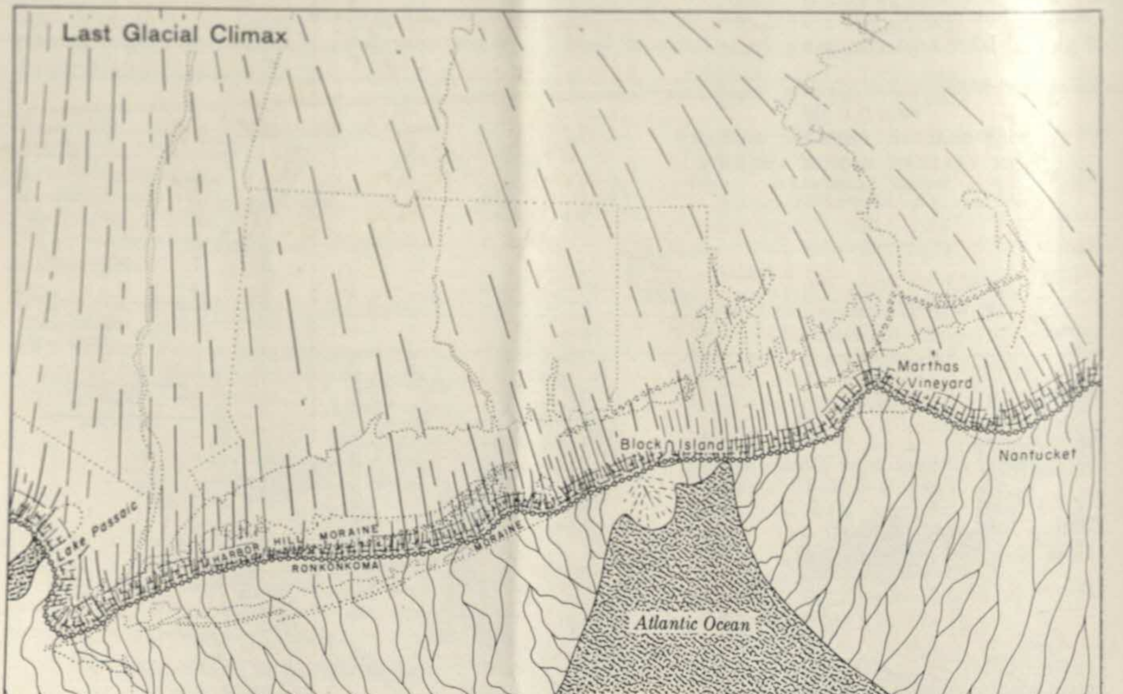


Fig. 6. Last glacial climax.

at least 1000 feet (305 m) of ice in Long Island Sound. Sea level was at or near its lowest position because of abstraction of sea water to form ice caps, and the continental shelf was above sea level. Ice-fed rivers aggraded wherever they spread outwash on land beyond the moraines. Outwash plains associated with moraines on Long Island extend below present sea level, presumably to the submerged deltaic edge of the continental shelf. An exception is found in a promorainal delta on the sea floor south of Block Island, marking one point where meltwater or discharges of ice-dammed lakes from north of the moraine met the shore of the ocean abutting on the ice front at this stage (Fig. 7).

It is not generally recognized that the submerged Hudson Channel which is intrenched in the surface of the continental shelf southeast of New York City could not have existed during the glacial climax, while meltwater streams were aggrading beyond the moraines. Neither could this channel have survived from earlier times under the conditions of aggrading glacial rivers. The Hudson Channel came into being at some time after construction of the outwash plains, or not earlier than a change to erosive conditions following ice retreat from the terminal moraine on Staten Island.

A number of ice border lakes such as Lake Arthur in northwestern Pennsylvania, and Lake Passaic in New Jersey, correlated with the last glacial climax and had shorelines partly inside and partly outside the terminal moraine.

2. *Leverett Stability and the Leverett Marine Stage* (Fig. 8). Recession of the ice border from the terminal moraines while crustal depression was at maximum allowed the glacial sea to spread northward so that it surrounded and made an island of the southeastern parts of Massachusetts,[7] at the same time penetrating the larger valleys of southern New England.[8]‖ Lake Hackensack in northern New Jersey, and the Glacial Tappan Sea in the Hudson Valley, south of the Highlands, may have had connections with the submergence in Long Island Sound by way of the East River. These Leverett marine stage estuaries were kept fresh by meltwater converging on a seaward outlet now submerged east of Block Island. The Leverett water plane in each valley is accurately marked by ice-marginal deltas built at successive positions of the ice border. In the case of the Willimantic Valley estuary, the Leverett waters

FIG. 7. Submerged promorainal delta south of Block Island (from Coast and Geodetic Survey Chart No. 1211).

completely transected the state of Connecticut, and the water plane extended above and beyond the present 612-foot (186 m) drainage divide into the Chicopee Valley of southern Massachusetts. In this and several other estuaries an abrupt northern termination of the water plane shows exactly how far the ice front had retired before uplift concluded the stability.[9] Approximately 2000 varves were formed in the clay beds of the Hackensack basin during this slow recession.

Today elevations on the Leverett water plain rise increasingly higher inland at rates determined by hinge lines. Starting from the initial or Hubbard Hinge Line, the tilt rates are calculated or observed to be 0, 2.2, 8, 18, and 24 feet per mile, measured northwesterly across southern New England. The figure of 18 feet per mile in northern Connecticut and central Massachusetts is double the maximum tilt observed in the Baltic. This exceptional upwarp marks a zone of previous great downwarp of the crust slightly iceward from the terminal moraines. At the northern border of Connecticut in the Willimantic Valley, a marine limit of 650-feet elevation (198 m) is recorded in contrast to zero elevation on the same water plane at New London on the Connecticut coast. Observable emergent parts of the water plane, projected southward, are in alignment with the submerged

‖ Housatonic, Naugatuck, Quinnipiac, Connecticut, Willimantic, Quinebaug, French, Blackstone, Taunton.

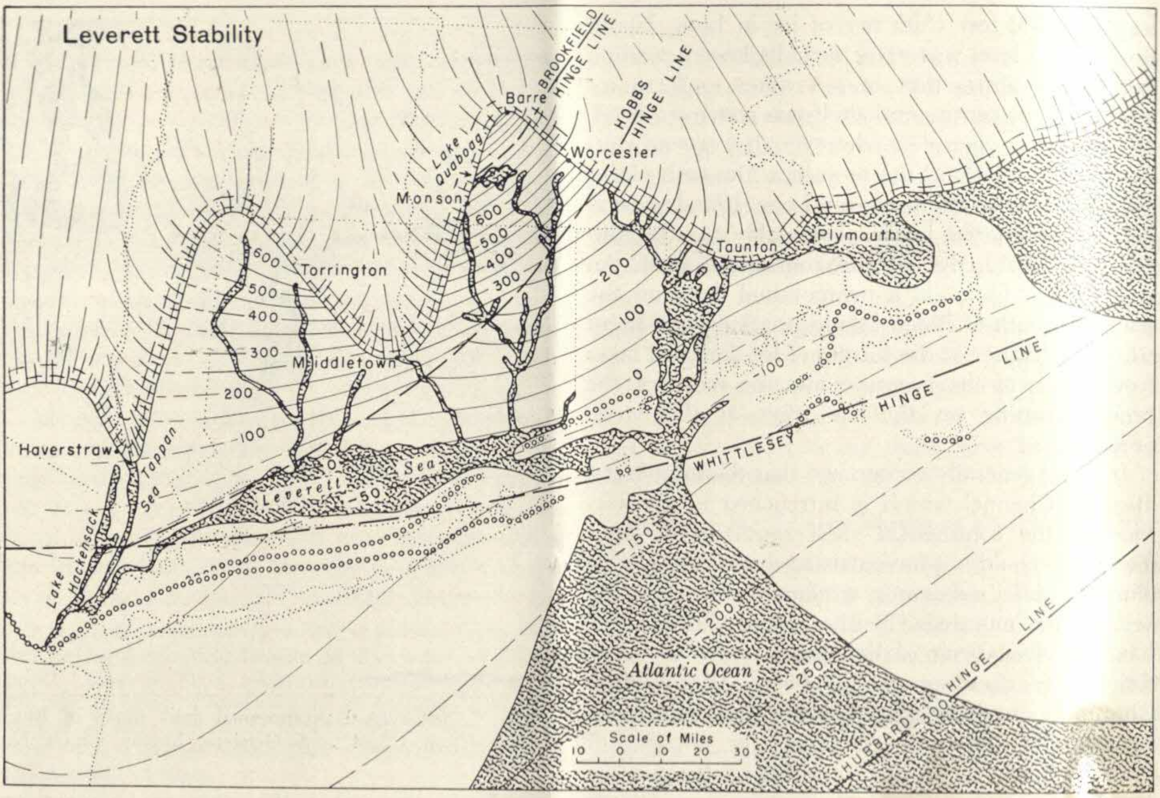


FIG. 8. Leverett stability, with isobases on the Leverett water plane.

promorainal delta 140 feet (43 m) below present sea level, south of Block Island (Fig. 2). The water plane must bend to gentler gradient where it crosses the Whittlesey Hinge Line in Long Island Sound, but horizontality cannot be reached until it encounters an initial or Hubbard Hinge Line. Location of the Hubbard Hinge Line depends on how much sea level has risen since Leverett time. If Leverett sea level lies at or below −300 feet (−91 m), where some calculations place it,[10] the Hubbard Hinge Line is at least 100 miles (161 km) south of Long Island.

3. *Hubbard Uplift* (Fig. 9). When the ice had melted back to a position marked by northern limits of several Leverett estuaries, and had thinned until its border consisted of great lobes in the Hudson and Connecticut lowlands, and over the Boston basin, with deep interlobates in the intervening highlands, the first great regional land movement commenced. Starting at the Hubbard Hinge Line, a wave of permanent uplift progressed northward, tilting successive crust blocks as it shifted to more northerly hinge lines. Hubbard uplift may have tilted an initial stage of Lake Agassiz represented by the Milnor beach. It is well recorded in small glacial lakes in Ohio, south of and antedating uncovering of the earliest Glacial Great Lakes.[11] Abandoned shorelines of Glacial Lake Arthur in western Pennsylvania, and Lake Passaic in New Jersey were tilted alike about 2.2 feet per mile.[12] These lake basins are located iceward of the Hubbard Hinge Line, and south of the Whittlesey Hinge Line. Tilt rates increase in directions generally northwestward across eastern Connecticut and central Massachusetts, but not until northern parts of the central highlands of Massachusetts were uncovered did the wave of differential uplift reach glacial lakes in the upper Ware and Millers river valleys, which are tilted to the extreme of 24 feet per mile.[13]

The most informative records of Hubbard uplift are in New England, where Leverett estuaries were abruptly drained and replaced by powerful glacial rivers. Emergence was more or less responsible for creating high waterfalls on the Housatonic and lower Connecticut rivers.¶ Beginnings of uplift at New Haven, Connecticut, caused ice-fed Mill

¶ Height of the Housatonic Falls below New Milford, Connecticut, exceeded 130 feet (40 m);[14] outlet rapids of Lake Hitchcock below Middletown descended 150–200 feet (46–61 m); and falls at the mouth of Salmon River in East Haddam, Connecticut, fell an estimated 190 feet (58 m).

River to intrench in the widespread 40-foot plain of deltaic outwash in that city, removing part of it to form the immense river terrace 20 feet lower, where the business center of the city is located. As uplift proceeded, Farmington River, flowing southward from the ice border, advanced to New Haven through the Quinnipiac basin, where for at least 800 years there had been a Leverett estuary accumulating red varved clays. These clays the Farmington overspread with white sands and gravels of the Wallingford sand plain before incising the basin to below present sea level.[15]

All rivers from the Hudson eastward to Narragansett Bay were extended seaward beyond the present coast. Stream terracing and intense activity of the westerly wind accompanied exposure of the sea floor; and the Hudson River, spilling southward through the terminal moraine at the Narrows as the lower Hudson Valley was tilted, cut the Hudson Channel as it intrenched in the continental shelf.

Although ice-dammed lakes have been a common feature in New England recessional history, Hubbard uplift was notable for a number of classic examples, which formed constantly changing patterns along the ice border. A steplike series of lakes draining from one to another, or sometimes coalescing, was formed in the northward sloping valleys on the west side of the Boston ice lobe; a similar series formed along the eastern margin of the Connecticut Valley ice lobe. Waters released by drainages of these lakes rushed along the ice borders, building kame terraces and cutting hillside channels, or sometimes discharging bedload over waterfalls to pound out spectacular rock gorges like Purgatory Chasm in Sutton, Massachusetts, or Toffit Falls Chasm where Lake Nashua drained away in Littleton, Massachusetts. Release of ice-dammed Farmington Lake at Tariffville, Connecticut, opened the present northward course of Farmington River through the empty lake basin and diverted the river from its temporary southward course to New Haven. Along the east side of the Connecticut Valley the discharges of ice-dammed lakes built torrential deltas and deltaic kame terraces along the shore of newly formed Lake Hitchcock in the stem of the valley, in so doing accurately recording several early stages of the lake, which was being tilted by land movement. Shores of all lakes of this period, although tilted variable amounts depending on their location and time of existence, are more steeply inclined than shores formed after the Hubbard uplift.

Hubbard upwarp approximated 300 feet (91 m)

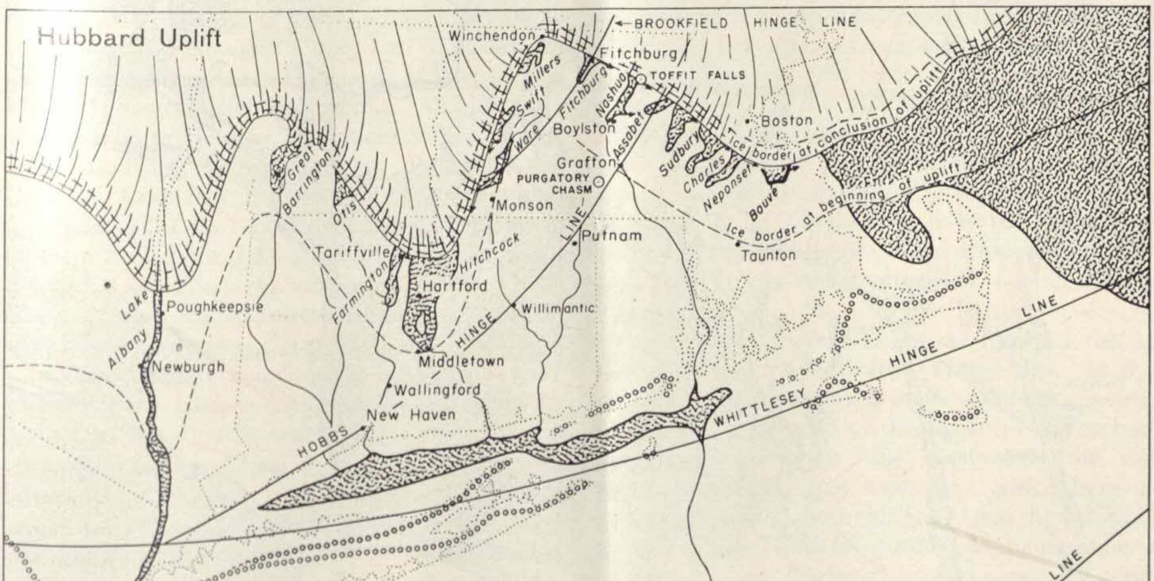


Fig. 9. Hubbard uplift.

in Long Island Sound, and perhaps 1000 feet (305 m) in central Massachusetts. So great was the tilting in the region of Worcester that the location of the divide between Blackstone and Nashua valleys has been shifted northward as much as 11 miles (17.8 km) from Grafton, Massachusetts, to Boylston. Upwarping may have been associated with earthquakes which caused land slips along the margin of the continental shelf. Submarine slides simultaneous with the addition of deltaic material to the shelf from rivers draining the ice are believed to have produced the short, deep submarine canyons indenting the continental slope from Georges Bank to Cape May. Trains of fresh sand extend from the foot of the slope far out upon the floor of the Atlantic basin,[16] and transportation of this material by gravity currents may have been generated by shaking of the sea bottom.

4. *DeGeer Stability and the DeGeer Marine Stage* (Fig. 10). Hubbard uplift was over before the ice uncovered Boston. Then, while the ice border melted back 140 miles (230 km) to Woodsville-Littleton in northern New Hampshire, there was complete stability of the crust. As the ice retreated northwestward across depressed New England, the second marine inundation, or DeGeer stage, of the glacial sea overspread coastal lowlands of eastern Massachusetts, New Hampshire, Maine, and New Brunswick, penetrating to the foothills of the White Mountains, and the base of Mount Katahdin. Melting back of the glacier face standing in this ice pack–covered sea left trails of gravels at the submerged mouths of meltwater tunnels. These deep-water deposits are today some of the most extensive esker systems in the world.[17] At places where they were built up to form proglacial deltas, like the "Promenades" at Portland, Maine, they record the water level with their flat tops.

As a result of later Whittlesey uplift the DeGeer marine limit water plane is tilted so that it projects above present sea level along a zero isobase extending from Cohasset, Massachusetts,[18] near Boston, to Yarmouth, Nova Scotia, from which line it extends inland in a northwesterly direction to elevations exceeding 600 feet (183 m) in central Maine. Iceward from the Whittlesey Hinge Line the earth's crust of DeGeer time was depressed toward the ice, but regions well beyond the ice

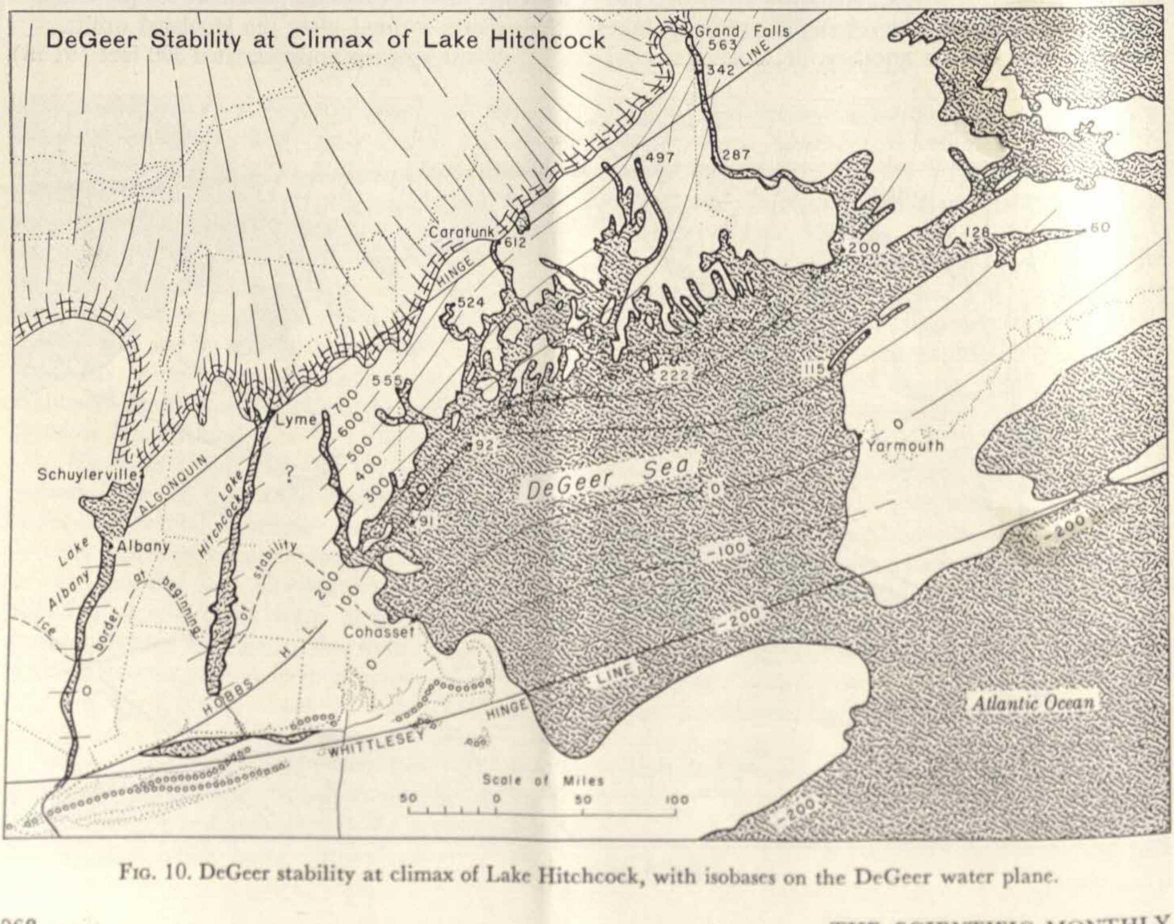


FIG. 10. DeGeer stability at climax of Lake Hitchcock, with isobases on the DeGeer water plane.

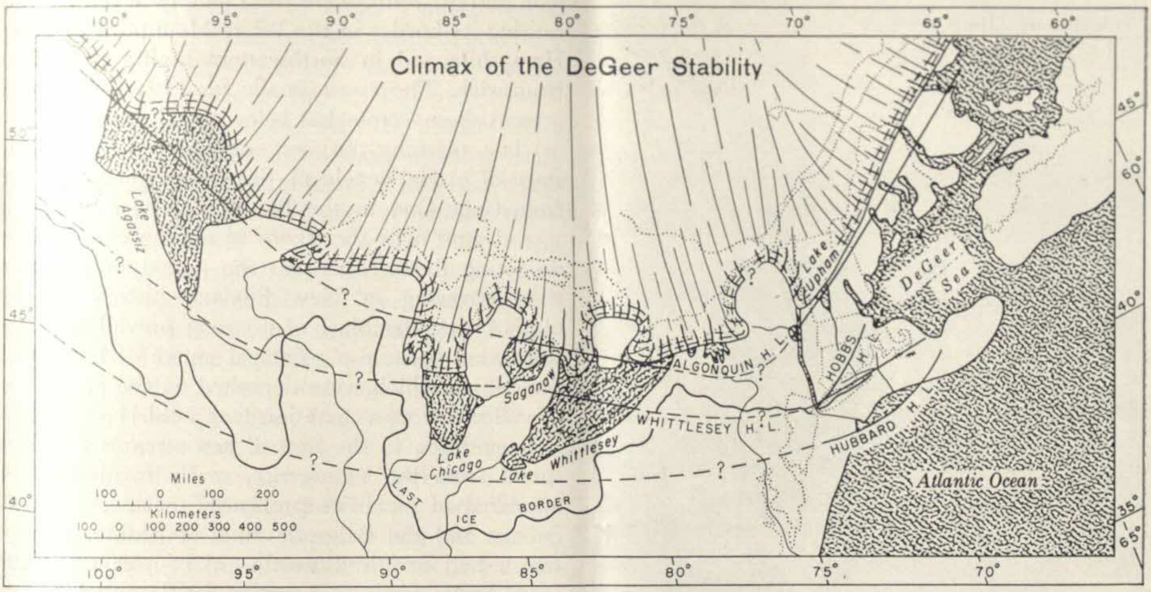


FIG. 11. Climax of the DeGeer stability.

border were more emergent than now, because of previous Hubbard upwarp and the lower sea level. The continental shelf and the fishing banks formed a belt of elevated land enclosing the DeGeer Sea in the Gulf of Maine, and along this land bridge the pine barren flora of New Jersey migrated to Nantucket and beyond.[19] Cape Ann in Massachusetts, highlands in coastal parts of Maine, and all of Nova Scotia were then islands. Melting back of the ice border in the Connecticut Valley allowed Lake Hitchcock to expand from Tariffville and Hartford, Connecticut, to Lyme, New Hampshire, 160 miles (257 km) north of the drift blockade that controlled the lake near Middletown, Connecticut. Lake Hitchcock varves record 4000 years of remarkably stable crustal conditions that followed the uncovering of Hartford.

DeGeer stability coincides in time with Lake Albany in the Hudson Valley, a fresh-water body graded to the DeGeer stage of sea level via the submerged Hudson Channel. It also correlates with Lakes Maumee and Whittlesey in the Glacial Great Lakes, and an ice recession of 250 miles (402 km) in the Middle West during the Herman stage of Lake Agassiz (Fig. 11).[20] Mastodon remains in half a dozen localities in New England are referable to DeGeer time.

5. *Whittlesey Uplift* (Fig. 12). The earliest Glacial Great Lakes came into being following Hubbard uplift, or not until after the ice border had melted back 150–200 miles (229–322 km) from the terminal moraines in southern Ohio. As ice in the Erie basin melted back from its position controlling Lake Whittlesey, an uplift began which was the first to leave a record in the Glacial Great Lakes, although it is second to the Hubbard in the history of New England.[21] The area uplifted was circumscribed by the Whittlesey Hinge Line, which embraces Lake Agassiz, extends eastward across the Michigan and Erie basins, and passes south of New England in the vicinity of New York City and Long Island Sound. Iceward of this line and including much of the region tilted in Hubbard uplift, the land began to rise as ice retreated from moraines near Grand Falls, New Brunswick, at Littleton and Woodsville, New Hampshire,[22] and the Port Huron–Alden morainal system of the Great Lakes.

In the Glacial Great Lakes the recorded rise of land began on the Whittlesey Hinge Line, but the boundary of movement gradually shifted iceward, until at the conclusion of the uplift it had reached the Algonquin Hinge Line. Earthquakes on the Hobbs Hinge Line in Connecticut, which forms a great crustal crack branching off from the Whittlesey Hinge Line, were forerunners of the movement in southern New England; and by disrupting a till blockade which had endured at the outlet of Glacial Lake Hitchcock for 4000 years, suddenly drained the lake when the ice front stood at Lyme, New Hampshire, and gave birth to 90-foot-lower Lake Upham, and the postglacial Connecticut River.[23] Fault dislocations in the DeGeer water plane, associated with Whittlesey upwarping, ap-

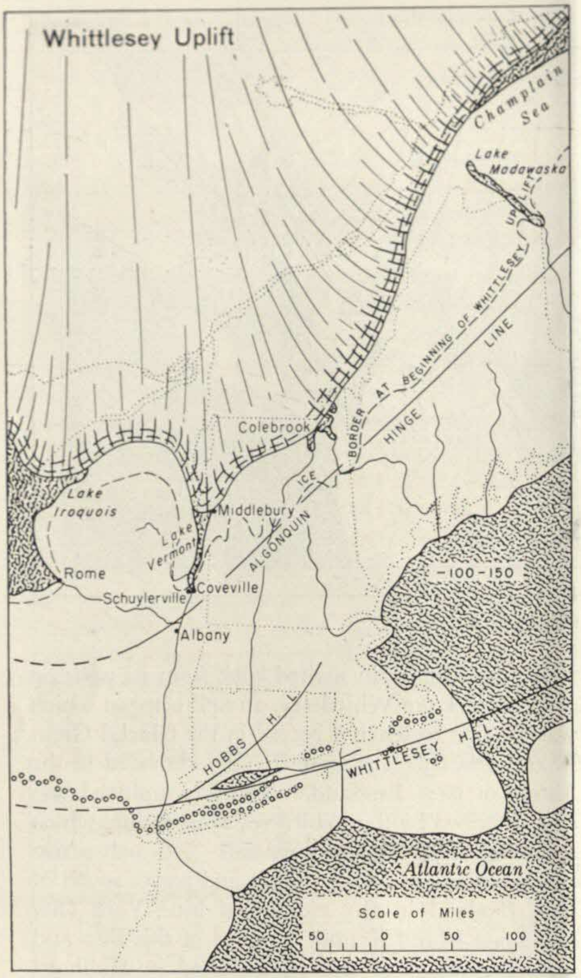


FIG. 12. Whittlesey uplift.

pear along northerly extensions of the Hobbs Hinge Line in southern New Hampshire, and near Portland, Maine, bordering Casco Bay.[24]

Subsiding stages of the DeGeer Sea in Maine, unlike the previous DeGeer upper marine limit, are generally fossiliferous, a fact which may indicate increased salinity as rising sea level inundated Georges Bank and other parts of the cuesta east of the Gulf of Maine. As land rose, rivers were extended to the receding shore. Many, including the Merrimac, Androscoggin,** Kennebec,[25] Penobscot, and St. John, were for a long time fed by the retiring ice border in the White Mountains of New Hampshire and in northwestern Maine and New Brunswick. The coast finally lay eastward of the present shore, somewhat below the 17-fathom (30-m) line, marking the next ensuing, or Champlain, stage of glacial sea level. Sea floors were exposed from Cape Cod to the Bay of Fundy, and rivers spread sand over the plains of marine clay before dissecting them. This was the period of greatest river terracing in New England history. River bluffs facing the blast of a strong prevailing westerly wind became a principal source of loess and dune sand, which were deposited on the east sides of valleys before vegetation took hold.††

In addition to the host of new streams that resulted from the emergence, newly formed lakes appeared in localities previously marine, such as Sebago and the Belgrade lakes of Maine. Rivers which had acquired northward courses resulting from northward drainages of ice-dammed lakes, flowed increasingly slowly as tilting decreased their gradients. Some turned back into lakes, which have since become aggraded with alluvium and swamp deposits that make possible a minimum slope for flow.‡‡

In the Hudson Valley, Lake Albany was completely drained by Whittlesey tilting, and replaced by a powerful Hudson River, ice-fed from Lake Iroquois in the Ontario basin, and from the Coveville stage of Lake Vermont in the Champlain basin.[27] Hudson Channel on the continental shelf, through which the river carried away immense quantities of Albany clay, was not altered by the uplift as it lay south of the Whittlesey Hinge Line passing through New York City. Submarine charts show the lower end of this channel terminating in deltalike deposits at a depth of 30–40 fathoms (55–73 m), which appear to be penetrated by the head of the Hudson Submarine Canyon. Accordingly the canyon may be no older than Whittlesey uplift.

** Androscoggin River wandered across the coastal belt of Maine and acquired a wholly new postglacial course. After bypassing the Salton basin of Androscoggin Lake and contributing sand to the "Desert of Maine" in Freeport, the Androscoggin advanced into the structural valley of the Kennebec River at Brunswick, damming it with a delta before slipping northward off the dam to join the Kennebec in Merrymeeting Bay. From this point the confluent rivers escaped seaward across a rock sill to reach a more easterly structural valley which leads to the city of Bath.

†† Charles River dissected its DeGeer marine limit delta at Watertown, Massachusetts, and advanced across the emergent clay plains of the Boston basin to points east of the present harbor, spreading sheets of sand and gravel which are not to be confused with DeGeer deltas and outwash of earlier date. This material the coastal winds worked into dunes and ventifacts that are encountered in excavations below present sea level in the Boston Back Bay.[26]

‡‡ Streams retarded by Whittlesey uplift include the Wallkill in New York, the lower Farmington in Connecticut, the Quabaug, Neponset, Charles, Sudbury, Concord, and Shawsheen in Massachusetts, the Contoocook in New Hampshire, and the Messalonskee in Maine.

At least 2000 varves of Lake Upham record connections of the head of the Connecticut Valley with ice as the lake was tilted,[28] and before the ice cap became restricted to the St. Lawrence watershed. Whittlesey upwarping split the beaches along the [illegible]0-mile length of Glacial Lake Madawaska in headwaters of the St. John River,[29] and may be recorded in the earliest evidences of marine penetration of the lower St. Lawrence Valley. But Whittlesey uplift ended, and stability followed as the ice uncovered Middlebury, Vermont, long before marine waters entered the Champlain basin.

At the end of the uplift the greater part of New England, or that lying between the Whittlesey and the Algonquin hinge lines, had been raised to the position it has today, leaving the DeGeer marine limit plane tilted upward to the northwest about 4 feet per mile. Tilt rates are less in Nova Scotia and New Brunswick, but are 5.5 feet per mile in the Hackensack basin of northern New Jersey and southern New York, where a combination of Hubbard and Whittlesey uplifts is recorded.

Migrating plants gained hold on the land as fast as ice or the retreating waters uncovered it. At Colebrook, New Hampshire, in the northern end of the Connecticut Valley, a tundra flora of *Dryas* and heaths with birch, poplar, and willow flourished beyond the ice border, its fossilized leaves preserved in lake clays[30] (Fig. 13). These occurrences of *Dryas,* now extinct in New England, are the first, and so far the only ones discovered in eastern North America, although fossil *Dryas* is common in the glacial deposits of Europe.

FIG. 13. Fossil leaves of *Dryas* flora preserved in clay, from glacial lake beds, Colebrook, New Hampshire.

6. *Champlain Stability and the Champlain Marine Stage* (Fig. 14). Whittlesey uplift was followed by crustal stability coinciding with early stages of Lake Algonquin in the Great Lakes, and with ice recession in the Champlain Valley. Two long-lasting and stable stages of Glacial Lake Vermont developed in succession, followed by a stable stage of glacial marine waters bringing whales and shellfish into the Champlain Valley from the St. Lawrence Valley as the ice border uncovered the northern spurs of the Green Mountains of Vermont. The water plane of this brief earliest stage of the Champlain Sea declines in elevation southwestward today from 523 feet (159 m) at Covey Hill, Quebec, to 331 feet (101 m) at Brockville, Ontario, in the St. Lawrence Valley, beyond which point the sea could not penetrate farther toward the Ontario basin. From Covey Hill the same water plane declines southeastward about 5 feet per mile in the Champlain basin to intersect the present lake surface at 95-feet elevation (28 m) near Whitehall, New York. If projected on southward it reaches a depth of 100 feet (30 m) below present sea level, where it is presumed to become horizontal on encountering the Algonquin Hinge Line at Hanover, New Hampshire.[21] According to these calculations, the earliest Champlain marine stage in Vermont correlates with a shoreline submerged at the 17-fathom line along the eastern and southern New England coasts, as elsewhere in the world where not deformed by crustal movements.

Probably the best evidence for the Champlain stability in New England is furnished by the three parallel and equally tilted water planes (two lacustrine and one marine) in the Champlain basin of northwestern Vermont.[31] Champlain stability coincided with the uncovering of the St. Lawrence trough at a time when the depths of water were so great that the only shores of the period are now high on the south slopes of the St. Lawrence Valley, those to the north having been faces of the retiring ice. Eventually, when land was uncovered on the Laurentian side of the St. Lawrence, the stability was over, and active Algonquin uplift was in progress. One of the few regions where the Champlain water plane can be traced in a direction of maximum tilt is in the basin of Lake Champlain, where the trend of the isobases is

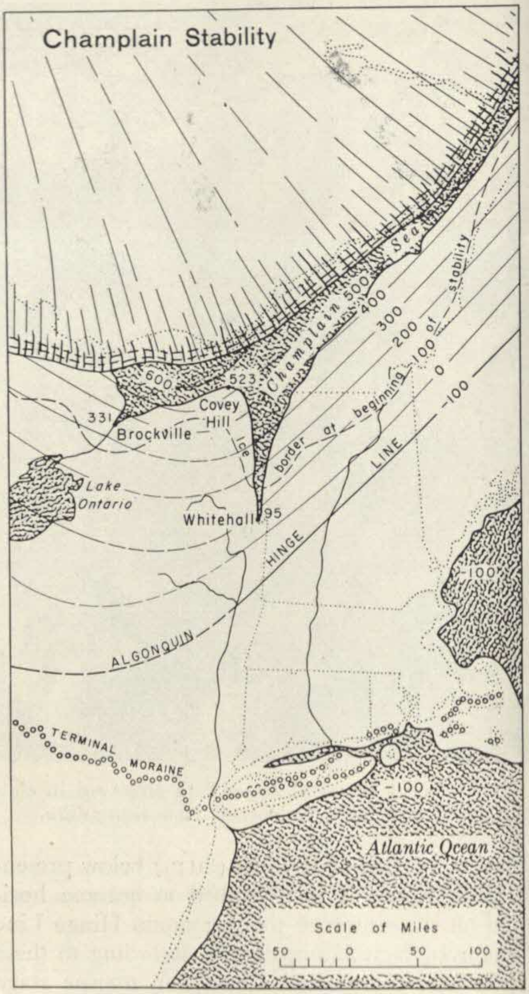


Fig. 14. Champlain stability, with isobases on the Champlain water plane.

across the beach lines, rather than parallel to them as in the lower St. Lawrence Valley; and where elevations on both sides of the basin can be compared.

7. *Algonquin Uplift* (upwarp included in post-Champlain isobases, Fig. 14). Following the Champlain stability, upward movements of the crust began north of the Algonquin Hinge Line, and have continued intermittently on this boundary line to present time. The Algonquin Hinge Line is now as accurately mapped in New England as it had been earlier in the Middle West. It roughly parallels Appalachian structure, in contrast to a southeasterly direction across the Great Lakes. Location of the junction of the two parts of the line in New York state, and its nature, are as yet undetermined.

Shorelines iceward from the Algonquin Hinge Line, if formed prior to Algonquin uplift, preserve complete records of tilting that was due to post-Whittlesey upwarping. Such are the Campbell beach of Lake Agassiz, the early (Kirkfield stage) beach of Lake Algonquin, the Iroquois beach in the Ontario basin, the highest Champlain beach in Vermont, and the initial shore of Lake Upham in the Connecticut Valley. These and all later water planes have a spectrumlike convergence toward the Algonquin Hinge Line, which permits three distinct stages of post-Whittlesey land movement to be identified and correlated, from the Middle West to the Province of Quebec and New England.

The Champlain water plane in Vermont and in the lower St. Lawrence Valley is tilted 5 feet per mile. Algonquin upwarp, as here defined, accounts for the uppermost 2.8 feet of this amount, which was accomplished before the stability of Ottawa time. Algonquin movement was in progress as the Laurentian side of the Ottawa and St. Lawrence lowlands were relieved of their ice cover. A weak upper marine limit associated with massive ice-marginal deltas and constantly degrading tributary stream deltas characterizes the subsiding stages of the Champlain Sea. Shore elevations of these metachronous water levels rise to 620 feet (189 m) at Lake St. John in the Champlain estuary of the Saguenay Valley, 690 feet (210 m) on Kingsmere Mountain at Ottawa, and 551 feet (168 m) at Mattawa, Ontario, where the North Bay outlet river from Lake Algonquin built a delta into the Ottawa Valley estuary. Marine fossils attributable to late stages of the Champlain submergence are plentiful as far inland as Pembroke, Ontario.

Algonquin uplift took place while the ice border was associated with middle and late stages of Lake Algonquin. In northern parts of Lake Agassiz and the Glacial Great Lakes this and succeeding uplifts have produced higher tilt rates than the earlier Whittlesey uplift. In New England, only those regions northwest of the Algonquin Hinge Line have been affected by post-Whittlesey movements. The Connecticut Valley northwest of the Algonquin Hinge Line at Hanover, New Hampshire, has been given increased inclination amounting to several feet per mile; and the shoreline of Lake Upham, which is tilted 4 feet per mile south of Hanover, is tilted 9 feet per mile north of Hanover, where it bears the combined record of Whittlesey, Algonquin, and Hudsonian movements. Southern and eastern New England outside the Algonquin Hinge Line has remained stable, or an "area of horizontality," since the Whittlesey uplift.

8. *Ottawa Stability, and the Ottawa Marine*

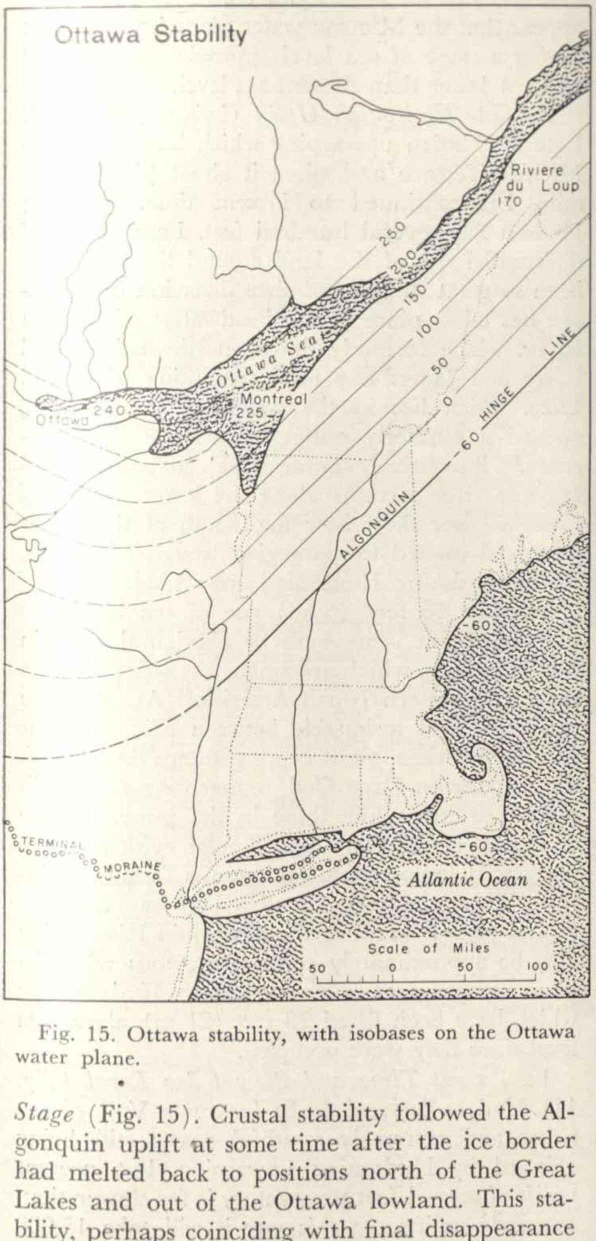


Fig. 15. Ottawa stability, with isobases on the Ottawa water plane.

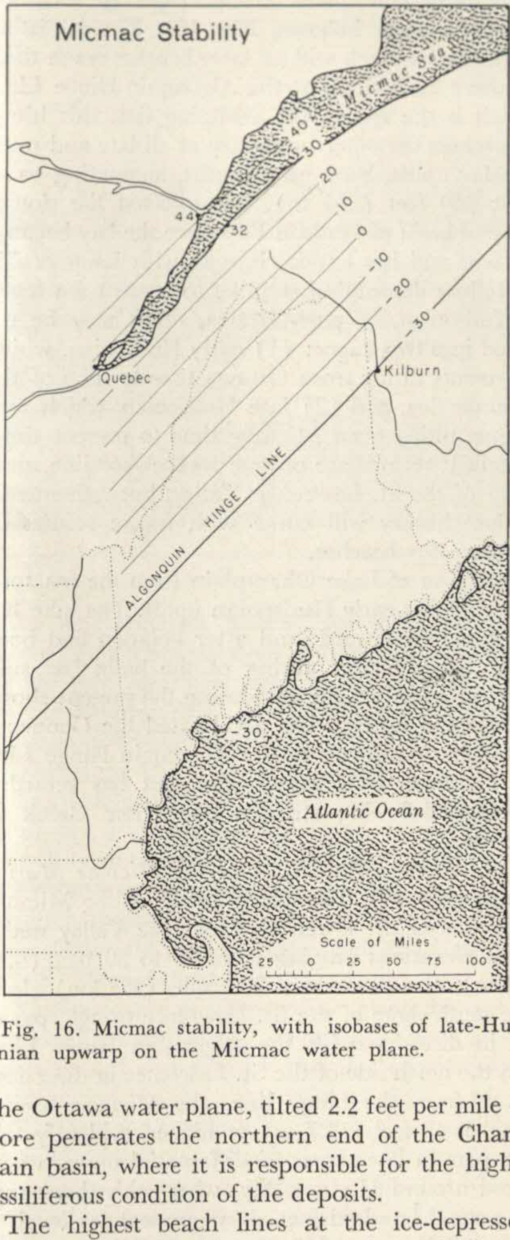


Fig. 16. Micmac stability, with isobases of late-Hudsonian upwarp on the Micmac water plane.

Stage (Fig. 15). Crustal stability followed the Algonquin uplift at some time after the ice border had melted back to positions north of the Great Lakes and out of the Ottawa lowland. This stability, perhaps coinciding with final disappearance of the ice cap in Canada, is marked by the Nipissing stage of the Great Lakes, and its marine equivalent, the Ottawa Sea, in the Ottawa, St. Lawrence, and Champlain valleys. These lake and marine stages had no association with ice. They have strong shorelines—that of the Ottawa being by far the best developed shore in the Ottawa Valley[32]—and not infrequently cut in rock along the lower St. Lawrence. It is several hundred feet below the Champlain marine limit and has elevations of 240 feet (73 m) at Ottawa, 225 feet (69 m) at Montreal, and 170 feet (52 m) at Rivière du Loup. The Ottawa water plane, tilted 2.2 feet per mile or more penetrates the northern end of the Champlain basin, where it is responsible for the highly fossiliferous condition of the deposits.

The highest beach lines at the ice-depressed center of the continent around Hudson and James bays may correlate approximately with Ottawa time. Marine submergence in James Bay reaches to within 200 miles (322 km) of the north side of Lake Superior, but scarcity of accurate measurements restricts an estimate of the highest marine evidence to the occurrence of land-locked seals in Seal Lakes of Western Ungava at about 800 feet elevation (244 m).[33] On the New England coast, as elsewhere outside the Algonquin Hinge Line, the shore of Ottawa time is submerged at approximately the 10-fathom (18.3 m) line.

9. *Early Hudsonian Uplift* (upwarp included in post-Ottawa isobases, Fig. 15). The Nipissing Great Lakes beach and all later beaches reach their southern limit of tilt at the Algonquin Hinge Line, which is the reason for assuming that this hinge line marks the outer boundary of all late and present-day uplift. Very great uplift, amounting to at least 800 feet (244 m), has affected the downwarped basin of Hudson Bay since the bay became ice-free and has left it rimmed with innumerable shorelines descending steplike to present sea level.

Hudsonian, or post-Ottawa, uplift may be divided into two stages: (1) early Hudsonian, which represents tilting from Ottawa time to time of the Micmac Sea, and (2) late Hudsonian, which represents tilting from Micmac time to present time. Excellent records are offered by the shoreline spectrum of the St. Lawrence Valley, but refinements in late history will come with future studies of Hudson Bay beaches.

Isolation of Lake Champlain from the sea took place during early Hudsonian uplift. The lake has an outlet northward, and after isolation had been established, further tilting of the basin has submerged all shorelines antedating the present shore. Hudsonian upwarp has accelerated the Connecticut River northwest of the Algonquin Hinge Line at Hanover, New Hampshire, but has retarded northward-flowing streams like Otter Creek in Vermont.

10. *Micmac Stability, and the Micmac Marine Stage* (Fig. 16). The exceptionally strong Micmac shoreline of the lower St. Lawrence Valley maintains elevations ranging from 20 to 30 feet (6.1–9.1 m) for a distance of 200 miles (322 km) along the south shore of the St. Lawrence trough, parallel in direction with the Algonquin Hinge Line. On the north side of the St. Lawrence in directions away from the hinge line, the Micmac terrace reaches 45 feet (13.7 m) or higher, evidently taking part in late stages of differential upwarp centered around Hudson Bay, where this shore may be several hundred feet above present sea level. It is generally conceded that the Micmac terrace, which the present weak shoreline in the St. Lawrence estuary cannot match in strength, represents combined effects of an important interval of crustal stability, and transgression by rising sea level—both factors giving shore processes opportunity to cut continuous sea cliffs and a broad wave-bench as far inland as the city of Quebec.

In appearance the Micmac shore resembles wave-cut parts of the present New England–Acadian shoreline, of which it may indeed be an early and uplifted phase. Projection of the limited profiles southward to the Algonquin Hinge Line makes it appear that the Micmac water plane began to tilt during a stage of sea level approximately 30 feet (9.1 m) lower than present sea level.

11. *Late Hudsonian Uplift* (isobases, Fig. 16). Late Hudsonian upwarping which has raised the Micmac Terrace and tilted it about ½ foot per mile, has continued to Present time, uplifting Hudson Bay several hundred feet. Late stages of differential rise of the land around the bay have been so great that a complete diversion of drainage has taken place in the headwaters of Ottawa River, which formerly flowed northward to Bell River and James Bay until the tilting of Grand Lake, which lies in the course of the Ottawa, opened a southerly outlet toward the St. Lawrence.[34] Successively lower deltaic deposits grading into river terraces along the lower course of Ottawa River show how the mouth of that river advanced toward the emerging trough of the St. Lawrence during Hudsonian upwarping.

The last 16 feet (5 m) rise of sea level has submerged fish weirs built by aboriginal man in Boston Harbor, which are among the oldest human records in eastern North America.[26] At or above this level, and assignable between Micmac time and present time, giant cypress stumps lie beneath the marshes on Cape Cod, representing warm climatic conditions like those in the present range of cypress in the Carolinas. Similar evidence of a postglacial temperature optimum is found in Europe. These shallowly inundated human and botanical records south of the Algonquin Hinge Line may be approximately contemporaneous with Eskimo campsites at river mouths in Hudson Bay which have been lifted 70 feet (21 m) above sea level since they were occupied.[35]

12. *Present Time and Present Sea Level* (Fig. 17). Hudson Bay and the St. Lawrence Valley have rising and extremely youthful coasts still being affected by Hudsonian upwarping, but present uplift of several feet per century is far slower than the early uplifts. At this rate it will take 15,000 years and an upwarp of 500 feet (152 m), not counteracted by rise of sea level, to drain Hudson Bay.

To what extent present earthquake activity is related to postglacial uplift is debatable. Hudson Bay is not notably seismic, although the St. Lawrence Valley and northern New York state are moderately so. No pattern of quakes follows the Algonquin Hinge Line, but points along the Hobbs Hinge Line in Connecticut and its extension into coastal New Hampshire have been centers of a number of quakes within historic time.[36] Glaciated

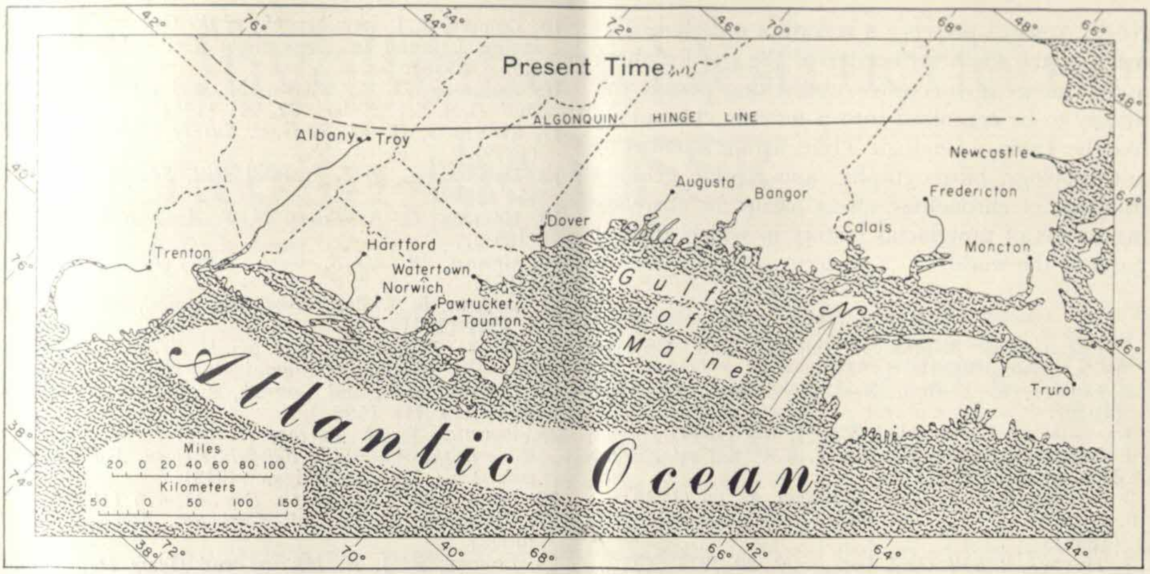


FIG. 17. Present time, marked by present sea level; tidal heads of drowned river valleys as shown.

regions, generally, are not more seismic today than unglaciated regions, and isolated cases like the Ossipee, New Hampshire, quake of 1940, originating at a depth of 30 miles (48 km), seem to relate to no recognized crustal patterns of the past or the present.

Most of New England is located on the stable side of the Algonquin Hinge Line, and the coast has suffered transgression by rising sea level of somewhat more than 100 feet (30 m) since the conclusion of Whittlesey uplift. This has driven the shore inland, beginning at the submerged coast of Champlain time, promoting the construction of offshore bars, stimulating the upward growth of tidal marshes, and drowning the channels and deltas of New England rivers which are no longer aggressively transporting bed load to the sea. Tidewater reaches inland to Albany and Troy, New York, a distance of 130 miles (209 km) along a drowned channel cut by the Hudson River in clay beds when the land stood relatively higher in the Whittlesey uplift. Tidal conditions affect the mouths of all the principal rivers of New England and New Brunswick.

Very old human cultures associated with glacial sea levels are now either submerged below present sea level, or accessible only where there are raised shores. For example, in northwestern Vermont a paleo-Indian culture, represented by an abundance of chipped stone artifacts at the elevation of the highest Champlain marine shoreline,[37] may prove to establish the presence of Man in New England in the Champlain stability. On the other hand, aboriginal occupation sites along the coastline of today must be considered Recent, for the building of Stone Age kitchen middens in New England ceased barely three centuries ago. The Titicut campsite on the Taunton River in Massachusetts, is located far inland among esker deposits of the Leverett marine stage, but precisely at present head of tide, so must date back no farther than a time when rising sea level became stabilized at the present New England-Acadian shoreline. Even so, the present shore may be many centuries old, judging by the great strength of such features as Sandy Hook, New Jersey, the claw of Cape Cod, and Nantasket Beach, Massachusetts.

Since evidence of transgression along the eastern seaboard is matched by similar evidence on all the continents, it is presumably due to continued world deglaciation. In the year 1928 tide gages of the U.S. Coast and Geodetic Survey in the Pacific and on both sides of the Atlantic began to record a rise of sea level averaging 0.02 foot (0.61 cm) per year, which has amounted to 0.4 foot (12.19 cm) in 20 years.[38] This rate of rise more than counteracts the effects of present upwarping, and if continued could raise sea level 20 feet (6 m) in the next 1000 years.

Conclusions

Glacial history has never been interpreted successfully from any other point of view than normal glacial retreat. Nor has progress been attained in reconstruction of time relationships where there is no workable method for making correlations between distant regions. Glacial deposits of eastern

North America preserve a record of orderly retirement of the southern border of the last ice sheet and evidence of successive uplifts that permit the history to be organized into a tectonic chronology like the Table of Geologic Time. To the sciences of anthropology, biogeography, and marine geology this type of chronology offers means for correlating events of postglacial history in widely distant parts of the world.

References

1. *Encyclopædia Britannica*. Chicago: Univ. Chicago Press, "Glacial Epoch" (1947).
2. LOUGEE, R. J. *Bull. Geol. Soc. Amer.,* **49**, 1891 (1938).
3. ———. *J. Geomorphol.* **3**, (3), 189 (1940).
4. LEVERETT, F., and TAYLOR, F. B. *U. S. Geol. Survey Monograph* 53 (1915).
5. LOUGEE, R. J. *Pan-Am. Geologist,* **49**, 65 (1938).
6. GILBERT, G. K. *U.S. Geol. Survey Monograph* 1 (1890).
7. HYYPPÄ, E. *Bull. Geol. Soc. Amer.,* **50**, 1913 (1939).
8. LOUGEE, R. J. *Ibid.,* (12), pt. 2, 1919.
9. LOUGEE, R. J., and VANDER PYL, A. W. *Sci. Monthly,* **73**, 275 (1951).
10. ANTEVS, E. *The Last Glaciation.* New York: Am. Geograph. Soc., Research Ser. No. 17 (1928).
11. HUBBARD, G. D., and ROCKWOOD, R. C. *Bull. Geol. Soc. Amer.,* **53**, 227 (1942).
12. DELLIQUADRI, L. *Glacial Lake Arthur.* Master's thesis, Clark Univ. (1953).
13. CESTRE, G. *Glacial Water Levels in the Upper Ware River Valley, Massachusetts.* Doctorat d'Université thesis, Univ. Paris (in preparation).
14. HOKANS, D. H. *Glacial Water Levels in the Housatonic-Naugatuck River Valleys of Western Connecticut.* Master's thesis, Clark Univ. (1952).
15. LOUGEE, R. J. *Physiography of the Quinnipiac-Farmington Lowland in Connecticut.* Colby Monographs, No. 7 (1938).
16. ERICKSON, D. B., EWING, M., and HEEZEN, B. C. *Bull. Geol. Soc. Amer.,* **62**, 961 (1951).
17. STONE, G. H. *U. S. Geol. Survey Monograph* 34, (1899).
18. DEGEER, G. *Proc. Natural Hist. Soc. Boston,* **25**, 454 (1892).
19. HOLLICK, C. A. *Trans. N. Y. Acad. Sci.,* 12, 189 (1893).
20. UPHAM, W. *U. S. Geol. Survey Monograph* 25, (1895).
21. LOUGEE, R. J. *Bull. Geol. Soc. Amer.,* 52, (12) pt. 2, 1921 (1941).
22. ———. *Proc. Natl. Acad. Sci. U. S.* **21**, 36 (1935).
23. ———. *Dartmouth Alumni Mag.* **27**, 5 (1935).
24. CROSBY, I. B., and LOUGEE, R. J. *Bull. Geol. Soc. Amer.,* **45**, 441 (1934).
25. PERKINS, F. M. *Terrace History of the Kennebec River in the Waterville–Fairfield Region, Maine.* Master's thesis, Univ. Michigan (1943).
26. LOUGEE, R. J. *Geograph. Rev.,* **41**, 475 (1951).
27. CHADWICK, G. H. *Bull. Geol. Soc. Amer.,* 39, 901 (1928).
28. LOUGEE, R. J. *N. H. Fish and Game Dept., Biol. Survey Connecticut Watershed Rept. 4,* 131 (1939).
29. KIEWIET DE JONGE, E. J. C. *Glacial Water Levels in the St. John River Valley.* Ph.D. diss., Clark Univ. (1951).
30. HOLLICK, A. *Brittonia,* **1**, 35 (1931).
31. CHAPMAN, D. H. *Am. J. Sci.* 5th ser., **34**, 89 (1937).
32. MACKAY, J. R. *Rev. can. geograph.,* **3**, 53 (1949).
33. DOUTT, J. K. *Ann. Carnegie Museum, Pittsburgh, Pa.,* **29**, 61 (1942).
34. BELL, R. *Bull. Geol. Soc. Amer.,* **8**, 241 (1897).
35. BELL, R. *Am. J. Sci.,* 4th ser., **1**, 219 (1896).
36. HOBBS, W. H. *Earthquakes, An Introduction to Seismic Geology.* New York: Appleton (1907).
37. RITCHIE, W. A. *Am. Antiquity,* **18**, (3), 249 (1953).
38. MARMER, H. A. *Geograph. Rev.,* **38**, 652 (1948).

The Involuntary Destruction of Science in the USSR

CONWAY ZIRKLE

Dr. Zirkle is professor of botany at the University of Pennsylvania. He is the author of The Beginnings of Plant Hybridization *and* Death of a Science in Russia. *He was born in Richmond, Virginia, and received a B.S. and an M.S. degree from the University of Virginia in 1921. After study abroad at the University of London and at Geneva, he returned to America and received his Ph.D. degree from Johns Hopkins University in 1925. He was a National Research Council Fellow before he joined the faculty of the University of Pennsylvania in 1930. The present article is based on his address as retiring vice president of Section L of the AAAS at St. Louis on December 31, 1952.*

IN the autumn of 1948 the newspapers of the world described the famous meeting of the Lenin Academy of Agriculture, the meeting in which genetics was attacked most viciously by a group which was seizing control of agricultural research in the Soviet Union. Throughout the sessions, which lasted from July 31 to August 7, the almost uninterrupted denunciations of genetics continued until, on this latter date, T. D. Lyensko, President of the Academy, announced that the Central Committee of the Communist Party had approved the line taken by the attackers. The members of the Academy received the ukase with an enthusiasm, which was described by *Pravda* (Aug. 8, 1948) as follows:

> This communication by the President aroused general enthusiasm in the members of the session. As if moved by a single impulse, all of those present rose from their seats and started a stormy, prolonged ovation in honor of the Central Committee of the Lenin-Stalin Party, in honor of the wise leader and teacher of the Soviet people, the greatest scientist of our era, Comrade Stalin.

The science of genetics was promptly outlawed and five Russian scientists found it expedient to recant.

To the enlightened citizens of the world, this procedure was as surprising as it was preposterous. The question which naturally arose was: "Why? Why should a nation which had expended so much of its revenues in supporting and developing a science, decide suddenly to destroy what it had been promoting and cherishing?" The destruction of a science—of any science—just did not make sense; it seemed particularly out of place in a nation governed by a philosophy which held that the future welfare of society depended in large part upon the advancement of science. The absurdity of the act was really so patent that its very incredibility was put to use by the defenders of the Soviets. They seized upon its senselessness as evidence that it never occurred. For example, Howard Fast,[1] in commenting on an article in the *Saturday Review of Literature* by H. J. Muller, wrote in his column in the *Daily Worker*:

> . . . It really makes one wonder what those Russians are up to and who profits through their cutting their own throats; but such a frenzy of unrestrained name-calling hardly refutes the calm and reasoned arguments put forth by Trofim Lysenko. . . .

To many non-scientists it seemed that there must really be "something" in the newer Russian stand after all. The higher authorities in Russia certainly could not be as foolish as they appeared; they could not be dismissed as morons, for all our evidence indicated the contrary. To most of us the Russians seemed to be both subtle and brilliant, for they had clearly been able to out-think our own political leaders far too often for our own comfort. What could they really be up to? How puzzling the situation was, is shown by Harrison Salisbury[2] who wrote (*New York Times* of February 27, 1949), somewhat naively:

> The American Nobel Prize winning geneticist, Professor Herman J. Muller, has charged that Lysenko's victory means the destruction of science in Russia. This seems to be an extreme view. Lysenko indicates plainly enough that the Central Committee is not nearly so much interested in theories as in results. A capable agronomist is not liable to be purged for creating a new hardy breed of spring wheat, regardless of whether he follows Morgan-Mendelian theories or those of Michurin.

Those who are equipped technically to evaluate the doctrines which displaced genetics, however, recognized at once that they were not new, but

were only a resurgence of some archaic errors which had been abandoned during the past century. Anyone acquainted with the history of biology would have no difficulty in dating them accurately. The origin of some could even be traced back to the Greeks, while others had come into prominence in the eighteenth century and had remained respectable up to seventy-five or eighty years ago. Obviously, this revival of past mistakes could not possibly represent an advance in science, and the reaction of the scientists of the free world was just what we would expect. They refused to accept the validity of the Russian claims in the absence of good, supporting, scientific data. We should mention that this was not, as some thought, an instance of a conservative and reactionary group refusing to acknowledge scientific progress. The basic question, however, still persisted. Why had the Russians destroyed a science?

A number of answers were given. Most of them were reasonable, and some were certainly valid in part. The whole picture, however, remained incomplete and obscure. Even when we combine and integrate the various answers, something still seems to be lacking. One gets the impression that something has gone wrong in the Communist world, that something has gotten out of control, that events are not proceeding according to plan. If this impression is justified, it is obviously important that we try to find out just what is happening. In this paper, our purpose is to explore briefly the whole affair, to examine the data which have become available and to try to fit them into a consistent whole.

During the twenty years which preceded the outlawing of genetics, the science had been attacked periodically by the Marxian doctrinaires and, as early as 1926, attempts had been made to foster a set of competing doctrines. In 1936 the attacks achieved sufficient prominence to reach the attention of the western geneticists, who naturally wanted to know why genetics was under fire. C. D. Darlington[3] concluded that:

> The rise of Hitler to power gave new life to the forces working against western science in general and against genetics in particular. Hitler's doctrine was founded on giving a distorted predominance to a distorted genetics. His theory assumed the permanent and unconditional, and homogeneous, genetic superiority of a particular group of people, those speaking his own language. The easy retort was obviously to repudiate genetics and put in its place a genuine Russian, proletarian, and if possible Marxist, science. For this purpose very little research was necessary: the classical personalities and achievements of Timiryazev and Michurin were there ready to hand. All that was needed was to discover a new prophet of Marxist genetics or Soviet Darwinism. The prophet was found in Trofim Dennissovitch Lysenko.
>
> . . . .
>
> A government which relies on the absence of inborn class and race differences in man as a basis of its political theory was naturally unhappy about a science of genetics which relies on the presence of such differences among plants and animals as the basis of evolution and of crop and stock improvement.

Darlington's interpretation is certainly valid, and we have in addition many other bits of evidence which substantiate his views. For example, Muller[4] stated that the Genetics Congress scheduled for 1937 was canceled because some geneticists insisted on their right to give papers on human heredity.

> This step [the cancelling of the congress] was taken only after the Party had first toyed with the idea of allowing it to be held with the omission of all papers on evolution and on human genetics in spite of the fact that many foreign geneticists had intended in such papers to attack the Nazi racist doctrine!

We also have evidence from other sources that human genetics was a dangerous subject in Russia. The attack made on the Medico-Genetic Institute of Moscow in 1936 is one example. We find additional confirmation in the acts of the Russian censors, in that the reports of the genetic debates of December 1936 were published (in the Russian edition) with all references to man expurgated, and the book itself was soon banned. Furthermore, Newman[5] stated that work on identical twins at the Maxim Gorky Institute was suddenly terminated in 1939 and nothing further was heard of the psychologists in charge of the work. To quote Muller[6] further:

> What causes the Communist officials to push Lysenkoism so strongly? To me, the answer is obvious: it is the type of mind that sees things as only black and white, yes and no, and so cannot admit the importance of *both* heredity and environment. Believing that it has found the complete answer to all the world's ills, through its particular way of manipulating environment, the Communist Party regards as a menace any concept that does not fit patly into its scheme for mankind. The genes do not fit into that concept, in its opinion, hence the existence of the genes must be denied.

It is extremely significant, of course, that genetics was the first science to be attacked. It may be that these attacks, together with their surprising success, were the first indications that science itself was vulnerable. The Marxian obscurantists must certainly have been encouraged with the outcome. As we would expect, sooner or later other sciences would also be assailed and some might be destroyed, even though they were not directly in conflict with Marxian dogma. The anti-scientific virus started its

infection in genetics, however, in a field where science conflicted with the Communist faith.

Michael Polanyi[7] offered an alternative explanation to those we have cited. He stated that, when the final authorities who control science are not scientists themselves, sooner or later quacks will flourish and ultimately dominate the field. Even with the very best intentions, those who are not scientists cannot decide scientific questions and cannot, in the last analysis, even choose between scientists and charlatans. It is only when science is free and autonomous, when enlightened scientific opinion is the court of last appeal, that imposters can be exposed as they arise. Otherwise quackery will flourish, for the plausible oversimplifications of quackery have always had a great popular appeal. Concerning the genetics controversy, Polanyi stated:

> The demonstration given here of the corruption of a branch of science caused by placing its pursuit under the direction of the State, is, I think, complete. The more so—I wish to repeat—as there is no doubt at all of the unwavering desire of the Soviet Government to advance the progress of science. It has spent large sums on laboratories, on equipment, and on personnel. Yet these subsidies, we have seen, benefited science only so long as they flowed into channels controlled by independent scientific opinion, whereas as soon as their allocation was accompanied by attempts at establishing governmental direction they exercised a violently destructive influence.

There are undoubtedly a number of other contributory factors in the Russian reaction against genetics. Genetics denies the inheritance of acquired characters, and this type of inheritance promises so much so easily that it has always been a favorite of those who want to make over mankind in a hurry. Any group of scientists who denied such inheritance was bound to be unpopular with those who held an escapist belief in the possibility of a quick reconstruction of man and his works.

We have by no means explored all the possible causes of the destruction of genetics. Soviet biology seems to be derived from a canon which included Marx, Engels, and Lenin, and those they endorsed. Lenin had blessed a nurseryman, an importer and breeder of plants, named I. V. Michurin (1855–1935), who thus achieved a high rank in the Communist Apostolic Succession. Michurin was always a staunch Lamarckian, and in his dotage he became violently anti-Mendelian. His Bolshevik record and Lenin's approbation had raised him beyond the criticism of mortals, so Michurinism is never evaluated adversely in Russia and his doctrines are regularly greeted with lavish praise. Even N. I. Vavilov, who prior to 1939 directed Russian research in genetics, dared not contradict any of Michurin's pronouncements and, as the lesser geneticists also wanted to live, any honest discussion of the basic problems of genetics was out of the question in the scientific academies of Russia. Consequently, in the "socialist competition" for position and power in the biological field the type of winner could be predicted easily. No honest scientist could ever be as good a Michurinist as those who fit their convictions to the Party line.

Any critical evaluation of the explanations we have thus far given must acknowledge that, although they may be valid as far as they go, they are certainly incomplete. They do not account for all that has happened to science in the Iron Curtain countries, and they tell us almost nothing about the growing hostility to science and to scientific standards. When we remember that such disciplines as soil science, pathology, physiology, and statistics also have been attacked and that they have been injured to such a degree that their recovery is doubtful, we must admit that more is involved in the spreading debacle than the basic conflict between modern genetics and the biology of Marx and Engels. If we are to understand what is really happening to science in Russia, we must penetrate much farther than we have into the Soviet way of life and we must not overlook the possibility that the Communists themselves do not know all that is happening and do not really understand what they are doing.

In spite of the fact that a number of our own scientists are acquainted intimately with Russian science and have studied intensely the Russian attacks on science, they have not succeeded in finding explanations of the affairs which are both complete and reasonable. This in itself suggests the existence of irrational factors, and of actions and decisions which are basically illogical. There is even evidence that the affair has grown completely out of control and that the Communists themselves are helpless and cannot stop what they have started. We now have reason to believe that the Communists are beginning at last to realize that they have injured their own strength and security but do not know how to remedy the situation.

When alogical factors begin to assume paramount importance in a situation which should be handled reasonably, we can always blame the state of affairs on historical accidents. Obviously the Russian destruction of genetics is such an accident, but to acknowledge this does not advance our un-

derstanding until we analyze and identify its causes. We must be able to recognize the significance of the important incidents in the history of the Soviets which, cumulatively, have produced an intellectual climate in which some sciences do not thrive and others actually wither and die. To list these incidents we will have to go back to the early days of Communism, back even to the Party line in biology as it was set by Marx and Engels, because the first liquidation of Russian scientists was a result of the resurrection of this line.

The hostility of Marx and Engels to scientific objectivity is still not realized generally; in fact, it has been beautifully camouflaged. Both Marx and Engels claimed to revere science and to be scientific themselves in their contributions to economics and sociology. They praised science consistently and called it the hope of mankind, but even so they denounced and misrepresented all scientific theories which conflicted in any way with their Communistic postulates. This action of the founders of Communism is only now coming to light. That Marx and Engels were both thoroughly hostile to the Darwinian concept of evolution through natural selection was concealed because they accepted the *fact* of evolution enthusiastically and they both praised Darwin highly. (Marx even wanted to dedicate *Das Kapital* to Darwin.) Moreover, they defended evolution at a time when the more conservative religious groups were attacking it. However, since the publication of the Marx-Engels correspondence in 1930, we can learn at last just what was acceptable to them in biological theory. We know now that their "line" both in evolution and in heredity is in conflict with scientific biology. Lysenko himself has pointed this out: "In his time Darwin was unable to free himself from the theoretical errors which he committed. These errors were discovered and pointed out by the Marxist classicists."

Although it is possible and even easy for us to explain how that portion of science which conflicts with a powerful orthodoxy can be outlawed, we will have to search further if we are to discover why scientific principles which are politically neutral are also denounced and suppressed. In fact, we will have to identify the conditions which determine how science develops under a totalitarian regime.

Paradoxically enough, the first signs of a drop in the quality of Russian scientists are to be found in the results of Lenin's attempts to foster science. According to his plans, the Communist state was to give science full support and train new scientists in wholesale lots. The dangers of such a procedure are obvious, even if all possible safeguards are observed. We have evidence, however, that the Communists did not recognize the dangers inherent in their program and used no safeguards whatsoever. They even made for themselves a number of unnecessary hazards in that they chose new scientists from politically "reliable" classes such as the laborers and the peasants. Of course these two classes could produce a number of good scientists, which they did. But Russia needed many thousands of first-rate scientists and, as we have learned from a number of refugees, many of the children of the intelligentsia and the professional classes were denied a university education and hence could not become professional scientists. Thus, in spite of her needs, Russia reduced the number of scientists she could obtain from just those classes which produce the majority of the scientists of the western world. Quantity Russia had, but the average quality was low, so low in fact that large numbers of the prospective scientists could not pass their university examinations.

For this difficulty, the Communists had a perfect collectivistic remedy. Cooperation was the answer. The students were divided into small groups that took their examinations in a body. They pooled their knowledge and, when a group was able to pass its examinations, the individuals who made up the group became scientists. We can now trace to those who passed their examinations cooperatively some of the horrifying examples of ignorance which some Russian scientists display. Thus it was not long before the Russian scientists were, literally, of all types, ranging from excellent to terrible, and the political authorities had a wide choice as to the kinds of scientists they could promote to positions of importance and influence. The political authorities were also uninhibited by any real scientific considerations in making their choices, for conditions in Russia are not such that the politicians prefer scientific standards to their own personal safety. In Russia the truth may be all right in its place, but it should never be allowed to become a nuisance and spoil one's career.

The outcome of the conflict between genetics and Marxian biology taught a direct personal lesson to all of the scientists in the Communist lands. It had become clear that promotion, status, and even safety did not depend upon a devoted attachment to the truth but to guessing right, to being on the winning side, to telling the political authorities just what the political authorities want to hear. The road to safety was clearly not to be found by a careful and honest interpretation of experimental data but by preserving a rigid

Communist orthodoxy, by endorsing just those doctrines which are compatible with the beliefs of the dominant clique. For example, in the field of linguistics, disaster could be avoided by agreeing "with the mighty discoveries of the greatest scientist of all time, with Stalin." But this is not all. Lesser authorities have also been canonized. Scientific orthodoxy is also set by the published work of Michurin, Lysenko, Pavlov, Williams, etc. A clever scientist thus has many guides to safety. If he is alert he may keep his job and his family will continue to eat.

The good scientists of Russia have learned their lesson, and have learned it well. The way to avoid the humiliating recantations is now clear. By paying close attention to the articles which appear in *Pravda* or *Izvestia* they can discover, without too much trouble, just what is expected of them. Those who have a sense of humor may even get a subtle revenge on the authorities who control their destinies. But in all these happenings there are most serious consequences for the Communist world. In a number of scientific fields her leaders have cut themselves off from the truth and, perhaps fortunately for the free world, they have no means for reestablishing the connections. Once the good scientists have been intimidated or disgusted, all the scientists are reduced to the same level, the level of the charlatans. Even if Stalin himself had wanted to know the truth about Mendel, he could not learn it. He could find no reason to believe what anyone would tell him.

If all the sciences in Russia had sunk to the level of those which have been injured most, the Communist regime could soon be dismissed as an unpleasant episode in the long instalments of human history; its internal weaknesses would soon reduce it to impotence and the world's worry would be over. We must recognize, however, one fact which might prevent the deterioration from spreading and becoming uniform throughout all science. There are fields such as engineering and applied physics where the results of bad science can be discovered immediately. In these fields quackery does not last long. Here, at least, if the political authorities cannot recognize good scientists they can at least discover quacks, and, by a ruthless elimination of the latter, may even maintain relatively high standards. On the other hand, the situation is very different in medicine and agriculture, for in these fields there are none of the simple tests which may allow a layman to reach a sound and discriminating judgment. In spite of quacks and cultists, patients recover and crops grow. In medicine and agriculture only honest scientists can evaluate the current practices. A large part of Russian science, however, falls somewhere between engineering and agriculture in the direct measurability of its practical achievements. In these intermediate sciences we would expect quackery to flourish inversely as the ease of identifying quacks. We certainly cannot judge all Russian science by what happens in one or two fields.

The present status of Russian science, of course, is of tremendous importance; it may even be more important for us to extrapolate the present tendencies and to estimate its future course of development. In December 1951 the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE (Section L) held a symposium entitled *Soviet Science.*,[8] and this symposium has been published. I refer you to this work for as good a picture as we can give of Russian science today. The following brief summary is based mostly on the evidence cited in that work.

1) Genetics has been extirpated in Russia and its place taken by an archaic quackery.

2) Statistics has lost its basic honesty. It is now used to falsify data for propaganda purposes.[9]

3) Psychology and psychiatry are practically dead, with little chance of recovery.

4) Biology and agriculture are so permeated by quackery that nothing of importance can be expected from them.

5) Physiology, pathology, and medicine are forced into a rigid and stupid orthodoxy which contains much quackery; their future development is inhibited, but some sound practices still survive. News from Moscow (January 12, 1953) shows that the high Soviet officials no longer trust their own physicians (*Pravda*). Their suspicions, of course, are justified, as the physicians dare not use methods in medicine incompatible with "Michurin science."

6) Geology is subjected to political attacks and pure research is discouraged.

7) Astronomy includes the amusing quackery of astrobotany, or the study of plant life on the planets. Certain theories as to the origin of the solar system are forbidden, but nevertheless much excellent work is being done by Russian astronomers.

8) In chemistry, certain theories, such as resonance, are forbidden, but much good chemical research is still being done.

9) The basic philosophical background of modern physics is labeled "idealistic" and is condemned. However, in physics, many of the words are separated from their meanings so that basic research is probably not hurt by its accompanying verbiage. Russian physics is definitely good.

10) Russian mathematics is truly excellent, a science of which any country might be proud.

11) Engineering in Russia is probably adequate for all military and civilian purposes.

What of the future of Russian science? Will the quackery spread until all science is engulfed or will it be contained and ultimately wiped out, so that the Russian sciences will again become sound?

The best answers we can give to these questions cannot be better than educated guesses. We must also limit our guesses to the immediate future. We may hope that in the distant future Russia will again become an important part of the civilized world and that Russian science will again be a part of world science. The prognostications for the near future, however, are not too hopeful.

Before we undertake the perhaps gratuitous task of guessing what the future holds, we should record both the factors which are unfavorable for the recovery of Russian science and those which tend to restore it to health. This procedure should be useful even if it does not lead us to any final answers, even if our listing is very incomplete and we omit many important factors because of our limited knowledge. We really do not have very much to guide us from the past history of science, for the present situation is, in part, unprecedented. We will begin by listing the unfavorable factors. This portion of the over-all picture will be very black indeed.

The first injurious factor is the low quality of many of the professional scientists. In the early days of Communism a great many badly trained and stupid scientists were produced. These discovered that they could rise to positions of importance by keeping their science Marxian and, particularly in biology, by reviving some abandoned doctrines of the eighteenth and nineteenth centuries and presenting them as their own new discoveries. To protect themselves they then had to attack as heretics the abler, older scientists who knew what they had done. These attacks met with such a spectacular success that all the scientists of Russia were taught just what they had to do to survive, and what they had to do did not include publishing the truth as they saw it.

Second, the scientists realized that their position was precarious in fields where science conflicted with Marxian orthodoxy and, in the universities where the advance of quackery was resisted longest, good students were advised by their professors not to specialize in these sciences. The result is that now in Russia certain fields exist in which there are no honest or capable scientists. Thus some sciences cannot recover unless scientists are imported from abroad to train and educate a new generation of Russians.

Third, when an individual scientist has worked in two or more areas and when he is on the wrong side in one of them, he becomes especially vulnerable. He becomes a bad man, a bourgeois reactionary or even a Trotskyite, and he can be attacked in all fields. Thus Orbeli, who was first denounced as a follower of Mendel, was attacked a year and a half later as a bad or western physiologist. Physiology itself turned out to be vulnerable and, with the attack on physiology succeeding, the tactical positions of psychology, pathology, and the related sciences worsened rapidly. Thus quackery was able to spread from science to science.

Fourth, enough of the low-grade scientists were raised by political pressure to membership in the scientific academies to outvote the good scientists. As no dissentient minority is allowed to exist in a totalitarian country, those in the minority were forced to recant. These recantations really shocked the western world. The almost unbelievable conditions under which scientists must live in a totalitarian regime are shown by the following quotation which, incidentally, comes from a source no Communist can question. It is from Stalin himself on the occasion of the reversal of the Party line in linguistics (*Pravda,* June 20, 1950):

> The slightest criticism of the situation in Soviet linguistics, even the most timid attempts at criticism of the so-called "new teaching" [the old Party line] were persecuted and suppressed on the part of the governing circles in the field of linguistics. For a critical attitude toward the heritage of N. Ya. Marr, valuable research workers were deprived of their jobs or demoted. Workers in the field of linguistics were promoted to responsible positions, not on the basis of merit but of unqualified acknowledgement of the teaching of N. Ya. Marr.

Fifth, the basic intellectual climate of Russia is hostile to anyone who is overly fond of the truth. In all scholarly fields, the truth as such is simply ignored. In history, for example, this state of affairs is described excellently in an article by Bertram D. Wolfe,[10] entitled appropriately "Operation Rewrite":

> Histories succeed each other as if they were being consumed by a giant chain smoker, who lights the first volume of the new work with the last of the old. Historians appear, disappear and reappear; others vanish without trace.

Sixth, the stage seems to be set for the spread of Party line science into many fields which it has not yet corrupted. *Izvestia* (November 13, 1952) carried an article by A. Topchiev declaring that it is necessary to conduct large scale discussions in the various fields of biology, chemistry, geology, geography, physics, and other sciences to destroy "obsolete and reactionary theories which hinder the forward movement of science." These discussions, followed by the recantations of those on the losing side, have thus far been the final step in the triumph of the charlatans.

Finally, the Communists have developed within their culture a behavior pattern which has acquired

enough autonomy to grow out of control. The final authority in science, as we have stated, is in the hands of non-scientists and the treatment of scientists has been such that they dare tell their masters only what they believe their masters want to hear. The political authorities in Russia thus have cut themselves off from access to the truth in many scientific fields. All Russian scientists, whether stupid, intimidated, or disgusted, have been reduced to playing the roles of charlatans when they deal with the politicians. The rulers of Russia are consequently not justified in believing anything that their scientists tell them. The result, of course, is that the politicians cannot correct conditions even if they want to, even if they are positive that quackery is rampant. They may eliminate some quacks, but this will not help. Although some who rode to affluence with Lysenko in 1948 have already been liquidated, the over-all situation in the biological sciences has not improved. In medicine, we have the ludicrous spectacle of the physicians and their commissar patients living in terror of each other.

We should emphasize that these more or less autonomous behavior patterns exist in all cultures and that they frequently defy all attempts made to control them. The pattern we have described in Russia now seems to be well entrenched and able to defy even the best intentioned commissars. We have experienced these rebellious cultural patterns in our own civilization, and we cite, as an example, the custom of buying and selling spirituous liquors which persisted in spite of adverse votes and even in spite of a constitutional amendment. The silly custom of duelling also showed a surprising vitality and lasted long after it met with general disapprobation. On a more frivolous level we might mention that the custom of tipping has weathered many attempts at suppression. Today it shows no symptoms of going into a decline.

To return to a more somber plane—a very serious condition arises if a behavior pattern, which is incompatible with a healthy science, develops as an integral part of a social system. In such a society, science has little future. Feuer[11] has shown the basic hostility of the official philosophy of communism—dialectical materialism—to scientific objectivity, and hence its basic conflict with science itself. Hartung[12] has pointed out that science is an institution which interacts with and is dependent on society in many complicated ways. Science is obviously vulnerable to an inimical society, and history shows many examples of science flourishing for a time only to decay and vanish as its enclosing culture grew hostile to intellectual honesty.

So much for the unfavorable factors; they might seem to be sufficient in themselves to doom all science in Russia. We should not overlook other factors, however—factors which work in the opposite direction. Two of these may be of real importance. First, we should note that science becomes healthy when it is merely left to itself and the scientists are free from any nonscientific meddling. Scientists are able to correct any errors they may make, and indeed do so. Actually, if they had not learned to correct past mistakes, modern science itself would never have come into being. Where science is free, it tends to stay healthy. Russian mathematics is a case in point, for it is a field in which the commissars cannot interfere. Any branch of Russian science, where good scientists still remain, may recover if it is only let alone. But this process will be slow under the best conditions. In other branches, where no good scientists remain, recovery can occur only if foreign scientists are brought in as teachers for a native crop.

Second, as we have stated earlier, in engineering and atomic physics, the ease with which the end results can be tested makes them relatively free from quackery. If an atom bomb does not explode, the physicists responsible cannot save themselves by quoting Marx, Engels, and Lenin. Progress may be slower in Russian physics than in the physics of the free world, but we may be sure that it will be made.

One thing we can state definitely about Russian science—it is in a state of flux. It is changing rapidly, and seemingly it is changing in many directions. We know that our guesses as to its future need be only a temporary expedient, because before long the future will be with us and we will know just how Russian science will have developed, during the period when a Russian behavior pattern which was hostile to science got out of control.

References

1. Fast, H. *Daily Worker* (Dec. 26, 1948).
2. Salisbury, H. *New York Times* (Feb. 27, 1949).
3. Darlington, C. D. *J. Heredity*, **38**, 143 (1947).
4. Muller, H. J. *Sat. Rev. Lit.* (Dec. 4, 1948).
5. Newman, H. H. *Multiple Human Births.* Washington, D. C.: American Association for the Advancement of Science (1940).
6. Muller, H. J. *Sat. Rev. Lit.* (Dec. 11, 1948).
7. Polanyi, M. *Sci Monthly,* **60,** 141 (1945).
8. *Soviet Science.* Washington, D. C.: American Association for the Advancement of Science (1952).
9. Rice, S. A. *Sci. Monthly,* **75,** 71 (1952).
10. Wolfe, B. D. *Foreign Affairs,* **31,** 39 (1952).
11. Feuer, L. S. *Philosophy of Science,* **16,** 105 (1949).
12. Hartung, F. *Ibid.,* **18,** 35 (1931).

The Origin of Trade in Indonesia

JUSTUS M. VAN DER KROEF

Dr. van der Kroef was born in Indonesia and is the author of various books and articles on its history and social institutions. He has spent the greater part of his life in the land of his birth, but studied for his Ph.D. at Columbia University. During World War II he served with the Netherlands Government and later made study trips to his native area. At present he is on the staff of the Department of Foreign Studies at Michigan State College.

PERHAPS the most outstanding aspect of some of the so-called primitive cultures that have been studied by social scientists is the close interrelationship that exists among all sectors of human behavior. Traditionally, the religious and moral concepts of the community are closely interwoven with social organization, and these in turn interact with the political structure and with economic pursuits and production patterns. Not one segment of life can be regarded as far removed from the moral religious core of the society in question, and every significant activity of man is directly related to the overarching system of ethical values which gives a community its distinct character.

Among the peoples and cultures of Indonesia, this interrelationship comes very forcibly to the fore. Although Indonesia is now an independent, national state, its inhabitants exhibit a great variety in social institutions and mores and the degree to which they have been westernized. The high rate of illiteracy (still about 75 per cent) and the inadequate communication system, with its resulting isolation, are some of the reasons for these differences. As a result, in many areas the social structure of village society is still intact, and with it the close interrelationship among all the sectors of human behavior. In this article an attempt will be made to show that the origins of trade in many, if not most, Indonesian communities seem to be embedded in the magic-religious core of society and that, as institutions, trade and commerce cannot be understood unless they are related to this religious essence.

Social and Religious Dualism

Fundamental to an understanding of indigenous trade patterns in Indonesia is the widely prevalent dualism in social structure and religion. Where the traditions of clan ordening survive, it is usually two clans or clan units that stand in a particular social and religious antithesis toward each other. One clan supplements the other, and both satisfy each other's needs. One clan unit delivers its women to the other, and in turn receives its women from still a third clan or clan unit (asymmetric connubium). Two social groups always confront each other, not only in marriage relationships, but also in the exchange of specific goods.

An example of such an arrangement is the traditional society of the Toba-Bataks in the central part of Sumatra.[1] Here kinship is based on a system of patriarchal clans. Between two clans a supplementary relationship exists. One clan, the *hula-hula,* delivers its women in marriage to its definite opposite, called *boroe.* As far as possible the hula seeks to marry off its women into this boroe and the "ideal" marriage is that between sister's son of one clan and brother's daughter in the other. Between boroe and hula there exists an important exchange relationship, in which the boroe is regarded as the "inferior" of its hula and as dependent on the latter for both its spiritual and material well-being. From its hula, the boroe receives first of all the *sahala,* the life-giving substance which the entire social group requires. Not only through marriage is the sahala transferred to the boroe, but also through the exchange of food and gifts. This exchange pattern is of the utmost significance, for it is fundamental to an understanding of the origin of trade. Whenever members of the boroe visit the hula-hula, the latter gives them fish to eat. Fish has a symbolic and religious significance, for it is the expression of fertility, whose fruit means long life. A man about to sow his land in the Batak country eats fish, and again after he has sown, in order to bring about a bountiful harvest. The fish eaten by the boroe strengthens its sahala. The hula-hula also gives the *oelos* to its boroe. The oelos is a garment or cloth of certain specifications; it is believed to be magically charged with the sahala. Between two clans there thus exists a traditional exchange in this one commodity, and its special religious significance also comes to the fore in, for example, the case of

a married woman desirous of offspring. Where the marriage has remained barren, the husband will visit the hula-hula of his wife and request that the hula give him the *oelos ni tondi* (the cloth that brings fertility and the life spirit), which is certain to make the wife pregnant. Also, land may be given by the hula to its boroe, and the land thus transferred carries with it the same concept of fertility and bountifulness associated with fish and oelos; for the land produces the food from which the community draws its sustenance, and by partaking of this food the boroe shares in the same physical substance of which the members of the hula are "made."

In return the boroe gives its hula-hula specified gifts, the so-called *piso,* or *pisopiso.*[2] These traditionally included first of all knives or sabers, although these are rarely given nowadays; at present metal objects, including minted coins and gold are generally substituted. The expression often used in the bestowal of the piso (knife) gift is "*piso hadjodjahan*"; i.e., "a knife on which one can lean." The piso is given by the boroe not only by way of exchange for the oelos, but also in other forms, such as food, as a kind of material assistance to the hula-hula when it needs it. For both the boroe and the hula-hula can exchange rice and fruit in place of piso and oelos, respectively. The hula-hula, instead of giving an oelos, or similar gift, can also bestow cattle and livestock on its boroe, and the hula hopes that the cattle will multiply in fertility along with the recipient boroe. It should be remembered, however, that all such gifts are substitutes, and that the pattern of exchange as it is understood among the Batak is that of oelos and piso, garment and knife. The religious and symbolic significance of these two objects will be demonstrated.

Marriage and Trade Relationships

It is remarkable how this kind of trading pattern finds its counterpart among the various peoples of the Indonesian archipelago. For example, on the eastern part of the island of Sumba in eastern Indonesia, many hundreds of miles away from the land of the Toba Bataks, one encounters the same arrangement.[3] Here the patriarchal clans or clan units, called *kabisu,* also have their definite opposites for purposes of trade and marriage. One kabisu delivers the women, the other "receives" (i.e., marries) them, and a third kabisu sends its women in marriage to the first. But at all times the dualistic element in the social order obtrudes: always two kabisu exist, one that receives, the other that gives; the same pattern of cross-cousin marriage emerges between the various clans. With this marriage relationship a trade relationship is united, which in earlier days probably had an entirely ritualistic character. The kabisu that sends its women in marriage to its opposite is also the giver of garments and cloth, sometimes also pigs (traditionally raised by women) and ivory bracelets. The kabisu that sends its males in marriage to its accepted clan opposite, sends first of all knives and sabers, or *parangs,* or sometimes horses, metal earrings, or minted coins. Again the principle that is followed here is the "meeting" and exchange of cloth and knife, respectively, the female and the male element in the clan relationship. In more modern times the range of objects given on both sides has been greatly extended, but the antithetical basis of the exchange is still preserved.

This relationship between opposite clans is irreversible—among the Bataks it is impossible for the boroe to give land to its hula-hula, as this would be sacrilege; nor is the marriage of parallel cousins allowed, as this would be incest. Or, as the Batak puts it, "Water cannot return to its source." The existing antithesis is religiously sanctioned and only through the proper "meeting" of the opposite units is the religious order preserved and strengthened. It should also be noted that the union of the appropriate antitheses has a totemistic character. In marriage, as well as in trade, the totem of the clan of the husband and male (women-receiving and men-sending clan) is joined with the totem of the clan of the wife and female (women-sending and men-receiving clan), symbolizing the cosmic unity of the human group through their mutual antitheses. Two totem "streams," so to speak, meet, each going in an opposite direction, just as in the well-known case of the *kula* of the Trobrianders in Melanesia, studied by Malinowski. The two antithetical commodities exchanged in the kula (bracelets and necklaces, respectively) are also said to have "married" when they are exchanged.[4] And the same can be said for the goods exchanged by the kabisu in east Sumba.

In Indonesia the knife, kris, or saber and the cloth or garment appear over and over as symbols of male and female, respectively, and with them traces of antithetical clan structure, with a preference for cross-cousin marriage, have been noted for virtually all the peoples of the archipelago. The metal knife or kris is a typical implement of the male, associated with hunting, cattle breeding, meat cutting, and so on. And over a wide area, too, metal or metal objects have a magic quality, because they have hardness and durability.[5] The hardness, so it is believed, reflects great strength, magic strength—not even fire can touch it. The owner of this hard-

ness becomes himself infused with magic strength, thus drawing support from the knife; hence the expression "a knife on which one can lean" among the Bataks, and hence the belief, still evident among the Javanese, for example, that a kris has a magic quality that can make its owner fearless and invincible. Similarly the cloth or garment in Java has, when it is prepared in the ancient *batik* or dye, a sacred property; the traditional batik sarong worn by Indonesian women is thus something more than a mere article of clothing, and its purchase is traditionally more than a mere economic transaction.

Another aspect of trade on Sumba needs to be mentioned because of its connection with the fundamental religious antithesis that underlies most traditional commercial transactions in Indonesia. This is the *pahamboer,* or "meeting," involving an exchange on an extra-clan basis between two groups of people, one from the coastal area and one from the mountain region. An agreement is reached between a coastal dweller and a mountain inhabitant that on a specified day they will meet, together with a fixed number of fellow-villagers each, for the purpose of trade on a barter basis. The people from the coast bring sea products, fish, coconuts, and clothing, those from the mountains bring food or sometimes crockery. The number of participants must be the same on each side, so that there is no risk of having to take back home what was originally brought for trade. A fixed date is set and the trade or exchange takes place.[6]

This aspect of trade is important for two reasons. First the pahamboer is surrounded by the old antithesis of two, although in this case not necessarily related, groups; although the action is trade, the term given to the transaction is "meeting." One would expect a term that would indicate the commercial character of the process; instead the element of coming together, of "meeting," is emphasized.[7] The same principle is, as has been shown, inherent in the exchange of male and female goods among the kabisu; here the totems of two opposite clans meet and are fused into a higher, religious unity. Proof of the antithetical character of pahamboer is further strengthened by a look at the similar dualism in society and religion in the island of Bali. In the pahamboer it is the sea people and the mountain people who come together. In the Balinese world order it is exactly this dualism between the mountain region and the sea that determines the entire sacred character of the social order and its religious function, reflected in village society, social classes, and cultural life.[8] Again the sacred antithetical element that underlies the trade transaction of the pahamboer, which has similar manifestations in many regions of the Indonesian archipelago, is evident.

It may seem scientifically unwarranted to stress similarities in such widely separated cultures as those of the Toba Bataks and the inhabitants of Sumba, or to emphasize identical patterns of antithetical social ordening for many, if not most, peoples in the Indonesian archipelago. The fact is that the evidence thus far amassed points undoubtedly to the existence of a clan dualism, or to its distinctive traces, with a corresponding exogamous (usually cross-cousin) marriage pattern and a religious-commercial supplementary function of clans or clan units in such widely scattered areas as the central part of Timor, Tanimbar, Seran, Wetar, Amboyna—indeed, in virtually all Eastern Indonesia except for Sulawesi (although among the Toradjas of central Sulawesi traces of it exist—in Sumatra, where even the Minangkabau society shows remains of it; in Borneo among many Daya communities; and finally even in Java.[9] Amboyna, for example, still exhibits the remnants of the ancient antithetical dualism in the structure of its village society. The village in Amboyna is divided into two parts; every part is not only a social unit but a cosmic classification category comprising all objects and events in the world around the villager. A list could be set up classifying all objects and characteristics associated with each of the two divisions:[10]

Left	Right
Female	Male
Coast or seaside	Land or mountainside
Below	Above
Earth	Heaven or sky
Spiritual	Worldly
Downwards	Upwards
Peel	Pit
Exterior	Interior
Behind	In front
West	East
Younger brother	Older brother
New	Old

The two divisions in the village are exogamous; marriage within one's own division is prohibited. This is a clear indication of orginal clan structure, although the clan as such has disappeared and in its place has come the closed village unit. The antithetical character of this ordening is of course quite apparent. In Java the ancient dualism has been further subdivided into a quadruple social division, also mutually complimentary and in time calling into existence an entire system of correspondences in which every color scheme, object of trade, profession, and household feature has its definite place.[11]

To be sure, the minor details of this great anti-

thesis may differ, but fundamental to all such communities is the comprehensive dualism in the structure of their society, religiously symbolized and sanctioned, and dependent upon the complementary function of the social units in marriage and economic pursuit. It is the universality of this dualistic element in Indonesian society that is perhaps its unique characteristic; it is also the key to an understanding of the origin and function of its trade.

Before analyzing the religious dualism in Indonesian society as it relates to trade, it is essential that some attention be paid to the potlatch theory, which has been offered as an explanation of the dualistic trade pattern in many societies beyond Indonesia. Briefly stated, this theory as developed by van Ossenbruggen, is as follows.[12]

The cloth and the knife, which recur over and over as symbols of a marriage and trade relationship, are not only representations of the female and male elements in society, but also of the one-time military supremacy of one clan over another. In Indonesia generally it is undoubtedly true that the woman-giving clan—for example the hula-hula among the Batak—is believed to be superior to the woman-receiving clan, the boroe. The latter provides the males for the marriage and sends out "male" gifts, the pisopiso. This sending of the piso (traditionally knives and sabers) is interpreted as symbolizing the surrender of the boroe to the superior hula-hula, "a putting down of the weapons by the vanquished tribe at the feet of the victor." It is quite possible that the relationship between hula-hula and boroe was not always a friendly one, and other studies of similar clan antitheses in Indonesia show that a spirit of enmity sometimes still lingers between the woman-sending, superior clan and the woman-receiving inferior clan. The enormous respect and awe of the boroe for its hula, even today, is perhaps a survival of this. According to this theory, the piso gift is therefore, in its original form, a symbol of surrender, given by the defeated to the victorious clan. Although in recent times actual knives or krisses are rarely given, the substitute gifts (metal, money, rice, or fruit) still have the name and symbolic significance of the pisopiso. The trade that thus develops arises out of a traditional enmity between two originally hostile groups, in which the victor perhaps tried to pacify and to bind the vanquished closer together for the sake of future peace and harmony by giving—as among the Bataks—land, cloth, and cattle, or food, all supposed sources of prosperity and fertility for the defeated group.

Out of this exchange arises the potlatch tradition, also characteristic of many peoples from the North American continent to Melanesia. Among certain North American and Melanesian clans, tribes, phratries, or other social units a most elaborate and excessively generous exchange of goods often takes place; blankets, shields, or bracelets by the hundreds if not thousands are given away, thrown away, or even destroyed with reckless abandon, each tribe trying to outdo the other. Elaborate meals are held, and hour-long speeches by chiefs are given, every one a challenge, designed to best the other. "It is, as it were, a waging of war by means of destroying or giving away various commodities;" hence, among the Tlingits of North America the potlatch is a "war dance," and the lance, or knife, on a stick as among the Kwakiutl, is its symbol.[13] Yet the purpose of the potlatch among the Tlingits, like the purpose of the clan trade among the Bataks and other Indonesian peoples, is the strengthening of the spirit of the collective soul, the life force of the community. All this could perhaps point to a rudimentary potlatch character of the dual Indonesian trade pattern. Further proof of the theory might conceivably be derived from the trading tradition among certain Toradjas of Sulawesi (Celebes). Within the village society there is virtual communism—every villager can obtain someone else's property. It is a different matter with persons of a neighboring though related village. These are not allowed to take property away free. In the case of such a Toradja who wants something, the customary procedure is to build up a pattern of mutual obligation. First, the Toradja in question bestows a gift on the owner of the object he desires. The latter is now under an obligation to give something of equal, and more often of even greater, value; not to do so would be an admission of inferiority. Thus a whole process of exchange can be set into motion, one gift forcing a countergift. In certain circumstances "this can lead to a contest in generosity and liberality, which can work in a very uneconomic manner." It is the element of trying to outdo each other in bestowing gifts that attracts our attention here; this element is basic to the entire potlatch process.[14]

Thus it would seem that the continuous exchange of specified goods between clans or clan units in Indonesia, carrying with it the possible existence of an ancient pattern of hostility between the clans and the tradition of the "surrender" of one to the other, is a rudimentary potlatch. Interesting as this theory is, it does not, unfortunately, account for all the facts. For one thing, it places too much emphasis on the giving of weapons as a symbol of sur-

render, and ignores the importance of the cloth gifts as symbols of fertility. The exchange of cloth and knife is not merely dictated by an ancient enmity or a desire to best the other clan in generosity. Cloth and knife are symbolic of female and male functions in society, which find their reflection in the carefully arranged marriage relationship between the clans, in which the traditional male element joins the accepted female element, both merging in a higher cosmic unity. This unity is constantly emphasized in the ritual, totems, and legends of most Indonesian clans. The potlatch theory does not seem to make much of a provision for this, unless one would wish to argue that the religious and social antithesis between the clans is but a later addition to the culture pattern of the group, a rationalization that came into existence, perhaps, after the potlatch process was established. Proof for such an argument, however, is altogether lacking, at least in Indonesia. Finally, it should be noted that the Toradjas are one of the few peoples in Indonesia with imperfectly developed clan dualism.

Religious Symbolism in Trade

The explanation of the trade antithesis lies elsewhere, namely, in the religious beliefs of the Indonesian clan societies. Although religious life and symbolism may vary in detail in these societies, again, most if not all, seem to subscribe to one fundamental sacral principle—the existence of two antithetical forces, which confront but also constantly merge and supplement each other in a higher sacred unity. In religious life this antithesis expresses itself in the opposition of deities of heaven and earth, and also in the dualism of male and female, or rather of the patriarchal and the matriarchal principles of life in the community; hence, in many areas, such as in certain parts of Java, one finds the belief in the descent of an entire village society from a single pair of ancestors, man and wife, who in turn descend from the male and female principles of the universe—i.e., the gods. Hence also the repeated appearance of deities or spirits, who are completely identified with male and female, earth and water, East and West, upper and under world. An example is the existence of the two ancient Batak spirits, Boras pati ni tano and Boroe saniang naga, who are personified forces of nature.[25] The first of these is male, the protector of fertility, who is invoked when a village is founded or a house is built and is associated with the earth.

The second is female, who often appears as a snake; all that has to do with water and its resources is connected with her. Most of the other gods of the Bataks appear to be of Hindu origin; perhaps only the earth god and the water goddess, the male and the female, seem to be original and unique in their society, as symbols of the great antithesis. Again, in the Fialarang myth of the Timorese in Eastern Indonesia, the antithesis reappears in the creation story of two original beings, a female earth and a male sky, out of which the human species and the universe have developed.[16] On the island of Bali it is mountainside and seaside, with gods and demons representing the upper world (mountains) and the nether world (sea), that influence the structure of religious beliefs as well as the social organization. One result of the antithesis is a magically sanctioned dualism in Balinese village society and a "male" and "female" division in the production of the rice on the communal fields.[17] Among the Ngadju Daya of Kalimantan (Borneo), the antithesis is represented by the two principal divine manifestations: the male Mahatala, symbolized as a hornbill, and master of the upper world, and the female Djata, represented as a watersnake and ruler of the under world. The entire cosmic outlook of the Ngadju is determined by his belief in the constant interaction and unity of these two divine principles; God to him is the oneness of these two divine representations: "The complete Godhead is watersnake and hornbill, Upper World and Under World, male and female, sun and moon, the holy lance and the holy cloth. . . ."[18] Again the appearance of the lance or knife and the cloth is not accidental. They are, as we have seen, the magic symbols of the traditional antithesis, the dualistic elements the interaction and subsequent unity of which are basic to the stability of the universe. In Minangkabau, in Java, in Sumba, even among the Papuas of New Guinea, the traces of this "dualistic monism" are apparent, in religion, in social organization, or in trade.[19]

It would appear, then, that the traditional exchange of gifts, fundamental to an understanding of the trade between clans or clan units in Indonesia, cannot be separated from the religious-cosmic point of view of the community in which the trade takes place. Elements of quality, cost price, profit, loss, or other economic rationalizations do not traditionally enter into the transactions. Trade is a sacred act, a ritual that satisfies first of all a moral-religious need. For it must be understood that the divine order, with its dualistic character, requires constant "rejuvenation"; i.e., the community must constantly repeat the processes of merging, "meeting," and fusion of the traditional antithetical elements, by proper marriage relations, by religious and totemistic ceremonies, and by

trade.[20] Only in this way, in the belief of the Indonesian, can the magic qualities of the universe be kept in balance and the continuity of the family and of the village be religiously assured.

From this, I think, it also follows that any economic improvement scheme, conceived along Western lines, such as those of the Point Four agencies, must reckon with the religious element in the economic want-creating process. A rationalized commercial life is alien to Indonesian tradition; should one wish to establish it, one will have to attack also the religious basis of society. For the Indonesian it is therefore not so much a question of learning better techniques or of acquiring unlimited wants for modern consumer goods as a question of espousing new cultural values, in which the adoption of the amoral orientation of Western economic theory and enterprise is the essential component.

References

1. VERGOUWEN, J. C. *Het rechtsleven der Toba Bataks,* 48–72 (1933).
2. *Ibid.,* 72–73.
3. NOOTEBOOM, C. *Oost Soemba,* 23–24, 102, 106, 127 (1940).
4. MALINOWSKI, B. K. *Argonauts of the Western Pacific,* 93, 356 (1922); also cited R. van Dijk in *Samenleving en Adatrechtsvorming,* 56 (1948).
5. LESQUILLIER, N. W. *Het adatdelictenrecht in de magische wereldbeschouwing,* 16 (1934).
6. NOOTEBOOM, C. *op. cit.,* 104–105.
7. VAN DIJK, R. *Op. cit.,* 57.
8. SWELLENGREBEL, J. L. *Kerk en Tempel op Bali,* Chap. 111 (1948).
9. VAN DIJK, R. *Op. cit.,* 34–38.
10. DUYVENDAK, J. P. *Inleiding tot de ethnologie van den Indonesische Archipel,* 3rd ed., 94–96 (1946).
11. *Ibid.,* 116–17.
12. VAN OSSENBRUGGEN, F. D. E. *Mededeel. Koninkl. Akad. van Wetenschap., Afdeel. Letterk.,* Deel 80, Ser. B, (3), 19 (1935).
13. *Ibid.,* 23.
14. DUYVENDAK, J. P. *Op. cit.,* 109–110.
15. VERGOUWEN, J. C. *Op. cit.,* 81.
16. VAN WOUDEN, F. A. E. *Sociale Structuurtypen in de Grote Oost,* 100–104 (1935).
17. GRADER, C. J. *Mededeel. Kirtya Liefrinck Tuuk* (Singaradja, Bali) aflevering 5, 66 (1938).
18. SCHÄRER, H. *Die Gottes Ideo der Ngadju Dajak in Süd Borneo,* 22 (1946).
19. VAN DIJK, R. *Op. cit.,* 35 ff.
20. *Ibid.,* 38.

WHEN I LOOK UP

When I look up into the stars,
Into the vast and velvet dark,
Gaze on black wonder of the sky,
Immense and stark,

I hear the winds beyond the dusk
That play through heaven like a lute,
Above the mountains of the clouds,
And I am mute,

And dumb and shaken by the wide
Tremendous worlds of silver light,
And I am one with boundless realms
Of stellar night. . . .

ANTONIA Y. SCHWAB

Ithaca, New York

The Teaching of Limnology in the United States*

DAVID G. FREY

The author, associate professor of zoology at Indiana University, has carried on research in widely separated areas. After receiving a Ph.D. degree from the University of Wisconsin in 1940, Dr. Frey spent five years as aquatic biologist with the U.S. Wildlife Service in Washington, Oregon, and Idaho, and in oyster research in the Virginia-Maryland area. Work with the U.S. Naval Medical Research included studies of fresh water areas in island groups—Mariana, Palau, Admiralty, and Yap. This was followed by a survey of war damage to fisheries in the Philippines. From 1946 to 1950 Dr. Frey taught limnology at the University of North Carolina.

LIMNOLOGY is properly the comprehensive and comparative study of all inland waters, regardless of their chemical content, temperature, or other characteristics. There are many different categories of such waters, but the most important in terms of their occurrence and direct relation to man are rivers, lakes, ponds, and reservoirs. Less important types would include hot springs, subterranean waters, bog waters, tree hole puddles, and temporary lakes and ponds in endorheic regions. The limnologist is usually first of all a biologist and only secondarily a chemist, physicist, geologist, meteorologist, and/or hydrologist; but the field is so broad and diversified that a fair number of the currently more than 1100 members of the American Society of Limnology and Oceanography are specialists in one of the physical sciences and only secondarily are biologists. Yet the ultimate goal of limnology is the understanding of the processes relating to the production of organisms in the water and by the water, and the differences between waters of the same general type. All processes—physical, chemical, and biotic—must be properly evaluated and integrated in order to arrive at acceptable conclusions. Stephen A. Forbes in 1887 emphasized the necessity for comprehensive studies in the following words:

> If one wishes to become acquainted with the black bass, for example, he will learn but little if he limits himself to that species. He must evidently study also the species upon which it depends for its existence, and the various conditions upon which these depend. He must likewise study the species with which it comes in competition, and the entire system of conditions affecting their prosperity; and by the time he has studied all these sufficiently he will find that he has run through the whole complicated mechanism of the aquatic life of the locality, both animal and vegetable, of which his species forms but a single element.

Too often the desirability of, and even necessity for, comprehensive studies is lost track of.

Limnology is considered to have had its real beginnings with the numerous and careful studies of Forel on Lake Geneva, Switzerland, in the late 1800s (Berg). With a few notable exceptions most of the centers of limnological research since that time have been established in the lake districts of Europe, Siberia, Japan, and North America. Work on lakes has been more intense than on other types of waters, and undoubtedly the limnological processes here are better understood than elsewhere. All the books on limnology that have been suitable as texts have dealt primarily with lake limnology. (The recent semipopular book by Macan and Worthington attempts to give a more balanced treatment of rivers and lakes, both their pure and applied aspects.) As a result of this emphasis in the textbooks, most limnology courses likewise emphasize lakes, and the beginning student often gets the erroneous impression that limnology is the study of lakes alone. To help correct this condition one of the real present needs in the field of limnology is a good summary of the current status of our understanding of the limnology of rivers, reservoirs, and ponds.

In 1930 J. G. Needham, reporting for a subcommittee within the National Research Council, listed 16 institutions in the United States and Canada offering courses in limnology. At the present time in the United States alone limnology is known to be taught at 66 separate locations, including eight

* Contribution No. 516 from the Department of Zoology, Indiana University, Bloomington. This paper was presented as part of a symposium "Teaching of Ecology in Urban Areas," sponsored jointly by the Ecological Society of America and the Botanical Society of America at Cornell, September 1952.

summer biological stations (Table 1). Moreover, the number of institutions in Canada offering courses in limnology is known to be much greater than the two listed in the 1930 report, although no attempt was made to determine their actual number. This is a remarkable growth within the past 22 years, and it is worth our attention to examine the present status of limnology in the United States as a guide to the problems involved in teaching the subject.

The geographic distribution of limnology in the United States is not correlated appreciably with the occurrence of natural lakes. Only ten of the states do not give some course in limnology; five of these, however, offer work in ichthyology, fishery biology, or fish management. Only the Great Plains and the Great Basin are largely without courses in limnology, although a course has been given at the University of Kansas for many years, and more recently a course has been started at the University of Nevada. The latter location is reported to have quite a variety of both lotic and lenitic habitats within a radius of 50 miles. The rather numerous courses in the Texas-Oklahoma region depend primarily on reservoirs for field work and demonstration of principles.

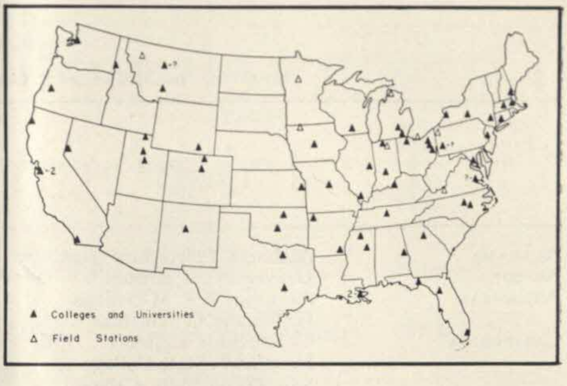
The distribution of limnology courses is determined primarily by the location of the large, state-supported schools, both the liberal arts schools and those technical schools that have set up programs in game and fish management. Approximately 80 per cent of all limnology courses in the United States are given at schools of this general description.

Two additional factors have helped determine the location of limnology courses:

1. The interest, drive, and activity of *men* trained in limnology as they have become located away from their alma maters. Several of these have been able to establish limnology courses at some of the smaller schools, and others are maintaining their research interests in limnology, even though they have been unsuccessful in setting up a course in limnology at a particular institution. In some instances, the research activities of these men have stimulated capable students to enter the field of limnology.

2. The *geographic factor* where it is particularly favorable has in a few instances influenced or at least facilitated the setting up of a course in limnology, especially at some smaller institutions, and in other instances where it is unfavorable has probably helped discourage establishing such a course.

Limnology is a specialized study, requiring considerable background training on the part of the student in the supporting sciences of biology, chemistry, physics, geology, and mathematics. For this reason courses in limnology are usually given at the advanced undergraduate-graduate level, frequently at the graduate level alone, and only in twelve reported instances at the undergraduate level exclusively. Students are usually required to have had an extensive training in other science courses before taking limnology. Sometimes only general zoology or biology is required, but more frequently several courses in zoology, plus botany, chemistry, and physics may be required. Naturally those courses offered at the graduate level generally have the more stringent prerequisites.



Locations where limnology is taught in the United States, 1952.

Such restrictions based on prerequisites limit the enrollment. Reported average enrollment in limnology courses varies from 3 to 60, with a mean of approximately 10. The largest enrollments occur at Wisconsin and Michigan State, where the elementary courses are offered every year. Each year approximately 750 students in the United States are exposed to some sort of organized course in the subject under discussion; usually these courses are designated limnology, sometimes hydrobiology, and less frequently aquatic biology. In quite a number of instances a particular course is offered only in alternate years, in an attempt to increase enrollment.

Limnology as it is now taught in the United States tends to be restricted to the larger schools, especially the state-supported schools, for three obvious reasons:

1. A *large student body* is required to maintain sufficient enrollment in such a specialized course, unless the course is given at the cultural level with a minimum of field and laboratory work. The land-grant technical schools of the various states and the combined technical and liberal arts schools of the Middle West have an advantage as regards

Table 1

Location of Limnology Courses in the United States

State	School	Location	Highest advanced degree offered with limnology as thesis subject
Alabama	Alabama Polytechnic Institute	Auburn	None
Arizona	University of Arizona*	Tucson	
Arkansas	Arkansas A & M College	Monticello	None
	University of Arkansas	Fayetteville	Master's
California	Chico State College†	Chico	
	Humboldt State College	Arcata	None
	San Diego State College	San Diego	Master's
	San Francisco State College*	San Francisco	
	San Jose State College	San Jose	Master's
	Stanford University	Stanford	Ph.D.
	University of California*‡	Berkeley	"
	University of California*	Los Angeles	
Colorado	Colorado A & M College	Fort Collins	Master's
	University of Colorado	Boulder	Ph.D.
Connecticut	Yale University	New Haven	"
Florida	Florida State University	Tallahassee	Master's
	University of Florida	Gainesville	Ph.D.
	University of Miami	Coral Gables	Master's
Georgia	University of Georgia	Athens	"
Idaho	University of Idaho	Moscow	"
Illinois	St. Procopius College*	Lisle	
	Southern Illinois University	Carbondale	Master's
	University of Illinois	Urbana	Ph.D.
Indiana	Earlham College Biological Station	Dewart Lake	None
	Indiana University	Bloomington	Ph.D.
	Notre Dame University	Notre Dame	"
Iowa	Iowa State College	Ames	"
	Iowa Lakeside Laboratory	Lake Okoboji	(Master's)
Kansas	University of Kansas	Lawrence	Ph.D.
Kentucky	University of Kentucky*†	Lexington	
	University of Louisville	Louisville	Master's
Louisiana	Louisiana Polytechnic Institute*	Ruston	
	Loyola University	New Orleans	None
	Tulane University	New Orleans	Master's
Maine	University of Maine*	Orono	
Maryland	University of Maryland	College Park	Ph.D.
Massachusetts	Harvard University	Cambridge	"
	University of Massachusetts	Amherst	Master's
Michigan	Michigan State College	East Lansing	Ph.D.
	University of Michigan	Ann Arbor	"
	University of Michigan Biological Station	Douglas Lake	(Ph.D.)
Minnesota	University of Minnesota Biological Station	Lake Itasca	(Ph.D.)
Mississippi	Mississippi State College	State College	Master's
	University of Mississippi	University	"
Missouri	University of Missouri	Columbia	Ph.D.
Montana	College of Great Falls§	Great Falls	
	Montana State College	Bozeman	Master's
	Montana State University Biological Station	Flathead Lake	(Master's)
Nebraska	University of Nebraska*	Lincoln	
Nevada	University of Nevada	Reno	Master's
New Hampshire	University of New Hampshire	Durham	"
New Jersey	Rutgers University	New Brunswick	Ph.D.
New Mexico	University of New Mexico	Albuquerque	"
New York	Cornell University	Ithaca	"
	University of Buffalo	Buffalo	Master's
	Vassar College	Poughkeepsie	"
North Carolina	North Carolina State College	Raleigh	Ph.D.
	University of North Carolina	Chapel Hill	"
Ohio	Bowling Green State University	Bowling Green	None
	College of Steubenville	Steubenville	"
	Franz Theodore Stone Laboratory of Ohio State University	Put-in-Bay	(Ph.D.)
	Kent State University	Kent	Master's
	Youngstown College	Youngstown	None

TABLE 1—(*Continued*)

State	School	Location	Highest advanced degree offered with limnology as thesis subject
OKLAHOMA	Oklahoma A & M College	Stillwater	Ph.D.
	University of Oklahoma	Norman	Ph.D.
	University of Tulsa†	Tulsa	
OREGON	Oregon State College*	Corvallis	
	University of Oregon	Eugene	None
PENNSYLVANIA	Duquesne University§	Pittsburgh	
	Pennsylvania State College*†	State College	
	University of Pittsburgh Biological Station	Lake Pymantuning	(Ph.D.)
RHODE ISLAND	University of Rhode Island*	Kingston	
SOUTH DAKOTA	South Dakota State College*	Brookings	
TENNESSEE	University of Tennessee*	Knoxville	
	Vanderbilt University	Nashville	Ph.D.
TEXAS	A & M College of Texas	College Station	Master's
	North Texas State College	Denton	"
	Sam Houston State College*	Huntsville	
	University of Texas*†	Austin	
UTAH	Brigham Young University	Provo	"
	University of Utah	Salt Lake City	Ph.D.
	Utah State Agricultural College	Logan	None
VIRGINIA	College of William and Mary§	Williamsburg	Master's
	University of Virginia Biological Station	Mountain Lake	None
	Virginia Polytechnic Institute*†	Blacksburg	
WASHINGTON	State College of Washington†	Pullman	
	University of Washington	Seattle	Ph.D.
WISCONSIN	University of Wisconsin	Madison	"
WYOMING	University of Wyoming	Laramie	Master's

* These schools give courses in ichthyology, fishery biology, or fish management, but not in limnology; the list of these schools is admittedly incomplete.
† These schools have recently discontinued instruction in limnology.
‡ Although no courses in limnology are offered, advanced degrees can be earned with limnology as a specialty.
§ The information listed has not been confirmed.

enrollment, in that a fair number of students are interested in limnology for various practical reasons, for example, fish management or sanitary engineering. Limnology is frequently a required course for students specializing in these technical subjects. It is significant that, at all schools except one, other courses in aquatics and ecology are given besides limnology. These include courses in various phases of ecology, algae, aquatic plants, ichthyology, fish management, fishery biology, biogeography, stream pollution, plankton, aquatic insects, and various field courses. Hence, limnology is almost invariably just one element in a diversified program in ecology and field studies, and a student can receive a fairly broad training in these fields.

2. A fairly *large biology staff* is required to free a man from the duties of teaching such courses as embryology, invertebrates, and comparative anatomy, which are usually considered more fundamental in the training of the general biologist and of the premedical student. It should be noted that nearly all the present teachers of limnology have had their basic training in zoology. An interesting exception is at the University of Massachusetts, where five separate departments cooperate in giving the course. This again illustrates the broad scope of the subject.

3. Considerable *money* is required for the purchase and maintenance of the hand-made equipment and instruments needed in carrying on field studies and laboratory analyses. Many of these items are too prohibitive in cost to be acquired only for class demonstration, but if they can also be used in the research activities of staff members and graduate students, the schools are much more likely to purchase them. Acquisition of these items is frequently aided by cooperative research programs between the schools and the state conservation departments.

At any institution various problems arise in the teaching of limnology, and the nature of these problems is in part related to geographic location. No single school can expect to have the entire gamut of inland waters close enough at hand for convenient instruction and research purposes; Washington and Oregon probably come as close to this ideal as any places in the country. Different locations have different problems: Oklahoma, Texas, and Kansas necessarily work almost exclusively on reservoirs; permanent streams are lacking close to Stanford, San Diego, and Miami; hard-bottom

Long Lake, west of Boulder, Colorado, at an altitude of approx. 11,000 feet. This lake might be considered typical of the many alpine lakes in the Rocky Mountain Region. (Photo by R. W. Pennak, University of Colorado.)

streams and thermally stratified lakes are not available close to New Orleans, whereas eutrophic lakes and streams with moderate to weak gradient are difficult to reach from the Flathead Lake Station in Montana; most schools in the northern tier of states cannot satisfactorily demonstrate thermal and chemical stratification in lakes during the regular school year, and Michigan State, therefore, concentrates its field work on the effects of ice and snow cover and other aspects of winter limnology. Because of the annual cycles that occur in waters, summer is probably the best time to teach limnology, although the geographic limitations of the particular field stations still hold. The Franz Theodore Stone Laboratory on Lake Erie is well situated for the study of many aspects of limnology, but its classes must repair to the mainland for direct observations on streams and stratified lakes. It should be possible, however, regardless of location, to teach the *principles* of the subject in lecture, and to illustrate a satisfactory number of these principles by studies on the particular waters that are available.

Rutter's Pond—a $4\frac{1}{2}$-acre artificial pond in central Oklahoma with an annual catch of fish by hook and line of 800 pounds per year. In many parts of the United States lakes and ponds must be provided artificially by man. (Photo by Lee Stevens. Furnished by W. I. Irwin, Oklahoma A & M College.)

Closely associated with the variety of aquatic habitats available for study is the matter of textbooks and literature. It has already been pointed out that as a result of a combination of circumstances all suitable textbooks stress the limnology of natural lakes. Persons working on the limnology of reservoirs in Texas, Oklahoma, and Alabama do not find current limnology texts particularly applicable to their situations. They feel a strong need for a book summarizing the limnology of reservoirs.

Courses specifically in stream limnology are given at Colorado and Indiana, although a number of other schools, such as New Mexico and Utah, stress stream limnology because of their geographic location. Here, in particular, the time is ripe for someone with the necessary time and energy to sift through the vast number of papers on stream limnology in a considerable variety of languages and to boil the various ideas down to their essentials to determine what generally applicable principles can be established. Such an undertaking would serve as a much needed stimulus to further work on streams.

Farm ponds, which are widely distributed over the United States, serve a variety of purposes, including the production of fish for the food and pleasure of the landowner and his friends. Yet the limnology of these small bodies of water and the factors controlling the production of fish in them are inadequately understood. In these waters, probably even more so than in lakes, emphasis in the United States has been on the fish management aspects of the subject.

This emphasis on the applied aspects of aquatic studies in the United States is attested to by the fact that at least seventeen schools in the counry offer courses in fishery biology, fish management, or ichthyology without at the same time offering any course specifically in limnology. Undoubtedly a certain amount of limnology is given in at least some of these courses, but one could scarcely expect this to substitute for a regular intensive course in the subject.

Another problem in the teaching of limnology is that of *field work,* and the *equipment* and *transportation* involved. It is quite generally agreed by those who teach limnology that field work is essential, although there are a few exceptions:

Wisconsin does not feel that field work is essential in the undergraduate course but is in the graduate course; Washington feels that field work might be dispensed with, if necessary, in the undergraduate course; and Yale does not provide any field experience in its graduate course.† Courses elsewhere vary from three or four field trips per quarter or semester up to almost daily field work at several of the summer stations. The number of field trips per course most commonly lies between six and ten.

Field work can have either of two main objectives: It can serve to give the student *experience* in many of the field methods of limnology, or it can serve to *illustrate* some of the methods used in gathering the data on which the principles discussed in lecture have been based. The former objective is that of the school offering training in fish management and perhaps also of the school with an extensive graduate program in limnology. It is necessarily limited to small classes for reasons of logistics and mechanics. Where a class is large or less selected the second objective is much more appropriate.

The initial cost, storage, and maintenance of equipment can be a considerable problem, particularly at the smaller school, and, as might be expected, the variety of limnological equipment available varies tremendously from school to school. Cost of equipment is such that most institutions must acquire the desired items over a number of years.

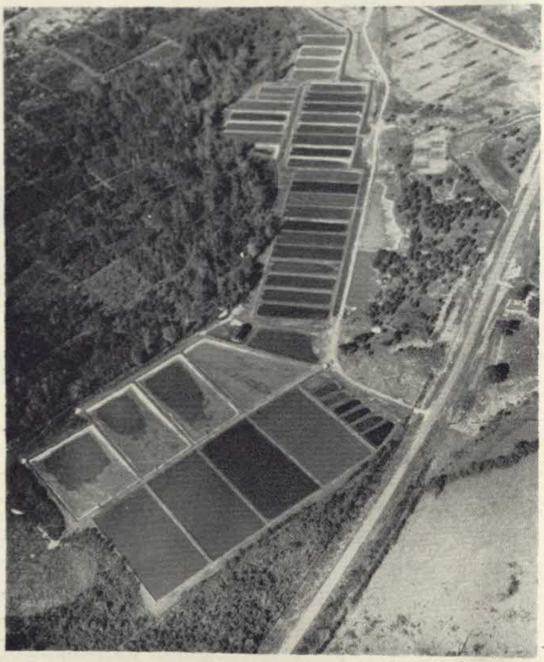
Although limnology is quite widely taught today, particularly in the large, state-supported schools, opportunities for graduate work in the field are more restricted, depending on the size of the school and the diversity of its faculty. Ten of the institutions listed in Table 1 do not offer any advanced training in limnology: 27 offer the master's degree in zoology, with limnology as a specialty; and 30 offer the Ph.D. on the basis of limnological problems. Not all in the last group are equally important in contributing to the field. Of the persons teaching limnology in the United States at present, half have had their training in one of five Midwestern states —Michigan, Illinois, Wisconsin, Minnesota, and Indiana, in that order. The only other concentration of background is the Cornell-Yale-Harvard region, accounting for approximately one fifth of the total.

Recent studies (for example, Knapp and Goodrich, 1951) have pointed out the disproportionate importance of small liberal arts colleges in contributing to the ranks of qualified scientists in the United States, which raises the question of whether there is not some way the potential scientists at these institutions cannot at least be made aware of limnology and the opportunities it offers. Possibly a good means of accomplishing this to the benefit of ecology as a whole, and to the benefit of students through the broadening of their training, would be the setting up of balanced courses in general ecology in those schools where they are not already present. Out of 521 schools whose recent course catalogues were available at Indiana University, 45 per cent offer one or more courses in some phase of ecology. At some schools the courses are listed as field botany or field zoology, but it is clear from the accompanying descriptions that at least a modicum of ecology is given. Most courses are listed either as plant ecology or animal ecology, with the former predominating. Many schools with biology departments list the

† If one recalls the fact that Hutchinson has trained such outstanding limnologists as Riley, Deevey, Brooks, and others, one doubts seriously that formal field training is a necessity.

Part of Jones Lake, North Carolina, showing the ancient cypresses characteristically draped with Spanish moss. The remarkable "bay" lakes in this region extend well back into Pleistocene time. (Photo by the author.)

Linsley Pond, Connecticut, a small natural lake which has contributed to fundamental advances in limnology out of all proportion to its size. (Photo furnished by G. E. Hutchinson, Yale University.)



One of the groups of experimental fish ponds at Alabama Polytechnic Institute used by H. S. Swingle and others in studying fertilization and fish populations. The largest ponds are 1 acre in area. (Photo furnished by J. S. Dendy, Alabama Polytechnic Institute.)

courses simply as ecology or general ecology, occasionally as bioecology, although undoubtedly these courses are often strongly slanted toward plants or animals, depending on the training of the teacher. There are also a considerable number of specialized courses such as range ecology, forest ecology, ecology of arable lands, ecology and control of weeds, insect ecology, trophic ecology, etc. In those schools where a student has the opportunity to take just one course in ecology, it is certainly desirable for him to be instructed in the over-all aspects of the subject rather than in some restricted man-made compartment of the field.

Bibliography

BERG, K. *Proc. Intern. Assoc. Theor. Applied Limnol.*, **11**, 41 (1951).

BREHM, V. *Einführung in die Limnologie.* Berlin: Julius Springer, 261 (1930).

CARPENTER, K. E. *Life in Inland Waters.* New York: Macmillan, 267 (1928).

FORBES, S. A. *Bull. Sci. A. Peoria,* **77**, (1887); Repr. in *Illinois Nat. Hist. Survey Circ.*, **15**, 537 (1925).

FOREL, F. A. *Handbuch der Seenkunde. Allgemeine Limnologie.* Stuttgart: Englehorn, 249 (1901).

HENTSCHEL, E. *Grundzüge der Hydrobiologie.* Jena: Gustav Fischer, 221 (1923).

KLEEREKOPER, H. *Introdução ao Estudo da Limnologia.* Sér Didática No. 4, Ministério da Agricultura, Rio de Janeiro, 329 (1944).

KNAPP, R. H., AND GOODRICH, H. B. *Science*, **113**, 543 (1951).

KUHN, H. *Lebenskunde der Gewässer.* Zurich: Orell Füssli, 167 (1945).

LENZ F. *Einführung in die Biologie der Süsswasserseen.* Berlin: Julius Springer, 221 (1928).

LIEBMANN, H. *Handbuch der Frischwasser- und Abwasserbiologie.* München: R. Oldenbourg, 539 (1951).

MACAN, T. T., AND WORTHINGTON, E. B. *Life in Lakes and Rivers.* London: Collins, 272 (1951).

NEEDHAM, J. G. *Science,* **71**, 265 (1930.)

NEEDHAM, J. G., AND LLOYD, J. T. *The Life of Inland Waters,* 3rd ed. Ithaca: Comstock, 438 (1937).

RUTTNER, F. *Grundriss der Limnologie,* 2nd ed., rev. Berlin: Walter de Gruyter, 232 (1952). (An English translation of this revised edition by F. E. J. Fry and D. G. Frey has been published by the University of Toronto Press.)

THIENEMANN, A. *Die Binnengewässer Mitteleuropas.* Die Binnengewässer, **1**, 255 (1925).

WELCH, P. S. *Limnology,* 2nd ed., rev. New York: McGraw-Hill, 538 (1952).

WHIPPLE, G. C. *The Microscopy of Drinking Water,* 4th ed., rev. New York: Wiley, 586 (1927).

Teaching Ecology in Urban Areas with Emphasis on Insects

ORLANDO PARK

Dr. Park received a Ph.D. degree from Chicago in 1929 and is now professor of biology at Northwestern University. He is a noted ecologist with entomological leanings. He has two major research interests: the analysis of activity of animals and the study of beetles of the family Pselaphidae. *In 1943 he was President of the Ecological Society of America. He was one of the authors of* Principles of Animal Ecology *and is section editor of Animal Ecology in* Biological Abstracts.

AT PRESENT the entomologist can claim the greatest ignorance about the largest natural group of organisms. It is not feasible to discuss the teaching of ecology with the class Insecta suspended in a biological vacuum. Therefore, I shall discuss the teaching of ecology in urban areas and draw on insects for background material.

Teaching ecology in urban areas appears to me to contain no qualitative aspects that would differentiate it from teaching the same subject in rural areas. Being based in a large metropolitan center versus being based in a small rural center places emphasis on suitable means of transportation and amount of time available for actual field work. Given the same level of competence of instruction, amounts of field, laboratory, and library equipment, and the same quality of student material, an instructor offering a field course in an urban center has a longer journey to the place of study and consequently less field time in which to accomplish his objectives. Admittedly this is a great disadvantage but not an insuperable one. It may be likened to sucking water through a straw, instead of drinking from a cup—in the former case one has to work harder, and over a longer period of time to achieve the same result.

During the past thirty years several problems and increasingly acute trends have formed a complicated background for teaching field work in the Chicago area. These include the following: First, constantly expanding urbanization has often resulted in the destruction or impoverishment of formerly excellent study areas. This has meant the abandonment of the area, and the need to find other equivalent study areas or to change the type of field work. Second, constantly expanding urbanization has often required a more efficient method of transportation for class and equipment.

These two items, tied as they are to conservation of field resources, are no less acute now and probably will continue to increase in amount and kind. One example will suffice. In the early nineteen-twenties one could walk from Miller's (now part of Gary), Lake County, Indiana, along the southern shore of Lake Michigan, through Porter County, and into Michigan City, LaPorte County, Indiana, and see only a few people and dwellings. This was an almost unbroken stretch of the famous Indiana Dunes. Here H. C. Cowles studied the dune complex with relation to vegetation, and these experiences assisted in formulating his concept of physiographic succession.[1] This dunes region became one of the better investigated field areas of the world. An incomplete list of contributions would serve to document the thesis that when a brilliant mind formulates an important concept, the resulting contributions in field and laboratory bring substantial enrichment to that science in many ways, some apparently quite unrelated to the initial stimulus.

The Indiana Dunes, as I knew them between 1922 and 1925, are gone. Large sections should have been preserved as an inland biological station offering terrestrial and limnological research facilities. Indiana Dunes State Park was established in the eastern part of this region, but it was established too late and covered too small an area. This Park will serve to conserve certain habitat types, but the strips of raw dune complex from several miles inland to the Lake Michigan strand—the several forest types, fore-dunes, beach drift, open sand, *pannes*, blow-outs, streams, and the excellent series of ponds used by V. E. Shelford in his work on pond

succession—these communities with their diversified biotas have largely disappeared. Marshes have been drained, low areas filled in, and high areas cut down; villages have grown, mansions have been built on sod-stabilized sand, and bathing and recreational facilities have been provided; modern roads now traverse the region, and everywhere are the stigmata of urbanization.

Space has been devoted to the Indiana Dunes for a definite reason. In a discussion of the teaching of field work, direct or indirect ways in which future problems can be attacked or solved should be covered. If field work is to be taught there must be people trained to teach it and localities in which it can be taught. The fate of the Indiana Dunes is an example. In the Chicago area there is also the pollution of such streams as the Des Plaines River, drainage of marshes and of tamarack bogs, and the destruction of remnants of the Prairie Peninsula. The fate of the Indiana Dunes suggests that biologists should impress administrative circles of institutions of learning with the importance of conserving areas for future study. It is not true that teachers in rural places can have access to such areas whereas teachers in urban areas cannot. The former can reach the places more quickly or more conveniently, or they may spend more time there, but if the areas are impoverished, the advantages of the rural teacher disappear.

I should like to discuss the difficulties of teaching ecology in any area, with any group of organisms from algae to Virginia deer. We are all laboring under qualitatively similar disadvantages, which will increase. There was a time when we walked into the field or went by street car to the end of the line and then walked; or by local train and then walked, or by private automobile. Later, groups of private automobiles were used. About the same time the bus, either institution-owned or chartered, came into use. This had the advantage of getting the class and increasing amounts of impedimenta in one private vehicle. This is much the present status of field class transportation. Administrators of higher education have not been won over to the advantages of transporting field classes by airplane, with the institution paying most of the transportation cost.

A third difficulty is the practical restriction of most field work to Saturday or, at most, to over the weekend. Since a student's time is occupied with additional courses during the week, Saturday, with its competing attractions and crowded highways is left. Good field weather may fall on any day of the week, but the field class must take Saturday weather.

Fourth, at times there is what appears to be an administrative handicap on field work. I refer to the matter of credit hours. In some institutions a one-hour lecture is the basis of evaluation, and two hours of laboratory work is the equivalent of a one-hour lecture. Field work may not even be evaluated. On a quarter basis, a course in field biology may be evaluated at four credit hours. Such a course may have two hour lectures a week and laboratory work on collected materials from four to six hours a week. The course also uses from six to eight Saturday hours for from four to seven weeks. Some adjustment of credit hours, or their abandonment, might serve to increase student participation.

Fifth, regional location of the institution offering local field work is a factor that needs only brief mention. In the north, severe winters and late springs restrict most class field work correspondingly. Some regions can offer field work the year around. Institutions located in such areas can give field courses the year around and should do so.

Sixth, there is also the impact of theory and methods on field work. This important aspect deserves our attention. In any good field course the lectures of necessity are in a state of flux from year to year. New ideas require discussion. In a limited series of lecture hours this means an inevitable condensation or excision of other lecture materials.

Similarly, as new ideas are incorporated in field work there is an inevitable condensation or abandonment of other field work, or the expansion of the course into two courses given in different semesters or quarters, or even offered by different instructors. With such changes reorganization of the Saturday schedule is necessary; additional equipment is usually needed; and often new laboratory work is developed as a consequence.

Occasionally some new concept requires a completely new type of field organization; for example, the study by a field class of periodicity of activity.[2] This requires a continuous observation and collecting period of at least twenty-four hours. This extended period of class participation requires camping-out, involving food and clothing, but an all-night-all-day trip is productive, even under poor weather conditions, and students seldom forget the impact of such an adventure. A nocturnal-diurnal analysis of the community, both physical and biotic, can range from simple to complex. It may employ full equipment for quantitative analyses, and in addition, sugaring certain trail stations, tape recorders with parabolic reflectors for picking up bird, amphibian, and insect sonification, and field telephones for interstation communication. On the

other hand, a trip may employ no more than simple observation, with the class following the informed instructor from place to place. Under certain conditions there can be too much organization. We should beware "dry rot" here as in biology in general.[3] We should not neglect natural history.[4]

Finally, there is the matter of identifications. Poor taxonomy may lead to misinterpretation and ridicule. Good field work and good experimental work may be rendered worthless by misidentification. Students should receive expert assistance. Even after a quarter's work students seldom can be trusted to key out miscellaneous arthropods below suborder except in special cases. The instructor should be able to intervene to give the class data reliability. His own background determines the differential reliability.

Students should be encouraged (*a*) to appreciate the difficulty of the taxonomic method, (*b*) to discriminate characters and learn to compose a simple taxonomic key; (*c*) to study the principle of natural variation in a population; (*d*) to study the relation of the ecological factor of selection to the genetic factor of inheritance, and (*e*) to study such infraspecies categories as varieties and subspecies. The library and museum are usually valuable allies.[5]

We have discussed several difficulties that beset the teaching of a field course. The first two are sociological and tend to increase the handicap of teaching in urbanized areas. The third and fourth are largely administrative. The fifth is geographic. The sixth is a natural concomitant of research. The last deals with difficulties imposed by an exacting taxonomic viewpoint. This is not a complete listing; other difficulties will present themselves. Good field work is difficult; it is also important, if proper attitudes toward the scientific method and the conservation of natural resources are to be inculcated.

An appraisal of teaching objectives in field courses covers first the elementary course in field biology and then the advanced course in ecology.

The elementary course in field biology (field botany or zoology) includes:

1. *Principle of variation.* All natural populations vary. This variability in structure, function, heredity, and ecology bears importantly on the life of the individual, its community associates and the evolution of its kind.

2. *The taxonomic method.* After study of variation within a population, the student is more prepared to cope with differences as between allied species. This objective has been discussed previously.

3. *Subspeciation.* The general background should be covered in lectures. Students need an introduction to such things as selection, barriers, and dispersal paths, isolating mechanisms and other concepts since species populations are the building blocks of the community. These are deep and muddy waters even today.

4. *The scientific method.* From the standpoint of first things first, this is the essential item in the equipment of every biologist. Students should know how to set up a controlled experiment and be able to appreciate its philosophical value.

5. *Response to the environment.* (*a*) The varied and often delicate responses to *physical* factors allows an almost endless series of field and laboratory observations to be made. Numerous examples will occur to every one and need no amplification. This work is productive and stimulates student interest. (*b*) Students are less likely to think of responses to the *biotic* environment, although they are keenly aware of this aspect as it affects their own behavior. Feeding patterns and stomach analyses may be illuminating. Laboratory exercises are numerous and include response of leeches to blood-bearing hosts, of clam-mites to clam-gill extract,[6] of myrmecophiles to their hosts;[7] colonies of termites[8] and ants[9] also offer possibilities.

6. *Principle of succession.* This can be introduced at the elementary or the advanced level. The subject has been mentioned previously with respect to the Indiana Dunes and needs no amplification.

7. *Development of the microsere.* There is growing research emphasis on microhabitats and microclimates. In terrestrial microseres insects are important and easily studied. Microsere study can be made in the field and continued in the laboratory. Since most of these microseres eventually are incorporated in organic soil formation and, consequently, are without a climax stage, their study supplements community succession, prepares the student for the complexities of community metabolism, and should increase interest in soil conservation.

Such terrestrial microseres include those of carrion, dung,[10] fungus conk,[11] tree hole,[12] and the decaying log.[13, 14, 15]

The advanced course in ecology includes:[16]

1. *Principle of succession.* Noted previously.

2. *Principle of convergence of seres.* Convergence to a regional climax is an important concept but can best be handled by lectures and assigned reading, unless the institution involved is prepared to undertake much more than the usual responsibility for financing field trips.

3. *The population.* Growing emphasis on nat-

ural and experimental population ecology at the research level is a happy sign of progress. Until we know much more about populations it will be difficult to raise our sights to the community, where many interdependent populations are involved in survival.

4. *The community.* This is one of the most difficult subjects. It involves so many different kinds of things, operating differentially or jointly through time and space that the subject could occupy the full time of a course. Several aspects can be mentioned here, but the danger in this method is that the aspects may become identified as independent, with consequent loss of synthesis.

(*a*) *Mensuration of physical factors.* The daily march of the several operating factors should not be neglected in so far as they may interpret or augment current study, and if such data are accumulated for those parts of the community under investigation. Practical meteorology is not an ecological end, but should be correlated with ecological events in the lives of individuals, populations, or the whole community.

(*b*) *Community metabolism.* The student gains perspective in the realization that there is an ecologically similar web of feeding interrelations within any major community type the world over. Direct field observations often provide evidence of the behavior of herbivores, predators, and parasites. Stomach analyses augment field work; lectures and readings broaden the viewpoint.

It is at this last level that the student can be interested in trophic levels, pyramid of numbers, biomass, and productivity. It should serve also to integrate group appreciation of the group's own food web, the problems of conservation, and the dangers involved in unscientific efforts to control populations.[17]

(*c*) *Community structure.* The principle of stratification in the broadest sense usually can be demonstrated with success. Associated with this are important features of community life as the structural and behavioral adjustments of organisms to a given stratum, to habitat niches and their microclimates.

(*d*) *Community periodism.* This has been discussed previously.

(*e*) *The biome.* Distribution of biomes over the world, their climates, biotas, and adjustments, is a stimulating subject but of such magnitude that it is usually developed in a separate course. From a field viewpoint it is seldom practicable to demonstrate several biomes within a few Saturdays of a quarter or semester. Until a class can be taken into the field for some extended period of time, or until cabin airplanes can be utilized, such concepts as the comparative study of biomes must be left to lectures and assigned readings.

References

1. Cowles, H. C. *Botan. Gaz.*, **27**, 95 (1899).
2. Park, O., and Strohecker, H. F. *Ohio J. Sci.*, **36**, 46 (1936).
3. Wheeler, W. M. *Foibles of Insects and Men.* New York: Knopf (1928).
4. Schmidt, K. P. *Chicago Naturalist*, 6, 81 (1943).
5. Schmidt, K. P. *Museum News* **30**, 5 (1952).
6. Welsh, J. H. *Biol. Bull.*, **61**, 165 (1930).
7. Park, O. *Ann. Entomol. Soc. Am.*, **25**, 77 (1932).
8. Emerson, A. E. *Sci. Monthly*, **64**, 337 (1947).
9. Michener, C. D., and Michener, Mary H. *American Social Insects.* New York: Van Nostrand (1951).
10. Mohr, C. O. *Ecol. Monographs*, **13**, 275 (1943).
11. Park, O. *Ecology*, **12**, 188 (1931).
12. Park, O., Auerbach, S., and Corley, Glenna, *Bull. Chicago Acad. Sci.*, **9**, 19 (1950).
13. Shelford, V. E. 1913. *Bull. Geog. Soc. Chicago* **5**, 1 (1913).
14. Blackman, M. W., and Stage, H. H. *Bull. N. Y. State Coll. Forestry*, **24**, 1 (1924).
15. Savely, H. E. *Ecol. Monographs*, **9**, 321 (1939).
16. Allee, W. C. Emerson, A. E., Park, O. Park, T., and Schmidt, K. P. *Principles of Animal Ecology.* Philadelphia: Saunders. (1949).
17. Leopold, A. *Wisconsin Con. Dept. Publ.*, **321**, 1 (1943).

SCIENCE ON THE MARCH

RECENT PALEONTOLOGICAL DISCOVERIES FROM CUMBERLAND BONE CAVE

MANY European caves are noted for their rich supplies of fossils, but very few caves containing the remains of extinct animals are to be found in the United States. Except for deposits in Florida, however, our knowledge of Pleistocene remains of eastern North America is limited to caves. Perhaps the most outstanding source of our knowledge of Pleistocene fauna has been the Cumberland Bone Cave, in Maryland, yet today this cave is not well known at all. This is unfortunate, since not only do the remains found in the cave fill many gaps in the paleontological record of the eastern United States, but they also tell us much about the climate and topography of bygone ages. Even more information should now be forthcoming from this site since we have reopened a section hitherto considered inaccessible.

Before 1912, the entrance to the cave was apparently a sinkhole situated on a limestone ridge on the south side of Wills Creek Valley about three miles northwest of Cumberland, Maryland. The town of Corrigansville is now situated below this ridge. A vertical shaft led down to several small chambers and thence down to the main room of the cave, approximately 100 feet below the surface. It is easy to see how this shaft developed, since the strata of limestone are vertical, so that the lines of cleavage are in a nearly perpendicular position. This gave access to surface waters, which then caused the development of a typical fissure cave.

The cave must have been known to local residents before 1912; remains of old rifles were found in it and, according to one source, it was a hideout for early settlers during Indian raids. In 1912 the Western Maryland Railroad, in excavating for a cut while pushing their tracks through Cash Valley to Connellsville, exposed the cave. Unfortunately, dynamite was used to loosen the layers of rocks down to the level of the roadbed, which was at the same level as the main room of the cave. An alert amateur naturalist noticed the large quantities of bones being removed in the rubble and notified J. W. Gidley of the United States National Museum. All the bones exhumed before this time were lost except for a few skulls carried home by curious workmen. For the next four years Gidley accumulated from this cave one of the largest collections of mammalian fossils ever found in this section of the country. After Gidley's death in 1931, C. Lewis Gazin, curator of vertebrate paleontology at the museum, completed the classification of the remains and is at present assisting us in identifying the fossils discovered within the past two years.

As originally found, there were literally thousands of fragments of bones, intermingled with the dirt and breccia of the cave and in many cases cemented together so as to make identification difficult. Although broken up, the bones show no sign of being water-worn, and there is an absence of sand or gravel; the bones must have been intermixed as a result of carcasses being caught in crevices near the surface and gradually falling apart until the disjointed bones eventually worked their way down to the bottom of the cave. The remains range in size from bats to mastodons, and only the bats, of all the specimens described, could be called true cave animals.

The Cumberland Cave fauna exceeds that of other Pleistocene caves in the number of mammalian genera recorded, but it contains a slightly smaller number of species than found in the Port Kennedy deposit or the Conrad Fissure. Forty-five distinct species of mammals, in seven different orders, are represented in the remains, plus remains of a few reptiles, some snakes, and a lone alligator tooth. Of these species, twenty-eight are now extinct, and most of the others are not present-day natives of the region.

The accumulation of bones must have been gradual, although all the mammals are pre-Wisconsin in age. The diversity of type indicates that widely varying climate zones must have existed during the time of deposition. This has led to much speculation and has given evidence of more radical changes in environmental conditions than had been originally suspected. In this one cave have been found such types as the wolverine, grizzly bear, and Mustelidae, which are native to Arctic regions. Peccaries, the most numerous type represented, tapirs, and an antelope possibly related to the present-day eland are indigenous to tropical regions. Ground hogs, rabbits, coyotes, and hare remains are indicative of dry prairie, but on the other hand such water-loving animals as beaver and muskrat suggest a more humid condition.

Several explanations are possible for these puzzling differences of species. It may be that animals of species existing now differ in habits from ani-

mals of the same species existing then. This may seem improbable, but we have no proof that animals have maintained the same type of behavior and reactions through the years. Another argument advanced is that the temperature of the region when the bones were being deposited was warmer than now, except in the mountain ranges, which have since been worn down by erosion. This argument seems untenable, for, geologically speaking, the remains are recent and the mountains must have had the same general size range as those of today. Further, it is known that Wills Creek and the nearby Potomac River were higher than they are now, so that the entrance to the cave must have been almost level with the valley floor and there was much less relief than exists today. However, animals adapted to the cold could have penetrated the warmer valleys and lowlands during the winter.

The ice sheet might have been advancing so rapidly that species of different environments could exist contemporaneously in the same region. This condition could not have existed for long, however, and the tropical type would either have succumbed or fled south. Gidley regarded the African types as relics of a former Holarctic distribution.

The most logical explanation would seem to be that the remains accumulated over a sufficient period of time to allow for gradual change in climate from one extreme to the other. If a gradual time range of fifty thousand years is postulated for the length of time necessary to accomplish this, then the diversity of bones can easily be accounted for and still place the remains in the period usually assigned to them.

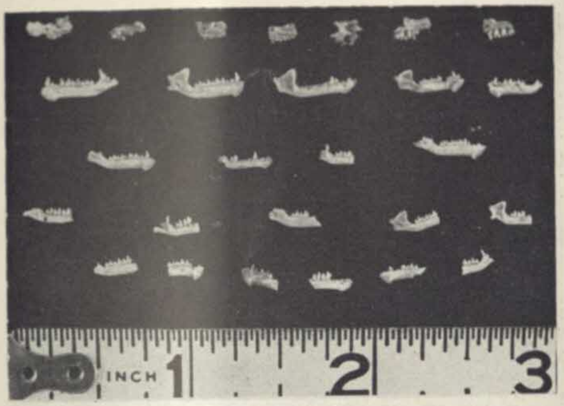
View of excavation overlooking original Bone Cave. Author standing on right.

As at present understood the cave remains represent the following vertebrates; the common name or name of an animal similar to that listed is given in parenthesis:

Loricata
  Crocodylid (crocodile)
Serpentes
  Ophidian remains (snake)
Galliformes
Tetraonidae
  Bonsa umbellus (*linnaeus*) (ruffed grouse)
Insectivora
Soricidae (Shrew)
  Sorex sp. (long-tailed shrew)
  Blarina brevicauda (Say) (short-tailed shrew)
Chiroptera
Vespertilionidae
  Eptesicus cf. grandis (Brown) (brown bat)
  Corynorhinus alleganiensis, n. sp. (big-eared bat)

View of trench being dug to cave.

Bone Cave open. Note stalactites on left.



Jaws of extinct bats range in size from 1/8 to 1/2 inch.

Carnivora
- Canidae
  - *Canis cr. priscolatrans* (Cope) (coyote)
  - *Canis armbrusteri* (Gidley) (coyote)
- Ursidae
  - *Eurarctos vitabilis* (Gidley) (black bear)
  - *Arctodus haplodon* (Cope) (grizzly bear)
- Mustelidae
  - *Martes parapennanti*, n. sp. (marten)
  - *Mustela cf. vison* Schreber (mink)
  - *Gulo gidley* (Hall) (wolverine)
  - *Lutra parvicuspis*, n. sp. (otter)
  - *Brachyprotoma pristina* (Brown) (skunk)
  - *Spilogale marylandensis*, n. sp. (spotted skunk)
  - *Taxidea marylandica*, n. sp. (badger)
- Felidae
  - *Felis cf. inexpectata* (Cope) (puma)
  - *Felis near strox* Leidy (tiger)

Rodentia
- Sciuridae
  - *Marmota monax* (Linnaeus) (woodchuck)
  - *Citellus cf. tridecemlinaetus* (Mitchell) (ground squirrel)
  - *Tamias cf. striatus* (Linnaeus) (chipmunk)
  - *Sciurus tenuidens* (Hay) (red squirrel)
  - *Glaucomys* sp. (flying squirrel)

Fossilized remains of prehistoric elk.

- Geomyidae
  - *Plesiothomomys potomacensis*, n. gen. and sp. (pocket gopher)
- Castoridae
  - *Castor canadensis* (Kuhl) (beaver)
- Cricetidae
  - *Peromyscus cf. leucopus* (Rafinesque) (deer mouse)
  - *Neotoma magister* (Baird) (wood rat)
  - *Parahodomys spelaeus*, n. gen. and sp. (wood rat (?))
  - *Synaptomys cf. cooperi* (Baird) (Lemming mouse)
  - *Synaptomys* (*Mictomys*) sp. (Lemming mouse)
  - *Microtus* (*or Pitymys*) (?) *cf. involutus* (Cope) (volve, field mouse)
  - *Ondatra cf. annectens* (Brown) (muskrat)
- Zapodidae
  - *Zapus* sp.
  - *Napaeozapus cf. insignis* (Miller) (jumping mouse)
- Erethizontidae
  - *Erethixon cf. dorastum* (Linnaeus) (porcupine)

Lagomorpha
- Ochotonidae
  - *Ochotona* sp. (pika)
- Leporidae
  - *Lepus cf. americanus* (Erxleben) (hare)

Proboscida
- Mostodontidae
  - *Mammut cf. Americanum* (Kerr) (mastodon)

Perissodactyla
- Equidae
  - *Equus sp.* (horse)
- Tapiridae
  - *Tapirus* sp. (tapir)

Artiodactyla
- Tayassuidae
  - *Platygonus cumberlandensis* (Gidley) (peccary)
  - *Platygonus vetus* (?) (Leidy) (peccary)
  - *Mylohyus exortivus* (Gidley) (peccary)
  - *Mylohyus cf. pennsylvanicus* (Leidy) (peccary)
- Cervidae
  - *Cervus* sp. (elk)
  - *Ococoileus cf. virginianus* (Boddaert) (white-tailed deer)
- Bovidae
  - *Euceratherium* (?) *americanum* (Gidley) (eland)

Of the fossils found in the cave, seven are new species. One, a bat of the Vespertileonidae family, *Corynorhinus alleganiensis,* which resembles the big-eared bat, is the first record of this genus from the Pleistocene. Even living representatives of Corynorhinus have never been recorded in this region. Oddly enough, no trace has been found of *Myotis* in the Cumberland Bone Cave. *Myotis* was originally listed as having been found there because the dental formula of the lower jaw of Corynorhinus is the same as *Myotis*. A later study revealed, however, that the mandible found had a shorter premolar and longer molar teeth than *Myotis*.

Four of the new species are Mustelidae. The first, *Martes parapennati,* a marten, is similar to *Martes diluviana* from the Port Kennedy deposit. *Lutra parvicuspis,* the second of the species, resembles a type of otter found in Central and South America

today. It is smaller than the other found in the eastern United States, *Lutra canadensis*. Another member of this species has also been found in Port Kennedy.

A third mustelid, *Spilogale marylandensis*, or spotted skunk, is represented by three fragments of mandibles. The complete skull, lower jaw, eleven vertebrae, and a section of the right humerus of a badger, *Taxidea marylandica*, represent the last new species of this family. Since very few badgers are now seen east of the Mississippi, this is an interesting find. It is similar in size to *Meles meles*, but only fragments are known of two other species of Pleistocene Taxidea.

Two rodents, both representing new genera and species, have been identified. The first, *Pleisiothomomys potomacensis* is a member of the Geomyidae, or pocket gopher family. It differs, however, from two other genera of this family, *Thomomys* and *Geomys*. The other rodent, a wood rat, *Parahodomys spelaeus*, belongs to the Cricetidae.

Another outstanding group from the cave has been the remains of thirty or more peccaries, from which the complete skeleton of one individual has been reconstructed. Two genera, *Platygonus* and *Mylohyus*, have been encountered, with two species of each. The mastodon remains seem to be those of young individuals.

Undoubtedly many more bones were still embedded in the cave when loose rocks finally made any further investigation too precarious. The Western Maryland Railroad in 1915 blocked the entrance so successfully that all attempts to get into the remains of the cave were a failure. It should be noted here that the railroad did a wise thing in closing the cave, since it had become rather popular and there was the imminent danger of tourists being seriously hurt either by falling rocks or by passing trains. A group of us once spent two days working at the blocked entrance and were able to advance only ten feet. Further work on that section of the cave is now impossible. However, elderly local residents have stated that the cave extended underneath the hill for some distance. The whole area about the cavern was carefully traversed, but no evidence of other openings was found, with the exception of a large sinkhole one mile west of the former opening. The owner of the orchard on which the sink is located claims that water drains out of the bottom. This hole, extending down about twenty feet below the surface, was carefully investigated, but no traces of a passageway could be found. It does appear that the hole is gradually widening, and it may be that this is a "back door" to the cave.

Remains of peccary taken from Cumberland Bone Cave.

The only other clue to the fact that the cave underlies the hill was found in an abandoned quarry two miles south of the present entrance. Several small openings pierce the wall of the quarry and then connect inside to continue as a winding passage for forty feet. Broken rock and dirt prevent further progress, but the passage seems to have collapsed only recently, probably because of the blasting operations of the quarry. If the passage continued in the same direction, it would connect with the Cumberland Bone Cave, since it is situated in the same hill.

After several visits to the site in the autumn of

Left and right mandibles of extinct bear, *Euarctus vitabilis*.

1950, it was noted that numerous weathered stalactites, stalagmites, and flow stone could be discerned on the exposed sides of the cuts. This indicated the former location of the cave, and from this we were able to deduce the general pattern of the original cave. It seemed that at least one of the smaller chambers should still exist on the opposite wall. A small opening was made about thirty-five feet above the tracks. Then followed a memorable moment. The first member of the party had no sooner squeezed through the opening than he found a bone stuck in a crevice. Upon examination it was identified by Dr. Gazin as the complete mandible of *Euarctus vitabilis,* a large bear now extinct. This was a good indication that a portion of the original drop had been found.

Further exploration of this room revealed that it was about twenty feet deep, ten feet wide, and two feet high. Careful digging operations were started in the dirt floor, and other bones soon came to light. An immediate problem arose as to the disposal of the dirt. Conditions were crowded, and the narrow entrance made shoveling of dirt impossible.

A second opening was then dug, connecting to the remnants of the chimneylike formation. This afforded an entrance into the actual site of most of the findings. As a result of this, dirt and rocks were gradually shoved toward the front entrance while access was obtained through the top of the chamber. By 1951, parts of the room had been excavated to a depth of two feet; work progressed slowly since each shovelful of dirt had to be sifted. After getting below the first foot, bones turned up with an encouraging frequency. Some of these were obviously recent, but many were heavy with mineral impregnation.

In 1952 operations had to be halted as there was danger of undermining the whole cliff, since the cave was found to extend through to a quarry. In addition, ventilation was so poor that respirators had to be worn, and goggles were needed as a result of the dust raised. However, through the interest shown by George Howarth, vice president and general manager of the Western Maryland Railroad, an agreement was reached whereby the railroad promised to remove the upper strata of rock down to the cave level, thus removing the danger of collapse, also permitting the opening of the cave to light, and thus solving the ventilation problem.

In December 1952 work resumed with the removal of several hundred cubic yards of rock to reach the floor of the cave. Then the walls were carefully removed, the debris being constantly searched for bones. Because so many remains were found lodged in crevices, dynamite had to be used sparingly to preserve the fossils. This meant that more time was required to complete the job, but the Western Maryland has generously continued to support the expense of opening the cave until the job is completed. The excavated rock and dirt are removed by placing a work train in the cut below the cave and scooping up the material with a railroad crane.

Actually, there is slight resemblance to a cave now. Rather, there is a cut forty feet deep, twenty feet wide, and fifty feet long at the bottom of which lies the dirt yet to be investigated. It is hoped that a solid floor will eventually be found under the dirt. Here there are likely to be crevices and joints where large bones would be caught. Some bones may be encased in the rocks, as was the situation in the original bone cave.

In conjunction with work being done on this cave, other caverns in the region have been investigated by us. Since September 1950, twenty-three new caves have been found within three miles of Cumberland. These and known caves of the territory have not yet yielded any remains that have been definitely established as being fossils, although a fair number of extinct species have been found. The only fragment which may be considered of interest is the top half of a femur of some sort of bison. This was discovered several years ago by a group while searching an unreported cave near Harpers Ferry, West Virginia.

Brother G. Nicholas

La Salle High School
Cumberland, Maryland

THE ONION SET INDUSTRY OF SOUTH HOLLAND, ILLINOIS*

SEED production is a highly specialized form of agriculture, requiring the use of special skills and environmental conditions. Unique industries have resulted from this fact, especially in the culture of bulbs used as seed. A well-known industry of this type is found in and around Holland, Michigan, the tulip center of the nation. Another bulb production center not so well known, but of national importance in horticultural circles, is located at South Holland, Illinois. Known locally as the South Holland area, this production center is noted for the growing of onion sets.

The South Holland area, less than 10,000 acres in size, produces more than half, and markets virtually all, the commercial onion set crop of the United States. The area is 20–30 miles directly south of Chicago, in Cook County, and is adjacent to the Illinois-Indiana state boundary (Fig. 1). Of the five hundred farm operators in Thornton and Bloom townships, in which the South Holland area is situated, more than 40 per cent raise onion sets and 94 per cent of the set growers are of Dutch origin. The concentrated production within fifteen square miles of between 500,000 and 1,000,000 bushels of onion sets yearly is an interesting example of agricultural specialization and concentration.

The Onion Set Industry of the United States

A general understanding of the nature and distribution of the industry within the United States is necessary to a consideration of the characteristics of the South Holland area.

Set growing is the second stage in a three-year cycle from seed to consumer. Seed is produced the first year from onion bulbs planted for that purpose, and a portion of the seed is then purchased by set growers in various parts of the country. The second year the seed intended for sets is planted thickly, about fifty pounds to an acre. This method stunts the growth of the plants and provides the grower with the small mature bulbs called onion sets. These are in turn planted the following year by home gardeners, truck farmers, and commercial onion growers.

Although a comparatively small industry, the commercial onion set industry grosses about one million dollars annually. Only 5 per cent of the total commercial onion crop in the United States are grown from sets. Approximately 3 per cent of these sets are grown by the onion producer for his own consumption, and so would not be classified as commercial sets. Almost all commercial onions are grown either from transplants or directly from seeds. The greatest buyer of commercial sets is the home gardener, who purchases them by the pound, quart, or bag, from seed, hardware, grocery, or drug stores.

Commercial onion sets are raised in only a few small areas of the United States: in the west, Greeley, Colorado, and Corvallis, Oregon, produce a small percentage. The Toledo, Ohio, and Louisville, Kentucky, areas, although significant producers in the past, are of little importance today. Minnesota has recently produced some sets, though its contribution has been extremely irregular. Racine and Kenosha counties, Wisconsin; Lake County, Indiana; and Cook County, Illinois, commonly referred to in the industry as the lower Lake Michigan region, produce more than 95 per cent of the total crop.

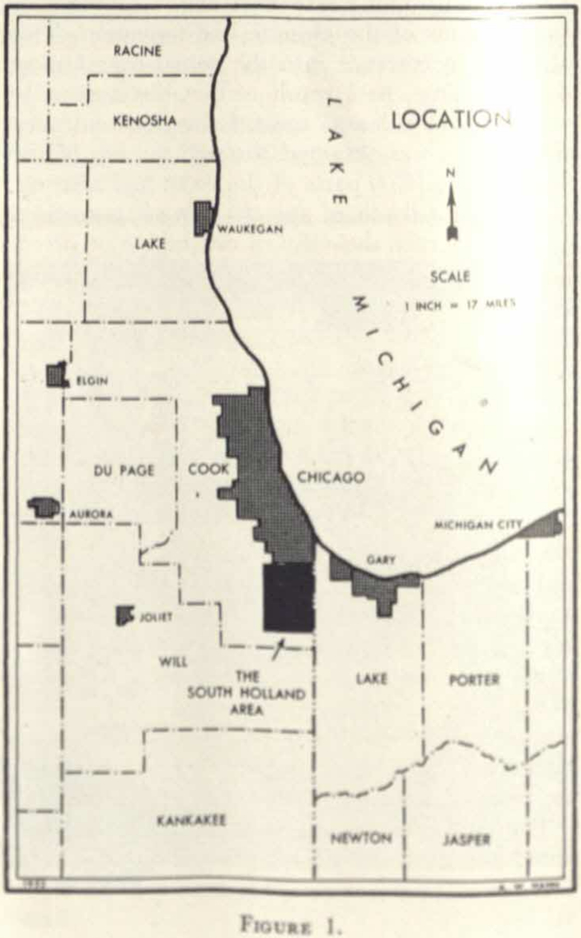


FIGURE 1.

* Data pertaining to this industry were obtained chiefly through personal interviews made within the area by the author during the summers of 1951 and 1952.

FIG. 2. Portion of the level lake plain near South Holland, Illinois. Note stacks of onion set crates.

South Holland is primarily an area of Dutch settlement and Dutch-dominated agriculture and industry. Outside of the Dutch the population is composed mainly of people of German descent. Of the eight shipping agencies in the area only one is not directed by Hollanders.

Onion sets are the largest individual crop in the area, but other crops are important from both an economic and an acreage standpoint. The South Holland area embraces approximately 7800 acres of agricultural land within two townships, and 30 per cent, or 2408 acres, is planted in onion sets. The remaining twenty-two townships of Cook County produce a combined total of less than 1000 acres. About 65 per cent of the agricultural land within the area is devoted to crops other than onion sets, although no single crop occupies more than half as much land as that occupied by sets. Tomatoes and sugar beets utilize 15 per cent and 11 per cent of the area, respectively, and are the two most important vegetable crops. Dry onions and asparagus are also notable from an acreage standpoint; the remaining vegetable crops are of minor importance. Among the general farm crops, acreage in corn is as important as oats, soybeans, alfalfa, and wheat combined. Agricultural land not used as cropland may be classified under two categories—wasteland and woodlot. These land-use types occupy about 5 per cent of the total.

The physical environment is generally well suited for the growth of high-quality onion sets. The moderating influence of cool winds off Lake Michigan, especially in late spring and early summer, helps to make temperature conditions almost ideal. Precipitation is adequate, although convectional storms, especially in the middle and late summer frequently create drainage problems already made difficult by the low flat surface of the lake plain occupied by most growers (Fig. 2).

The silty clay loam and silt loam soils, 12–18 inches thick, which cover most of the land used for set raising, are unusually fertile and generally well suited for sets. The Dutch farmer, however, through his tenacious efforts in draining these soils must receive primary credit for their present suitability.

The onion set industry entails a year-round operation, in which mechanization now plays an increasingly important part. Locally constructed seeders, cultivators, harvesters, and processing equipment have greatly increased efficiency in production and considerably lessened dependence upon manual labor, although it is still an important factor in most operations (Fig. 3).

There is evidence of a varying intensity of labor utilized on farms of different sizes. On farms of less than ten acres, there is one laborer for every two or three acres of cropland. On farms larger than one hundred acres, there is one laborer for every nine or ten acres of land. On the average, the ratio is approximately one laborer to every seven or eight acres of land. With the increase in size of farms, therefore, the amount of land tended by one laborer increases, or the number of laborers per given amount of acreage decreases. Doubtless the more intensive farming on the smaller farms on the

one hand, and the greater utilization of mechanized equipment by the larger operators on the other, explain this condition, at least in part.

Both yields and prices of onion sets have been unstable throughout the history of set growing in South Holland. The profits of high yields are usually offset by corresponding lower prices of sets, and in years when per acre production is low prices may be contrastingly high. However, when comparing average yields and average prices of the past decade with the two decades prior to 1940, two significant facts may be noted: (1) Average yields of the past ten years have increased by 18 per cent over the 1921–30 period and by 29 per cent over the years 1931–40. (2) Average prices of the past ten years, excluding World War II, have decreased by 4 per cent over the 1921–30 period and 11 per cent over the years 1931–40. Thus it is noted that the increase in yield of the past decade was from two to three times as great as the decrease in prices. Acreage has remained constant except for the World War II period.

The average annual gross income to growers in the South Holland area during the past thirty years has been approximately $750,000. During the past decade, excluding World War II, about $854,000 was received annually, and in the periods 1921–30 and 1931–40 approximately $720,000 and $666,000 were derived, respectively. Thus, despite declining prices, increases in yield have produced a total income during the past decade which is substantially higher than either of the other two ten-year periods.

Total capital investment in the South Holland onion set industry is estimated to be approximately $2,500,000, about one half of which is in mechanized equipment. More than $350,000 is spent on set harvesters alone. The remaining portion of capital expenditures is used for such things as warehouses and onion set crates.

About two thirds, or approximately 750,000–1,000,000 bushels of the sets marketed annually by the local agencies are grown in the South Holland area, and the remaining one third is grown in other portions of northern Illinois and southern Wisconsin. About 60 per cent of the sets sent from South Holland are shipped by rail, although truck transportation is gaining in importance here as elsewhere.

FIG. 3. Hollanders using onion set grading equipment.

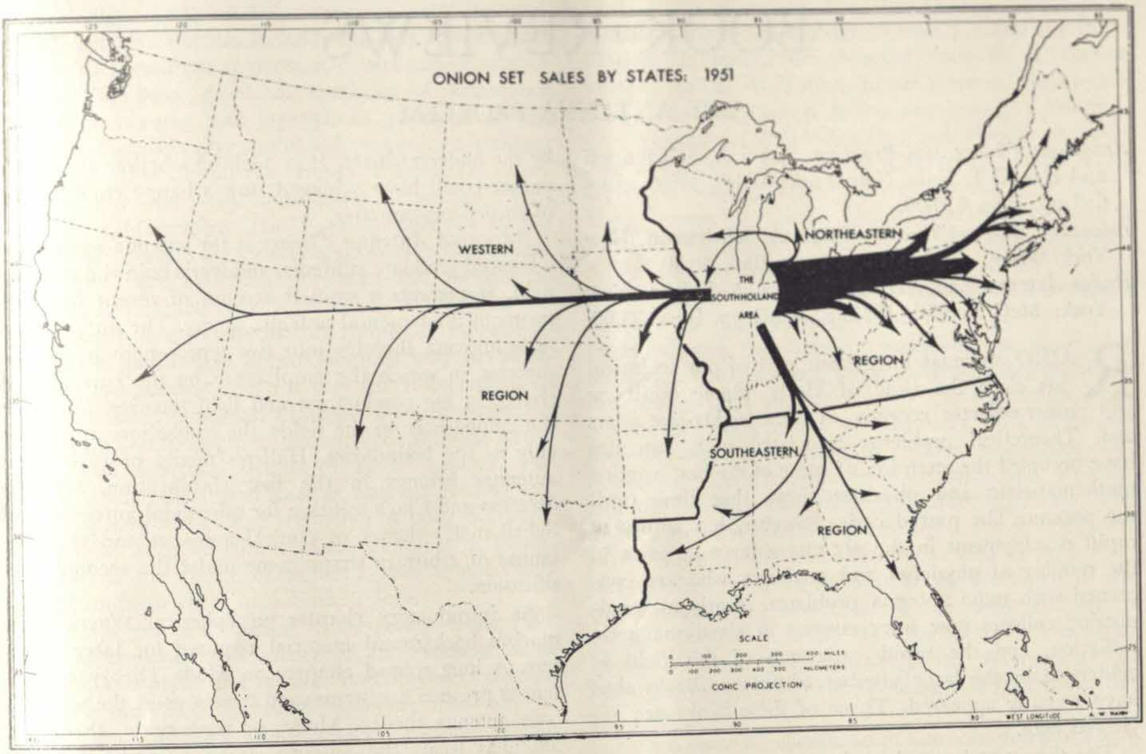


FIGURE 4.

Onion sets are distributed by South Holland shippers to at least forty-one states, which may be divided into three regions, commonly used by South Holland dealers in referring to the distribution of their product (Fig. 4).

As might be anticipated, the northeastern region serves as the purchaser of most of the sets marketed from South Holland. This region contains approximately 53 per cent of the total population of the United States, but it serves as a consumer for about 71 per cent of the total South Holland shipments made by rail. The southeastern region shows the second most important consumer concentration. Here 15 per cent of the total United States population purchases about 12 per cent of the sets marketed from South Holland by railroad. In this region the percentage of sets sold might be somewhat higher if the destinations of shipments by truck were known. South Holland set consumption, when compared with population concentration, is of least significance in the western region. Here 32 per cent of the total population of the country purchases only 17 per cent of South Holland sets.

The outlook for the onion set industry of the South Holland area is good, although it warrants constant observation. The national reputation of South Holland for high-quality sets should continue to grow as prospective increases in demand are met with continued high-quality produce. Despite lower prices, it is assumed that continued research and accumulated capital investments, especially in equipment, should further develop efficiency in operations so that continued satisfactory profits may be obtained. Slight shifts in distribution of consuming areas should in no way affect the currently favorable position held by the South Holland area compared with that of other producing areas, but might serve to encourage even greater production stability within the local area than heretofore. The physical environment is almost ideal at present, and its utility may improve as new and better methods of fertilization are more universally accepted and applied. Skills, techniques, interest, and devotion related to this type of agriculture, either transmitted or developed by the present-day Dutch farmer, have contributed significantly to the success of the industry up to this time. They may be expected to be passed on to the next generation, the Dutch farmer of tomorrow. These qualities will add to the prospects for a good and stable future within the South Holland onion set industry.

ARVIN W. HAHN

Department of Geography
Concordia Teachers College

BOOK REVIEWS

THE ANTENNA PROBLEM

Antennas: Theory and Practice. Sergei A. Schelkunoff and Harald T. Friis. New York: Wiley, 1952. xxii + 639 pp. Illus. $10.00.

Advanced Antenna Theory. Sergei A. Schelkunoff. New York: Wiley, 1952. xii + 216 pp. Illus. $6.50.

Radio Antenna Engineering. Edmund A. LaPort. New York: McGraw-Hill, 1952. xii + 563 pp. Illus. $9.00.

RADIO antennas have been part of our technical art since the time of Hertz' dipole oscillator and corner-reflector receiver of some eighty-five years ago. Theoretical problems associated with antennas have occupied the attention of some of the best applied mathematicians and physicists from that time until the present. The past decade, however, has witnessed rapid developments in the art and a large increase in the number of physicists and engineers who are concerned with radio antenna problems. Nearly all engineering colleges now offer courses in electromagnetic radiation, and the rapid expansion of this field is evidenced by the large number of antenna books that have recently appeared. Three of these books are reviewed here.

The theory and practice of antennas are covered by two well-known members of the Bell Telephone Laboratories, at a level suitable for advanced undergraduates and beginning graduate students. Following an introductory chapter which surveys the material to be discussed, Maxwell's equations are introduced (without use of vector analysis) and applied to plane wave propagation. A chapter on spherical waves, both in free space and on diverging wires, provides a lucid introduction to radiation phenomena. Three chapters on the directional properties of antennas and arrays include methods of antenna synthesis and a discussion of superdirective arrays.

A chapter on antenna current gives a thorough and understandable analysis of the factors that affect antenna current distribution. The clarity of the treatment here, as throughout the book, reflects the authors' long experience and their deep understanding of the subject. Impedance as a function of a complex variable is treated, and then the impedance characteristics of antennas are covered. The last six chapters dealing with antenna practice give an excellent and authoritative summary of the present state of the art.

The engineer, the teacher, and the student will all find this book a delight to read. For here the complicated phenomena of electromagnetic radiation are presented with rare simplicity. The book is comprehensive and thorough and contains a large amount of new and original material. It is unusual also to find so much high-level quantitative discussion presented without the use of mathematics beyond what is normally acquired by the undergraduate. It is doubtful whether any single author could have achieved such a happy combination of theory and practice.

Advanced Antenna Theory is for antenna specialists, advanced graduate students, and workers in the antenna field. It presents a unified account of recent developments in fundamental antenna theory. The author classifies antenna theories into two types—namely, circuit theories, in which the emphasis is on the current and charge in the conductors; and field theories, in which the emphasis is on the fields, the conductors appearing only as the boundaries. Hallen's theory of cylindrical antennas belongs in the first classification, whereas Stratton and Chu's solution for spheroidal antennas and Schelkunoff's theory of conical antennas and thin antennas of arbitrary shape come under the second classification.

An introductory chapter on Spherical Waves summarizes background material required for later chapters. A long second chapter on Mode Theory of Antennas presents a systematized treatment of the author's own antenna theory. Mode, or wave-guide, theory of antennas treats the antenna as an opened-out waveguide capable of supporting many modes, each of which may be described by a conventional transmission line diagram. In contrast to mode theory, the resonator theory of antennas is developed in Chapter 3 in connection with the solution for spheroidal antennas. A chapter on integral equations prepares the way for a comprehensive treatment of Hallen's method of solution for cylindrical antennas. A final chapter on natural oscillations, and several appended tables containing information of value for solving numerical problems, conclude the book.

Most of the material presented is new. In the words of the author, "to obtain the most general solution of Maxwell's equations is easy; to find that particular solution which satisfies the various supplementary conditions is far more difficult." One of the unusual features of this advanced text is the number of examples of particular antenna configurations that are set up for solution, mostly by the method of spherical mode analysis. Schelkunoff does more than set forth antenna theory. He stresses the physical interpretation of the mathematical methods employed. This approach is one that the engineer and physicist find most satisfying because the insight so gained assists them in their job, which is "the mathematical formulation of physical problems in a manner that will lead to correct results." Without doubt this book will become a classic in the field of electromagnetic engineering.

LaPort, chief engineer of the RCA International Division, has written an antenna design book, based upon the knowledge and experience gained from thirty

years of engineering on low-, medium-, and high-frequency antenna systems. In addition to a long chapter on each of these three subjects, it covers radiofrequency transmission lines, graphical synthesis of impedance matching networks, and logarithmic potential theory. Several appendices give useful information on penetration depth of earth current, mutual impedance, radiation patterns, and world noise zones, as well as a very extensive bibliography. The book is different in the respect that besides the radiation properties of antennas it considers the matching networks and mechanical engineering for each type. Although it is true that antenna theory is the same for all frequency ranges (although the permissible approximations may be different), the mechanical design is very much dependent on frequency. Besides several hundred line drawings, the book has about 55 pages of photographs showing installations and structural details of actual systems. The treatment of transmission lines as antenna feeding and matching networks at high frequency is quite thorough. Material on such important practical topics as factors involved in the choice of antenna sites is not likely to be found elsewhere in published form.

This is a practical "recipe" book giving detailed design and construction information on antennas made of wires, and masts and towers. It is intended particularly for field and operating engineers who may be called upon to select and construct antenna systems for use at frequencies below 30 mc.

It would be expected that there might be considerable overlap of subject material among three books dealing with antennas. Such is not the case, however, and each of these books deserves its own place on the bookshelf of the engineer or physicist who is concerned with antennas. In addition, Schelkunoff's advanced text is to be highly recommended to the applied mathematician who is seeking problems to test his ingenuity and skill.

E. C. JORDAN

Department of Electrical Engineering
University of Illinois, Urbana

BRYOZOA

The Tertiary Cheilostomatous Polyzoa of New Zealand. David Alexander Brown. London: British Museum (Natural History), 1952. xii + 405 pp. Illus. £4 10*s.*

DR. BROWN's study was published as a monograph, independent of the preceding five volumes grouped under the title of *Catalogue of the Fossil Bryozoa,* issued from the Geological Department of the British Museum (Natural History). He elected to use Thompson's word "Polyzoa" in preference to Ehrenberg's "Bryozoa." The holotypes, some paratypes, and other specimens figured by Dr. Brown are deposited in the museum.

In discussing the previously published papers that included Tertiary Bryozoa from New Zealand, Brown calls attention to the inadequacy of the descriptions, and especially of the illustrations. His monograph includes a restudy of the collections of T. Hincks, G. R. Vine, A. W. Waters, E. C. Jelly, J. W. Gregory, J. E. Tenison-Woods, Bracebridge Wilson, F. Canu and R. S. Bassler, G. Busk, P. H. MacGillivray, W. H. Cawne Warren, and F. W. Hutton, to the extent of their availability. He also studied collections from (1) Waipipi, (2) Wanganui, (3) Hawkes Bay, (4) Mangamak Horizon, Hunterville, (5) North Canterbury, Weka Pass, Dovedale Stream, Waipara River Crossing, (6) North-West Nelson, (7) North Otaga, MacDonalds Quarry, Oamaru, Campbells Beach, All Day Bay, Otepopo, S. D., and (8) Southland.

Some of the collections were not localized in detail; hence their full stratigraphic values were lost. Other faunules were incompletely sampled, but those that were excellently secured serve as dependable guides to the ecology and stratigraphy of the formations from which they were taken. Dr. Brown's work illustrates how Bryozoa can be used in making correlations and ecological interpretations as dependable and accurate as those based upon any other group of organisms. The accuracy of the results depends upon the thoroughness in the study of the microdetails, as well as proper interpretation. Some of the results are illustrated in the distribution table, which shows the range and stratigraphic correlation of the 147 species and 15 varieties identified from New Zealand, with those known to occur in Australia. His comparisons with other localities are very general.

Brown bases his classification upon the work of Canu and Bassler (1917–35), Hincks (1880), Busk (1852–84), Levinsen (1909), Harmer (1902–34), Hastings (1930–49), Marcus (1921–42), and Silén (1938–44). He finds that there are many points on which it is impossible to accept the views of Canu and Bassler, whom he follows with respect to the units above the genera, but with whom he differs in the definitions of certain genera.

The classification of the Ascophora by Silén is "based chiefly on the hypothesis that the Cheilostomata and the Ctenostomata are closely related and sprung from common ancestors in the not far distant past." This view is not adopted by Dr. Brown, yet Silén's discussions influenced him to devaluate the importance of the ovicells in classification to a much greater extent than Canu and Bassler. Brown agrees in part with the Jullien and Calvert classification by considering the frontal wall of the orifice an important feature. Whenever the structural data alone are used, his classification is more closely allied with the work of Canu and Bassler than with that of other specialists in this field.

The body of the monograph, from page 42 to page 378, consists of the descriptions of species and genera, with text figures and diagnostic discussions of a few of the families and higher units of his classification.

It is necessary that a student of Bryozoa should have access to Dr. Brown's monograph, whether he is following the biological trend of bryozoan development during the Tertiary, or dealing with problems of ecology and disposition of the Cheilostomatous population in Tertiary seas.

HORACE N. CORYELL

New Orleans, Louisiana

BUILDING CURRICULA

Improving Undergraduate Instruction in Psychology. Report of a study group supported by the Carnegie Corporation of New York and the Grant Foundation which met at Cornell University, June 27–August 16, 1951. Dael Wolfle, Chairman. New York: Macmillan, 1952. vii + 60 pp. $1.25.

FROM the psychologist's standpoint, the recommendations of this study group are grounded in the scientific point of view. From the educator's standpoint, they are traditional rather than functional—assuming such clear-cut dichotomies possible.

First, the authors propose objectives of undergraduate instruction in psychology. Of the three legitimate objectives discerned (knowledge, habits of thought, and attitudes and values), the first is to be stressed. The recommended curriculum consists of introductory courses (most important), elementary special-interest courses, core courses, advanced courses, and integrating courses. Although intended primarily for the liberal arts student, they also consider this array the best kind of training for prospective psychologists.

The course outlines are valuable, showing differences in the developmental and the cross-sectional approaches to introductory courses. The use of one type of approach, in the opinion of the study group, should preclude the use of any part of the other by a given author of a textbook. Other course outlines presented are for perception, motivation, thinking and language ability, and statistical reasoning.

That the authors do not set themselves up as a group intending to dictate the psychology curriculum in every situation is borne out in a number of places. They imply, for example, that alternates to the proposed curriculum are entirely possible in given situations. And, in the last chapter, they suggest research problems intended to help in the resolution of certain questions which, in the study group, were solved by general agreement.

The authors hold little hope for fulfilling the personal adjustment and growth (mental hygiene) objective for courses in psychology, suggesting that most psychologists should not assume the role of therapists, this function belonging to other institutional personnel. Hence, there would be no "psychology of adjustment," for example. But, if one of the objects of education is to form, the psychology of adjustment could well contribute by pointing up the various facets of adjustment and the means for attaining them, as well as to propose a unifying principle which serves an integrating purpose. It is not clear that subsidiary aims, such as the promotion of adjustment, in addition to major emphasis on psychology as a liberal arts discipline, detract from the value of the courses for certain students. The movement to widen the horizons of higher education unquestionably will bring students to many college campuses who need adjustment in addition to training. Why should not the psychology teacher aid student personnel officers by being, at least in part, concerned with the student's personal and social growth? The answer to this question will vary from one institution to another and even from class to class because the complexion of study groups, in terms of mentalities, behavioral characteristics, experiences, and needs, varies widely. Are we building psychology curricula for psychologists or for students? The question is pertinent in any institutional setting.

Instructors in psychology should welcome this little book, compressed in size but meaty in content. It would be expecting too much that all readers will agree with all the recommendations made, but there will be few indeed who, in their day-to-day association with their classes, have not come in contact with most of the problems presented. The book is worthy of wide circulation and thorough reading. This reviewer feels that the authors have made a significant contribution to better instruction in psychology.

LESTER NICHOLAS RECKTENWALD
Department of Psychology, Villanova College

BRIEFLY REVIEWED

From Lodestone to Gyro-Compass. H. L. Hitchins and W. E. May. New York: Philosophical Library, 1953. 220 pp. Illus. $4.75.

THE history of instruments is not only a fascinating subject but also a most useful one, for it provides us with the means to judge ancient science objectively. The achievements of the ancient author or scientist depended very much on the precision and range of his tools. Hence, a story of the compass written by two British naval officers well acquainted with modern navigation instruments and familiar with their use from practical experience should be welcomed.

The earlier chapters on the compass in legend and history, on magnetic charts, and on errors of the magnetic compass in a ship and their discovery in the course of time make pleasant reading, although they have suffered from lack of acquaintance with foreign literature, as is also proved by the short bibliographies accompanying each chapter. We miss several important books, such as A. Ritter von Urbanitzky's *Magnetismus im Altertum* (Vienna, 1887), E. O. von Lippmann's *Geschichte der Magnetnadel* (Berlin, 1932), and Winter's essay in *Forschungen und Fortschritte* (Vol. XV, 1939).

The last 150 pages deal with the nineteenth- and twentieth-century developments of the magnetic compass, their transmission, the gyrocompass, compasses for aircraft, inductor compasses, and sun compasses. These chapters, well written and illustrated with well-drawn figures, will suit the general reader admirably. This book is a good example of first-class popular science in the best sense of the word—explaining in simple terms but at the same time conveying real information.

R. J. FORBES
University of Amsterdam

I Drank the Zambezi. Arthur Loveridge. New York: Harper, 1953. xiv + 298 pp. Illus. $4.00.

ARTHUR LOVERIDGE, of the Museum of Comparative Zoology at Harvard University, knows and loves his Africa and its natural history. His latest trip was made in 1948, to Nyasaland, a province new to him. He was accompanied by his wife and her sister; the latter acted as chauffeur as well as ardent collector of small creatures.

Conditions in Africa had changed a great deal since his previous journeys there, and much destruction had taken place in the forests. Imagine having to buy wood for your campfire from the government when you are camping on the edge of a forest in Equatorial Africa! Even the price of dead monkeys had doubled. The animals were brought in and the collector kept the skull and skin for the museum, but turned the meat back to the hunter to be eaten.

This region was studied a century ago, but numbers of the species then described had not been taken since, and one object of the trip was to collect topotypes of of these little-known species.

Camps were made in the lowlands along streams and on the high Nyika Plateau at an altitude of 7500 feet, and specimens were caught in every camp—some long-lost species, and others new to science. "Snakes were present at Mtimbuki in pleasing variety," and all members of the party, assisted by natives, brought in daily what they had found. Often the porters were women, and out of forty of these, fourteen carried babies on their backs, as well as loads on their heads.

Although the expedition wanted some of the larger animals, most attention was paid to the smaller ones, such as the elephant shrew, the blesmol, and squirrels; land shells were found by the hundreds, and lizards, snakes, and frogs. After all, Loveridge is primarily a herpetologist, and there is as much thrill in capturing a long-lost lizard or a new species of snake as in obtaining a species of zebra in order to decide the question of whether it was really different from others, as had been stated years before.

One of the things that Loveridge wanted was Müller's clawed-frog, but he was unable to get into the exact locality where it occurs. The storks could, however, and the collector ingeniously shot one that carried half a dozen of these desiderata in its gullet.

It is a splendid narrative of an interesting and sometimes thrilling collecting trip, with observations not only on the animals but on native habits, told with appreciation and a sense of humor. There were exciting times, but Loveridge did not get excited. There were hardships, such as going through an unusually arid season and then an unusually wet one. With chigoes in his toes, bot-fly larvae in his leg, and stinging ants everywhere else, he still enjoyed it. So will the reader.

W. M. MANN

National Zoological Park
Washington, D. C.

Eiweiss. Heinrich Hellman. Stuttgart: Schwab, 1952. 163 pp. DM 5.80.

WITHOUT protein, life is unknown. With this thought Professor Butenandt, Nobel laureate, begins the foreword he has written to this little volume by his associate of many years in the Institute for Biochemistry at Tübingen. Dr. Hellman has undertaken to present in a concise but quite extensive fashion the present knowledge of the chemistry of the proteins as a class and of certain ones having special biochemical significance. In accord with the plan and the restrictions of space, the exposition is essentially categorical but is evidently drawn from a great fund of knowledge that has been carefully weighed and assayed. Numerous investigators are named but without references to their publications. The bibliography, a brief listing of monographs, does not include the recent (1950) comprehensive book of F. Haurowitz.

After brief discussion of the constituent amino acids, of their properties and form (designated in lower case), and of the peptide linkage and the dimensions in the peptide chain, of the influence of hydrogen bonds, the disulfide linkage and salt bridges on the organization (folding) of the proteins, the fibrous proteins and their α and β forms and the globular proteins in native and denatured forms are considered. Methods are described for the isolation and purification of proteins from natural sources, with especial attention to procedures based on solubility properties, and on the use of ultracentrifugation and electrophoresis; for the determination of molecular weights, and of the shape of protein molecules, as by birefringence of flow and with the electron microscope. Studies on the hydrolysis of proteins by acids, alkalies, and specific enzymes, and of the separation, identification, or the determination of the resulting amino acids by ionophoresis, selective adsorption (chromatography) on columns or paper, partition separation, as by the Craig countercurrent technique, by isotope dilution, and microbiological assay, have been outlined. Studies on the arrangement of the individual amino acids in the protein molecule, culminating in the work of Sanger on insulin, are described.

The concluding third of the book presents brief, rather cursory surveys of enzymes, including the better-known coenzymes, the hormones of protein nature, protein toxins, viruses, hemoglobin and other blood proteins, including the immunoglobulins, and, finally, of the metabolism of the proteins and amino acids.

This little book may be recommended to those who desire a brief authoritative survey of proteins at about the level of a good course in introductory biochemistry. It may be pointed out that the practical absence of the more intricate sentence patterns makes for easy reading of the German text.

RALPH C. CORLEY

Department of Chemistry
Purdue University

Tornadoes of the United States. Snowden D. Flora. Norman: University of Oklahoma Press, 1953. xiv + 194 pp. Illus. $3.50.

THE author of this, the first volume concerning tornadoes, has studied these fascinating storms (often wrongly called cyclones) with exceptional care for many years. For thirty-two years he was the section director for Kansas of the U. S. Weather Bureau. He has written many scholarly articles on individual storms, but this book can be read with enjoyment by almost anyone. It discusses all the chief aspects of these storms and their occurrence, and includes many suggestions for mitigating their damage. Tables and maps of frequency as to area, month, and hour of the day supplement brief descriptions of especially disastrous storms, arranged by cities, states, and years. There are numerous illuminating photographs.

Tornadoes have occurred in every one of the American states, and in each of the months. Although they are most frequent in the Midwest, hundreds have occurred recently in the South and many scores in the Northeast. During recent decades, the average number in the United States is about 200 per year; the death toll often rises to more than 300 people per year, and the property loss to many millions of dollars.

The whirling winds of some tornadoes attain velocities of more than 500 miles per hour. Destruction is also caused by the "explosions" of buildings passed over, because the air pressure at the center of the storm is low, "nearly a vacuum."

Almost unbelievable happenings occur in tornadoes, with many miraculous escapes. They are such small storms, having an average width of about 400 yards and a length of a few miles (damaging only an average of three square miles), that even where they are most frequent, they normally strike a given small area only rarely. Sometimes, however, a small area may be hit by a succession of tornadoes.

In the thirty-four years ending with 1949, the states with most tornadoes officially recorded are: Kansas, 587; Iowa, 512; Texas, 461; Oklahoma, 369; Arkansas, 299; and Mississippi, 207. States with 100–190 were Alabama, Nebraska, Minnesota, Illinois, Georgia, Florida, Louisiana, Indiana, South Dakota, South Carolina, Tennessee, Wisconsin, and Ohio.

The states having the greatest property damage from tornadoes during the thirty-four years ending with 1949, with the millions lost, are: Oklahoma, 52; Illinois, 48; Missouri, 44; Texas, 34; Georgia, 28; Ohio, 25; Indiana, 23; Kansas, 20; Iowa, 20; Minnesota, 18; Arkansas, 17; Alabama, 16; and Mississippi, 15.

The states with the most deaths caused by tornadoes in those thirty-four years are: Illinois, 911; Arkansas, 866; Mississippi, 792; Alabama, 714; Oklahoma, 664; Texas, 632; and Georgia, 527.

A person living for the normal life span, sixty-eight years, in Arkansas has one chance in 1529 of being killed by a tornado. In Oklahoma one's chances would be one in 1684, in Missouri one in 4132, and in Kansas one in 5736.

Among the subjects discussed by Flora are: When the Tornado Strikes, Cause and Structure, Destructive Force, Forecasts and Warnings, When to Expect Tornadoes, How to Escape Death and Protect Property, Freaks of the Storm, Some Outstanding Disasters, Tornado Hazard by States, Other Whirling Storms, and Tornadoes Elsewhere.

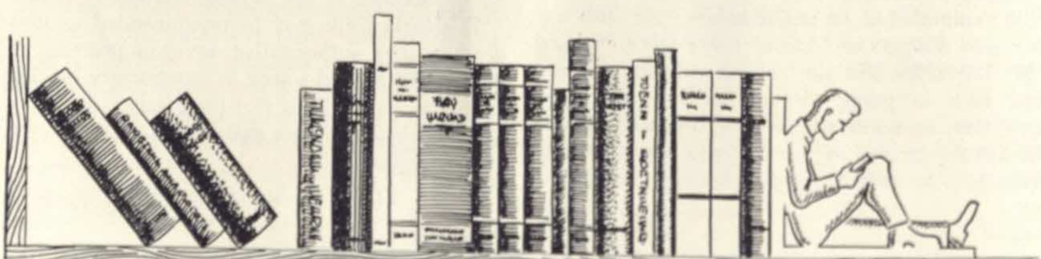
In brief, this book attains a high level of interest, reliability, and constructiveness. None of us in the United States is free from danger from tornadoes (which also occur very widely elsewhere). The effective presentation of facts about tornadoes and of methods found successful in reducing their destructiveness makes this book a contribution of which the author and the U. S. Weather Bureau can be proud.

STEPHEN S. VISHER
Department of Geography
Indiana University

Early Man in America. A Study in Prehistory. E. H. Sellards. Austin: University of Texas Press, 1952. xvi + 211 pp. Illus. $4.50.

RAPIDLY accumulating information in the Americas on the association of artifacts and human bones with the bones of extinct animals or other evidence of comparable antiquity poses a difficult problem for those who wish to acquire or maintain familiarity with the data of the field. Dr. Sellards' book is a valuable and timely compendium of the subject which will be highly useful to students of American archaeology.

ALBERT C. SPAULDING
Museum of Anthropology,
University of Michigan



ASSOCIATION AFFAIRS

PRELIMINARY ANNOUNCEMENT, SEVENTH BOSTON MEETING DECEMBER 26–31, 1953

FROM the programs and other events already arranged, it is apparent that the 120th Meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE will be particularly well balanced, well attended, and significant—one of the best meetings in the long annals of the Association. At this time the Association, soon to enter its 106th year, with 238 affiliates and 50,000 individual members, is on the threshold of careful studies to see how its services to science, to scientific organizations, to scientists, and to society may be improved and increased.

One of the fundamental purposes for which the Association was founded, in September, 1848, still endures: ". . . by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States. . . ." At Boston this December the annual meeting for the year 1953 once more will bring together leaders and younger men and women in the principal fields of science, not only to read papers reporting current research and to discuss their specialties, but also to attend outstanding symposia and to consider some of the problems that affect science and the world today. This 120th Meeting has as its theme "Scientific Resources for Freedom," and a number of the 18 sections of the Association and participating societies will have programs devoted to physical resources, scientific manpower, and scientific techniques—men, materials, and methods—related to the national economy, security, and welfare.

Although this year's 120th Meeting is typical of AAAS meetings in the past—with national meetings of large societies, interdisciplinary sectional symposia, sessions for contributed papers arranged by many of the sections, distinguished evening addresses, a large-scale Exposition of Science and Industry, and a showing of the latest foreign and domestic scientific films—there is a growing trend toward recurrent conferences in which many scientists, irrespective of their specialties, will be interested. At Boston, in addition to the Academy Conference representing the 40 academies of science now affiliated with the Association, the Conference on Scientific Editorial Problems II, and the Conference on Scientific Manpower III, there will be one or two sessions on "The Scientist in American Society" and two sessions on "Transmission of Ideas." For the first time in many years, a past president of the British Association for the Advancement of Science, Dr. A. V. Hill, will be present and will address the American Association.

AAAS general symposia. The Association will sponsor two general symposia, each of two sessions, in accordance with the decisions of the 1953 Symposium Committee consisting of E. U. Condon, chairman; Frank A. Beach; Bart J. Bok; Charles D. Coryell; A. M. Gaudin; A. Baird Hastings; Jerome C. Hunsaker; James R. Killian, Jr.; Paul C. Mangelsdorf; Philip M. Morse; Alfred C. Redfield; Francis O. Schmitt; Earl P. Stevenson; George B. Wislocki; and Raymond L. Taylor, secretary.

On December 27, the general symposium, "Species Which Feed Mankind," suggested and planned by Paul C. Mangelsdorf, will deal with the scientific aspects of several of the critical species of plants and animals that comprise the basic food sources of man. Although the symposium will stress the most recent findings in genetics, plant pathology, and animal husbandry, it is important for all.

On December 29, the second general symposium, "The Sea Frontier," with Alfred C. Redfield and Jerome C. Hunsaker as co-chairman, will bring together a number of the phenomena of the interface of land and salt water. Aspects included will be the geology of beaches, littoral oceanography, marine ecology, and interrelated engineering and industrial problems.

Focus of the meeting. The activities of the meeting period will center in downtown Boston in the Mechanics Building at 111 Huntington Avenue. Here will be located the Main Registration and Information Center (the only source for special booklets on Boston's points of interest and other literature), the Visible Directory of Registrants, the AAAS Office, the AAAS Science Theatre, and the Annual Exposition of Science and Industry. In the meeting rooms of the building will be held the general symposia and principal sectional programs and, from 8:30 to 11:30 P.M., December 29, the Biologists' Smoker. In nearby Copley Square, the geneticists will occupy the Sheraton Plaza Hotel, and other sessions will utilize auditoria in the Boston Public Library and in Boston University Junior College. Three blocks east of Copley Square, the zoological societies will be based at the Statler Hotel, and two blocks farther, the three science teaching societies will meet in the Hotel Bradford. In Horticultural Hall, three blocks in the opposite direction from Mechanics Building, there will be other sessions. Between the extremes in each direction, the distance is little more than one mile. In general, no one will be more than 5 to 10 minutes away from any other meeting point. Underground trolleys along Huntington Avenue and Boylston Street, with subway connections, will be convenient for those who wish to visit demonstrations or open houses at MIT and Harvard University.

Hotels. The Statler Hotel will be AAAS Headquarters and the locale of such evening events as the Association's Presidential Address, by Detlev W. Bronk, and the AAAS Reception on December 28, and also the

annual addresses of the Scientific Research Society of America, the Society of the Sigma Xi, and the United Chapters of Phi Beta Kappa, the dates of which will be announced. Sessions will be held in the Statler (zoologists), the Sheraton Plaza (geneticists), and in the Bradford Hotel (science teachers). Other hotels, to be used primarily for sleeping accommodations, are: the Touraine and Parker House in downtown Boston; the Copley Square, Lenox, and Vendome, near Mechanics Hall; and the Somerset and Kenmore on Commonwealth Avenue in the Back Bay area. Headquarters of each participating society will be given in a later announcement. Detailed housing information and a coupon for room reservations will appear in SCIENCE and THE SCIENTIFIC MONTHLY beginning about the end of July.

Advance registration. As in recent years, advance registrants will receive the General Program-Directory early in December by first-class mail. Coupons will appear in the AAAS journals beginning in late July.

THE PROGRAMS

A—Mathematics

Section A will schedule a vice-presidential address.

B—Physics

Section B will have two symposia; in addition, it will cosponsor with Section M "Transformations within Metallic Crystals" arranged by A. M. Gaudin, and the symposium of Section D; the retiring vice-presidential address, on a subject relating to the upper atmosphere, will be given by O. E. Hulburt. The *American Meteorological Society* will hold a national meeting with two or more sessions for papers.

C—Chemistry

Section C, with Randolph T. Major as program chairman, will have several sessions for contributed papers, principally on Dec. 27, and a six-session symposium, "Feeding the Nation": I and II—Human and Animal Nutrition arranged by Robert S. Harris; III—Chemicals in Food, by Charles N. Frey; IV—Chemistry of the Sea as Related to Food Problems, by Harold J. Humm; V—Growth and Nutrition of Plants, by P. W. Zimmerman; VI—Recent Progress in Food Processing, by B. E. Proctor. Appropriate parts will be cosponsored by Sections Nm, G, and O. Dr. Major will give the vice-presidential address. *Alpha Chi Sigma* will schedule a Chemists' Luncheon.

D—Astronomy

Section D will have a comprehensive symposium and panel discussion on "Current Progress in Radio Astronomy" arranged by Bart J. Bok, and a vice-presidential address, "Identifications of Solar Lines," by Charlotte Moore Sitterly, the morning, afternoon, and evening of Dec. 26, in the Lecture Hall of the American Academy of Arts and Sciences.

E—Geology and Geography

Section E is scheduling a two-session symposium in geology and two one-session symposia in geography concurrently on Dec. 28; a two-session symposium, "Water for Industry," on Dec. 29; and concurrent sessions for contributed papers in geology and geography, Dec. 30 and 31, with appropriate parts of the week's program cosponsored by the *Geological Society of America* and the New England Division, *Association of American Geographers.* The Geologists' Smoker and vice-presidential address by Arthur C. Trowbridge will be the evening of Dec. 30. The *National Speleological Society* will meet the afternoon of Dec. 26.

F—Zoological Sciences

The *American Society of Zoologists* will open four days of sessions Dec. 27 with a symposium, have four concurrent sessions the mornings and afternoons of Dec. 28 and 29, and a second symposium and demonstrations at Harvard on Dec. 30. The Zoologists' Dinner will be on the evening of Dec. 29; an illustrated address will be given by Paul Weiss, vice president of Section F, which will cosponsor the American Society of Zoologists symposia. The *Herpetologists League* will meet the afternoon of Dec. 28. The recently incorporated *Massachusetts Zoological Society* will have sessions for papers. The *Society of Systematic Zoology* will open four days of meetings with a symposium the evening of Dec. 27, hold sessions for papers, and business meetings the other days.

FG—Zoological and Botanical Sciences

Among the societies whose fields lie in both botany and zoology, the *American Society of Naturalists* this year will hold its annual meeting with the Association with a business session, a symposium, and a presidential address. *Beta Beta Beta* will hold its biennial convention, with an address by Edmund W. Sinnott, executive sessions on Dec. 28 and a luncheon and afternoon session on Dec. 29. The *American Society of Human Genetics* has scheduled three morning sessions for papers, Dec. 28–30; a business meeting, Dec. 28; three afternoon symposia: "Human Genetics and Medical Education," Dec. 28, "Genetic Factors Affecting Intelligence," jointly with the *American Eugenics Society,* Dec. 29, and "Genetics and the Races of Man," cosponsored by the Genetics Society of America, Dec. 30. The annual dinner and presidential address of the society, by C. P. Oliver, will be the evening of Dec. 29 at the Copley Square Hotel. The sessions of the annual meeting of the *Genetics Society of America* include meetings of the Executive Committee, Dec. 27 and 30; concurrent sessions for papers Dec. 28–30; a luncheon, business meeting, and demonstrations the afternoon of Dec. 29; and the joint symposium, "Genetics and the Races of Man," with the American Society of Human Genetics. The *Ecological Society of America* will cosponsor appropriate sessions of Section G and of the American Society of Zoologists and may have a program of its own. The *Society for the Study of Evolution* will have a program at Boston, arranged by Alfred Romer, and it is expected that the *Society for Industrial Microbiology* will again have several days of sessions with the AAAS. The *National Association of Biology Teachers* will hold its annual meeting Dec. 27–31 with the Association.

G—Botanical Sciences

Section G will have sessions for contributed papers, a number of symposia—including one of two sessions on "Native American Crop Plants and Climatic History in Relation to Man," arranged by Volney Jones and cosponsored by Section H, and one on plant physiology cosponsored by the New England Section of the *American Society of Plant Physiologists*—and a Botanists' Dinner at which Edgar Anderson will give the vice-presidential address.

H—Anthropology

Section H will have a two-session symposium on "Non-Human Primates and Human Evolution" arranged by James A. Gavan, Dec. 27; a symposium on "Theoretical Models for the Study of Cultural Process and Change" by Evon Z. Vogt; the joint symposium with Section G; a group of invited papers on "New England Archaeology" arranged by Douglas Byers, Dec. 29; and sessions for contributed papers, Dec. 29, in the fields of archaeology, social anthropology, etc. The Anthropologists' Dinner and vice-presidential address by Clyde Kluckhohn will be on the evening of Dec. 27.

I—Psychology

The program of Section I includes sessions for invited papers in the areas of learning, comparative behavior, brain function, human engineering, and sensory processes—arranged, respectively, by Fred D. Sheffield, Burton S. Rosner, Walter A. Rosenblith, Leonard C. Mead, and Edwin B. Newman—and for contributed papers, over the period Dec. 28–30. The vice-presidential address will be given on the evening of Dec. 30 by Frank A. Beach.

K—Social and Economic Sciences

Section K has planned two or three two-session symposia, including "Economic Problems of New England," Dec. 27, and "Effects of War on Scientific Development," Dec. 29. The *National Academy of Economics and Political Science* will have a symposium cosponsored by Section K and in collaboration with *Pi Gamma Mu*.

L—History and Philosophy of Science

The program of Section L includes a joint symposium, "Art and Science," with the Philosophy of Science Association, on the afternoon of Dec. 27; on Dec. 28 a symposium, on "Criteria for Validity in Science" arranged by Philipp G. Frank, cosponsored by the *Institute for the Unity of Science;* and a joint symposium, "Science and General Education," with Section Q and the History of Science Society. The *History of Science Society,* holding its annual meeting with the AAAS, will have a day or two of contributed papers and other events and will cosponsor appropriate symposia of Section L. The *Philosophy of Science Association* will arrange a program for Dec. 30 and cosponsor the symposia of Section L.

M—Engineering

Section M will have a series of symposia: "Aids to the Blind" arranged by Eugene F. Murphy, cosponsored by Sections N and I; "Safety as a Natural Resource"; "The Boston Banks and the Growth Potential of New England" cosponsored by Section P; and the one on metallic crystals referred to under Section B.

N—Medical Sciences

Alpha Epsilon Delta National Premedical Honor Society will hold its annual luncheon Dec. 29. The *American Physiological Society* again will have a symposium under the auspices of the Survey of Physiological Science. The *American Association of Hospital Consultants* will sponsor a symposium on "The Research Function of the Hospital" arranged by E. M. Bluestone; speakers include Jack Masur, Dean A. Clark, and Harvey Agnew, and discussants.

Subsection Nm—Medicine

Subsection Nm will sponsor a four-session symposium "Antimetabolites and Cancer" arranged by Cornelius P. Rhoads and Allan D. Bass, Dec. 28 and 29. The vice-presidential address will be given by Dr. Rhoads.

Subsection Nd—Dentistry

Subsection Nd plans three sessions on Dec. 29 arranged by Howard R. Marjerison.

Subsection Np—Pharmacy

Over the period Dec. 26–31, Subsection Np will have sessions for contributed papers and symposia cosponsored by the Scientific Section of the *American Pharmaceutical Association,* the *American Society of Hospital Pharmacists,* the *American Association of Colleges of Pharmacy,* the *American College of Apothecaries,* the *American Drug Manufacturers' Association,* and the *American Pharmaceutical Manufacturers' Association.*

O—Agriculture

Section O plans some four sessions for Dec. 28 and 29.

P—Industrial Science

Section P, now in its third year, will have a program arranged by Francis J. Curtis. The New England Section of the *American Industrial Hygiene Association* will have a two-day program arranged by W. M. Pierce, consisting of joint meetings with other groups on Dec. 28, a technical session of the society the morning of Dec. 29, and papers of general interest on industrial hygiene, on the afternoon of Dec. 29. (Those interested in giving papers should communicate with F. J. Viles, Jr., Department of Industrial Medicine, MIT.) The *Society for Industrial Microbiology,* it is expected, will have sessions for contributed papers and a symposium, as in prior years.

Q—Education

Section Q plans a two-session symposium on "Visual Efficiency in Industry" and another of three sessions on "Conserving Human Resources," sessions for contributed papers, and a vice-presidential address by Donald D. Durrell, Dec. 28–30. The *AAAS Cooperative Committee on the Teaching of Science and Mathematics* will have a two-session symposium, arranged by George G. Mallinson and cosponsored by Section Q and the three science teaching societies, Dec. 27. The *National Science Teachers Association* will have three days of sessions, a number of them concurrent, others joint with the National Association of Biology Teachers and the American Nature Study Society, Dec. 28–30. The *American Nature Study Society's* annual meeting, from Dec. 26–30, includes a program on marine biology, two sessions on animal ecology, sessions and presidential address, and a field trip with the National Association of Biology Teachers.

X—Science in General

The *Committee on Disaster Studies, National Research Council* is sponsoring the symposium "Disaster Recovery II," arranged by Harry Williams. The *National Association of Sciences Writers* will hold its annual meeting with the AAAS and have a program. The *Scientific Research Society* and the *Society of the Sigma Xi* will sponsor evenings addresses and, on Dec. 29, hold their annual conventions with the Association. The *National Geographic Society* and the *United Chapters of Phi Beta Kappa* will arrange evening addresses.

Call for Papers by AAAS Sections

The following sections of the Association will have sessions for contributed papers. The secretaries or pro-

gram chairmen to whom titles and brief abstracts should be sent, *not later than September 30, 1953,* follow:

C—Chemistry Dr. Ed. F. Degering, George Washington Inn, New Jersey and C Streets, S.E. Washington, D. C.

E—Geology and Geography Dr. Jack B. Graham, 3400 North Westmoreland Street, Falls Church, Va.

G—Botanical Sciences Dr. Stanley A. Cain, School of Natural Resources, University of Michigan, Ann Arbor, Mich.

H—Anthropology Dr. Gabriel Lasker, Wayne University, 1512 St. Antoine Street, Detroit 26, Mich.

I—Psychology Dr. William D. Neff, Department of Psychology, University of Chicago, Chicago 37, Ill.

Nd—Dentistry Dr. Russell W. Bunting, School of Dentistry, University of Michigan, Ann Arbor, Mich.

Np—Pharmacy George F. Archambault, Pharmacy Branch, Division of Hospitals, Federal Security Agency, Public Health Service, Washington 25, D. C.

O—Agriculture Dr. C. E. Millar, Department of Soil Science, Michigan State College, East Lansing, Mich.

Q—Education Dr. D. A. Worcester, University of Nebraska, Lincoln, Nebraska.

RAYMOND L. TAYLOR
Associate Administrative Secretary

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE is an organization that has roughly fifty thousand individual members, and that represents affiliations with nearly two hundred scientific societies and academies. The Association has great potentialities for service to individual scientists, scientific groups, to science in general, and to our modern democratic society. It is the deep wish and the firm intention of the officers of the Association to do all in their power to help realize these potentialities.

To move forward with constructive and wise developments will require the best thinking of all the membership—hard and serious thinking, objective and loyal thinking. It will require imagination and the courage to run some risks in the attempt to serve great ends. We are confident that these qualities will be available to serve our Association.

In a situation such as ours, there is always danger that some members fear that changes will be too abrupt and too revolutionary, while many others are disappointed because there seems to be no progress. We ask for both confidence and patience. To the members of the first group we want to say that there is not, and never has been, so far as we know, any desire to bring about change that would be disruptive either in tempo or character. To the members of the second group we want to say that progress toward the Arden House goals is necessarily and properly slow; even though that progress has not become evident as yet, or in any event has not been publicly acclaimed, it is occurring.

Although it is the expressed will of the Association, as evidenced by the unanimous vote of the Council in favor of the Arden House policy, to alter, to modernize, and to enliven the annual meetings of the AAAS, we wish to caution that these changes should be worked out sensibly and gradually, proceeding by trial so that it can be assured that the meetings do in fact furnish that sort of inspiring contact with all of science which, we are sure, is desired by the membership.

In particular it is clear that for the time being it is important to include, at the big annual meeting, a certain body of short reports of current research in specialized fields. We bespeak, for our Association and, particularly, for the meeting in Boston this next winter, a full and active participation. We pledge that we will do our utmost to see to it that the Association, in all branches of its work, deserves your active support.

DETLEV W. BRONK, *Retiring President*
E. U. CONDON, *President*
WARREN WEAVER, *President-Elect*

THE SCIENTIFIC MONTHLY

JUNE 1953

The Black Madonna: An Example of Culture Borrowing*

LEONARD W. MOSS and STEPHEN C. CAPPANNARI

The authors are members of the faculty of Wayne University, in Detroit. Leonard W. Moss is an instructor in sociology and is a candidate for a Ph.D. degree at the University of Michigan. He spent two years in Italy with the 15th Air Force Intelligence during World War II. Dr. Cappannari is an instructor in anthropology and received his Ph.D. degree from the University of California, Berkeley. He is a Fellow of the American Anthropological Association.

Aphorism

"I am black, but comely,
O ye daughters of Jerusalem,
as the tents of Kedar,
as the curtains of Solomon.
Look not upon me because I am black,
because the sun hath looked upon me.
My mother's children were angry with me,
they made me the keeper of the vineyards;
but mine own vineyards have I not kept!

From the *Song of Songs which is Solomon's*

THE existence of black madonnas in various parts of the world has been noted by many authors. It should be explained that some of the so-called black madonnas are not actually black, but are dark brown in color. We have classified these representations of the Virgin Mary in three distinct categories.

First there are the *dark brown or black madonnas with physiognomy and skin pigmentation matching that of the indigenous native population.* In this class we include such madonnas as Nuestra Senora di Guadaloupe Hidalgo in Mexico and the various Negroid madonnas found in Africa. Though the Catholic Church has never given official approbation to these art forms, they are tolerated, probably as one way of bringing the religion closer to these populations.

Second, there are *various art forms which have turned black as a result of certain physical factors.* The change may have been brought about by (*a*) deterioration of the paint pigment (some of these

* Based on a paper presented to the Anthropology Section of the American Association for the Advancement of Science, December 28, 1952, St. Louis, Missouri.

madonnas have been repainted in flesh tones only to turn black again), (*b*) smoke damage stemming from the use of votive candles in areas adjacent to the statues, (*c*) smoke damage resulting from a fire in the church, (*d*) oxidation of the silver used in the construction of the image, and (*e*) other physical factors such as the accumulation of dirt over the ages.

Third, are the *miracle-working madonnas*. It is this class of madonnas that is investigated here. The hypothesis that there has been here an attempt to anthropomorphize the Virgin is not tenable, since the natives of these regions are Caucasoid. The various physical explanations cited in our second group do not seem applicable to this category. One must also discard the physical explanation, which may differ with the various madonnas under question. In such cases we find: (*a*) There is no evidence for physical deterioration of the pigment, no smoke damage, no oxidation, and so on. (*b*) Where there is evidence of such physical damage, the madonna has been repainted *black*.

The madonnas in this third category are found in such diverse locations as: Altötting, Bavaria, Germany (Our Lady of Altötting); Czenstochowa, Upper Silesia, Poland (Our Lady of Czenstochowa); Montserrat, Catalonia, Spain (La Santa Imagen, Our Lady of Montserrat); Einsiedeln, Switzerland (Our Lady of the Hermits); Chartres, France (Notre Dame du Pilier, La Vierge Noire); Tindari, Sicily (Madonna di Tindaro),* Enna, Castrogiovanni, Sicily (La Madonna della Grazia);* Lucera, Apuglia, Italy (Madonna della Spiga); Potenza Province, Basilicata, Italy (Santa Maria di Vigianno); Manfredonia, Apuglia, Italy (Santa Maria di Siponto);* Monte Vergine, Avellino, Italy (La Madonna di Constantinopoli).*

Of the eleven madonnas, we have felt it necessary to eliminate two from further consideration in the development of an hypothesis. Bavarian legends hold that the "Black Mother of God" at Altötting was preserved from smoke damage despite the ravishing of the church by flame in A.D. 907. The face, hands, and feet of this statue allegedly turned black "with age" at a later date. This explanation is deficient in that it fails to explain why the rest of the statue, in skin tones, did not undergo similar deterioration. Moreover, according to Dr. George Lechler, who is familiar with this icon, the art form is that of the seventeenth century. He believes it to be a copy of an earlier form.[1]

* The above-cited madonnas were not included in the original paper. Subsequent investigation revealed the existence of these representations of the Virgin.

The madonna at Czenstochowa is darker of skin than most Central European depictions of the Virgin. However, the figure is distinctly thirteenth to fourteenth century Byzantine in form, and this skin coloring is a characteristic feature of varied Byzantine portraiture. According to Ernst Scheyer, who studied this madonna at the behest of the Polish government, the present image was restored in the nineteenth century.[2] How the image got to Czenstochowa is debatable. Local legend describes a miraculous appearance of the statue sometime in the fifteenth or sixteenth century. For those who would rather explore the alternative possibility of diffusion, it may be noted parenthetically that the Queen of Poland *ca.* 1515 was Bona Sforza of Bari, Italy.

We wish to emphasize that all the black madonnas mentioned above are "miracle-working madonnas," and, with the exception of the Polish madonna, all these images are found in areas occupied by the Roman legions. The bulk of this paper is centered on the madonnas of southern Italy, but let us note some of the history and legend attached to other black madonnas listed above.

The statue of Our Lady of Montserrat was supposedly carved by St. Luke, in Jerusalem (La Jerosolimitana), and legend holds that it was brought to Barcelona by St. Peter. The statue was removed from Barcelona during the Moorish invasion of Catalonia in A.D. 718. Hidden in a cave near Montserrat, it was rediscovered in 880. Earliest archival notations indicate that the image was black at least as long ago as A.D. 718.

Our Lady of the Hermits at Einsiedeln, Switzerland, exhibits a history that may be traced back as far as A.D. 835. St. Meinrad built a chapel to the Virgin that year. Local tradition, however, alleges that the statue was brought there by Crusaders returning from the Middle East. This statue does not appear to be Byzantine in origin, nor does it give evidence of being a ninth century German art form.

The Black Virgin at Chartres dates back to the fourth century. Early Christian travelers to that area found an altar made by the Druids (?) upon which was seated a woman holding within her arms the figure of a child. This pagan image was black in color. The cathedral at Chartres, founded in the fourth century, was dedicated to the Virgin and Child. Although the present statue of the Virgin at Chartres is depicted as black, it is of more recent origin.

The Sanctuary of the Madonna of Tindari occupies the site of a fifteenth century church that had been built on the ruins of a temple to Cybele. This

Our Lady of the Hermits, the Madonna of Einsiedeln, Switzerland.

ancient temple was mentioned in the writings of Strabo and Pliny. Greek influence in Tindari dates back to the founding of the city by the elder Dionysius in 395 B.C., and the city was colonized by Peloponnesian exiles driven from their homeland by the Spartans. This black madonna at Tindari is perhaps the most famous of the Sicilian images of the Virgin.

Castrogiovanni, a corruption of the Arabic *Kasr-Yanni,* was the ancient Castrum Ennae. The mountain, upon which the town is situated, was the principal seat of worship for Demeter-Ceres. Founded *ca.* 664 B.C. by the Syracusans, the town was considered a choice prize by the many invaders of Sicily during the course of history. A temple to Ceres and Proserpine built at the summit of the mountain was the object of pagan pilgrimages before the introduction of Christianity in the eleventh century. The present church, Madonna della Visitazione, built by Queen Eleanora in 1307, incorporates in its south wall a pillar from the old temple of Ceres. The current statue of the madonna was constructed in the mid-nineteenth century. This image of the Virgin is not black.

Ancient Enna was sacred to both Ceres and her daughter Proserpine. It was in this locale that the abduction of the daughter by Pluto took place. According to Greek mythology, Proserpine (Persephone) was called Saviour, having gone through death and resurrection. It is at Castrogiovanni that one finds the most interesting adaptations of pagan symbolism by the Catholic Church. Until mid-nineteenth century, the images of Ceres and Proserpine were used in the church as the Virgin and Infant Jesus, despite the fact that Proserpine was female![3]

One writer, William Paton, has this to say about the ritual connected with the Castrogiovanni madonna:[4]

> On the day of the fete of the "Madonna of all the Graces" her worshippers place before her statue large sheaves of grain and bunches of wild flowers, and form processions in her honor, composed of men in long white tunics, who carry flowers in their hands, make offerings of grain and other products of the soil before the altar in the churches . . . it seems most reasonable to believe that many of the old pagan rites have been preserved in their essential forms in Christian ceremonies of today. . . . it is safe to say that Christian priests have added little to that ritual [of Demeter], have taken little from it, and today religious ceremonies practised

by the farming communities of Sicily are essentially the same as they were twenty-five centuries ago, with the exception that Christian saints have usurped the honors and dignities of pagan deities.

The history of the District of Apuglia shows frequent and prolonged culture contacts with the various groups that have occupied the Mediterranean basin. On this basis we look for indications of diffusion. The written record of Foggia Province begins around 975 B.C.[5] At that time one Diomedi, King of Etolia (a section of ancient Greece), landed at Rodi and journeyed to the Temple of Minerva at Lucera. There he remained, established a kingdom, married the daughter of the King of Daunia, and built a temple to Ceres (goddess of grain). Diomedi also built temples to Calcante (god of prophecy) and Podalirio (god of medicine) at Castel Drione, site of San Severo.

The region was occupied by the indigenous Italic tribes, the Pencezi and the Messapi. To the north were the Molise, in what is now the District of Abruzzi. According to the local legend this area was first visited by the Phoenicians as early as the tenth century B.C. Phoenician temples to Ma or Ammar were utilized by the Greeks, who rededicated these same temples to the Cretan Rhea or Cybele. Cybele, the mother goddess, gave birth to Demeter, the Greek goddess of grain, fertility, and earth. It is pertinent to our thesis that there were actually two Demeters. One was the sorrowful Eleusinian mother. The second and more powerful was Demeter Melaina, the Black Demeter associated with the earth and fertility.

The Greeks built temples to Demeter in and around the area of Foggia Province. Lucera existed as a city-state independent of Rome until 400 B.C.; then she allied herself with Rome and later (319 B.C.) became a colony of the Roman Empire. It was during this period that a cult of the Egyptian Isis was introduced into this area by seagoing natives of Apuglia. We should mention that Isis-Horus have been depicted as black by the Egyptians.

Many scholars have observed resemblances between the Osiris myth of death and resurrection and the story of Jesus. Carl G. Jung suggested that the wife of Osiris, Isis, and her son Horus may be considered as an Egyptian anticipation of the Virgin-child complex.[6] Direct contact between Hebrews and Egyptians is described in the story of Joseph, who may be regarded as a vizier under a Hyksos king, during the second Intermediate Period of Egypt.

It was Herodotus who suggested the connection between the Egyptian Isis and the Greek Demeter. While never explicitly acknowledged by the Romans, it seems evident that Ceres is a Roman adaptation of the Greek Demeter. At this point it should be stressed that the Roman Ceres was likewise depicted as black.

St. Peter, on his way to Rome in A.D. 42 installed Basso as the Bishop of Lucera. St. Basso built a cathedral to the Virgin Mary on the precise spot occupied by the Roman temple to Ceres. The cathedral was dedicated to the Madonna della Spiga (literally, sheaf of wheat, and later, ear of corn). After the fall of Rome this area was occupied by the Eruli, Ostrogoths, Greeks, Longobards, Moslems, Greeks (again), Normans, and the forces of the Holy Roman Empire, in that order. Frederick II (grandson of Frederick Barbarossa) invited the Moslems into the area in 1225 and built a mosque for them on the site of the Cathedral Madonna della Spiga. The Anjou dynasty, which came to power after the death of Frederick II, forced the Moslems from Lucera and rebuilt the mosque into the present-day Cathedral di San Francesco. The statue of the Virgin and Child was re-installed in the Cathedral in 1300.

Santa Maria, the Patron Saint of Lucera, is credited with the liberation of the city from the Moslems by Charles II (Carlo Secondo di'Anjou). A second major miracle attributed to the statue is the end of a plague of cholera on July 13, 1837. The statue miraculously moved its eyes on that date and for two consecutive days thereafter, bringing to an end the plague which had ravaged the city. If *dulia* and *hyperdulia* (veneration and adoration) are accorded saints, then the only word for the adoration of this image is that of *latria* (worship). The Madonna is worshipped for its power rather than the grace normally accorded the Virgin. It is showered with wheat, corn, and other sacrificial offerings on feast days, particularly on those feast days which coincide with the seasons of planting and harvesting. It is accorded powers relating to fertility (human, animal, and vegetative).

Elsewhere in southern Italy one finds other examples of these madonnas. The present-day church of Santa Maria di Siponto occupies the site of Sipontum, an ancient Roman colony. Because of flood threats, the city of Siponto was abandoned in A.D. 1256, and the inhabitants moved to Manfredonia two miles to the east. Records indicate that the present church was constructed prior to 1117. Before its reconstruction by Julius III in 1508, there are indications that the Saracens used the building as a mosque. The lower level of this church contains the tomb of Emilius Tullius (A.D.

593) and the miracle-working madonna. Local legends attest to the fertility powers of the icon. The origin of this statue is said to be Byzantine.[7]

At the summit of Monte Vergine, near Avellino, was the temple to Ceres in which the high priest Atys held sway as the Sybelenne oracle. The mountain draws its name from one Virgilius, a great necromancer and compounder of herb drugs (l'Orto di Virgilio). Christianity was late in coming to this stronghold of pagan beliefs. Henry Swinburne, the English author and traveler, claimed that the term "pagan" was a corruption of the word *pagi* (or *pagani*) meaning heathen inhabitants of the local villages.[8] In A.D. 1119 William of Vercelli dedicated the mountain to the Virgin Mary and founded a Benedictine abbey (the White Monks) on the site of the temple to Ceres. Swinburne acknowledged the tribulations of establishing Christianity in this area:[9]

> The missionaries sent among them to preach the faith of Christ found no means of conversion so easy and efficacious as those of admitting some of the names and ceremonies of the old church into the ritual of the new one. By thus adopting many tenets and forms of Paganism, they reconciled their proselytes to the idea of exchanging Jupiter for Jehovah, and their lares and penates for saints and guardian angels. To this expedient of priestcraft must be ascribed many strange devotions and local superstitions, still prevalent in Roman Catholic countries, which ought not to be confounded by the adversaries of that church with its real doctrines. All the truly learned and sensible persons of that communion reject, abhor, and lament such depravation; and, were it possible to reason rude minds out of hereditary prejudices, would long since have abolished them.

La Madonna di Constantinopoli (la Schiavona) is enshrined in the church, which contains four standing columns of *porta santa* marble that are part of the original temple to Ceres. This portrait of the Virgin is in two parts. The bust, carved of brown wood, is the work of Montano d'Arezzo and was completed about 1340. The head was brought from Constantinople by Catherine, wife of Philip d'Anjou, the titular emperor of that city. Legend holds that this painted head was the work of St. Luke in Antioch.[10] Swinburne takes issue with the legendary origin of the head:[11]

> This image is of gigantic or heroic proportion, and passes for the work of St. Luke the Evangelist, though the very size is an argument against its being a portrait from the life, had we even the slightest reason to believe he ever handled the pencil. There are in Italy and elsewhere some dozens of black, ugly Madonnas, which all pass for the work of his hands, and as such are revered.

The Black Madonna of Czenstochowa, Poland.

Swinburne goes on to explain that there was a painter in Constantinople called "Holy" Luke because of his piety and because of his exemplary life devoted to painting representations of the Virgin. His work was later attributed to St. Luke, since no one knew of another saint or painter by that name. Swinburne casts doubt on the idea that St. Luke was ever an artist.

Catholic sources, for the most part, have denied the possible connection between the black madonnas and the Roman Ceres. Nevertheless, it was St. Augustine who noted that the Virgin Mary represents the Earth and that Jesus is of the earth born. Archbishop John Hamilton in his *Scot's Catechism* (1552) states ". . . [these statues] darkened into something not far from idolatry . . . when . . . one image of the Virgin (generally a black or ugly one) was regarded . . . as more powerful for the help of suppliants. . . ."

A Mrs. Jameson, writing in the 1890's on the *Legends of the Madonna,* remarks:

> Because some of the Greek pictures and carved images have become black through extreme age, it

was argued by certain devout writers that the Virgin herself must have been of very dark complexion; and in favor of this idea, they quoted from the canticles, "I am black, but comely, O ye daughters of Jerusalem." [*sic*]* But others say her complexion became black only through her sojourn in Egypt. At all events, though the blackness of these antique images was supposed to enhance their sanctity, it has never been imitated in the fine arts. . . .

As we have previously noted, although some of the black madonnas are black because of age, this mechanistic dismissal fails to explain *all* the black madonnas. It is at this point that we suggest our hypothesis: The black madonnas are Christian borrowings from earlier pagan art forms which depicted Ceres, Demeter, or Isis as black in the color characteristic of these goddesses of the earth. Along this line, in 1335 a prior at the Chalis Monastery named Guillaume de Digulleville noted that Mary represents the earth, which is the body and darkness.[12] Hence, Mary being of the earth, can rightfully act as celestial attorney for all earthly sinners.

Experts on medieval art forms have demonstrated the influence of classical mythology on the paintings of the Middle Ages. It is entirely possible that medieval painters borrowed from classical mythology, dropped the original forms, and related them to the Virgin Mary.[13] However, in this paper we are concerned with some statues which seemingly predate the period of the Middle Ages. We are probably dealing with more direct forms of culture borrowing, possibly as part of direct culture contact.

Students of mythology have long pointed out that "black" could be regarded as associated with the earth. Carl G. Jung has equated "black" with the fertility powers of the earth, with the power of death, and with a fear of the power of darkness. Although we do not claim that any such necessary and inherent relationship would obtain in all parts of the earth, this association can be demonstrated easily for the cultures involved in our thesis.

We will reiterate that all the black madonnas are *powerful* madonnas, i.e., they are miracle workers. They are implored for intercession in the various problems of fertility. They are given *latria* rather than hyperdulia. Hence, we are equating their blackness with their power, which far exceeds that granted most madonnas in this sphere.

Anthropology is replete with examples of syncretism and reinterpretation as parts of the general process of acculturation. The development of Hinduism is one of the classic examples of syncretism in supernaturalism. Equally illustrative and analogous to our topic is the almost endless and intricate modification of various Egyptian deities in the early dynastic periods. When the capital of Egypt was moved to Thebes, Amon, who was the local god of the city, merged with Rē, the national sun god, to become Amon-Rē.† Changes in the culture of any society are always influenced by the pre-existing customs and institutions of that society. This is not to say that displacements never occur, or that innovations never obliterate older patterns of behavior.‡ For the most part, however, mankind is not easily detached from the patterned customs and beliefs which have fastened themselves securely upon human beings. The adoption of new beliefs is facilitated when the beliefs can be equated in some fashion with the older and compatible experiences.

It is in this light that we offer our hypothesis that these madonnas exemplify a reinterpretation of pagan customs, that they have functioned as aids in the preservation of continuity in the transition from pagan beliefs to Roman Catholicism.

* The canticles (*Song of Songs*) refers not to the Virgin but to a conversation between King Solomon and his black Queen. The intent of the original Hebrew version clearly sets forth the relationship between the two lovers.

References

1. Lechler, G. Personal communication.
2. Scheyer, E. Personal communication.
3. Sladen, D. *Sicily*. New York: Dutton, 315–24 (1907).
4. Paton, W. A. *Picturesque Sicily*. New York: Harper and Brothers, 250–51, 252–53 (1897).
5. Corrado, G. *Lucera, Nella Storia della Patria*. Lucera: Tipografia Scepi, 1–38 (1937).
6. Jung, C. G., and Karenyi, K. *Essays on a Science of Mythology*. New York: Pantheon, Bollingen Series, *passim* (1949).
7. Hare, A. J. C. *Cities of Southern Italy and Sicily*. New York: Routledge and Company, 286 (1905).
8. Swinburne, H. *Travels in the Two Sicilies*. London: Emsley, Vol. I, 120 (1783).
9. *Ibid.*, 121.
10. Ayscough, J. *Saints and Places*. New York: Benziger, 348 (1912).
11. Swinburne, H. *Op. cit.*, Vol. I, 123.
12. Jung, C. G. *Psychology and Religion*. New Haven: Yale University Press, 85, 126 (1938). From Delacotte, Abbe Joseph, *Guillaume de Digulleville Trois*, Romans-Poèmes du XIVᵉ Siècle, Paris (1932).
13. Panofsky, E., and Saxl, F. *Metropolitan Museum Studies*, 4, 228–80 (1933).

† For a discussion of this kind of gradual modification of deities, see H. Frankfort *et al.*, *The Intellectual Adventure of Ancient Man* (Chicago: University of Chicago Press, 1946), pp. 31–121.

‡ A recent work discusses this process in detail. See L. S. B. Leakey, *Mau Mau and the Kikuyu* (London: Methuen Company, 1953), pp. 78–85, 112.

Evolution of a Transportational Route as the Core of a Suburban Region

GEORGE LANGDON

Dr. Langdon has long been interested in urban development and is the author of an article, "Delimiting the Main Line District of Philadelphia," which appeared in Economic Geography. *He is a graduate of Pennsylvania State College and received a Ph.D. degree from Clark University, Worcester. He is now professor of geography and geology at the State Teachers College, Mansfield, Penna.*

SUBURBAN regions are commonly strung out along major arteries of transportation leading from a metropolis. In the vicinity of the transportational route the characteristics that distinguish the suburb are best displayed. This is the "core" of the region. As one progresses farther from this core, the distinguishing characteristics diminish in intensity and blend in with the features of the adjoining region. Often the interrelationships of the characteristics of two adjoining suburban regions are so intimate that great difficulty is encountered in determining a boundary line that will separate them. Thus the characteristics that give a region individuality are most prominent in its core. It is intended, here, to illustrate how a suburban region can develop a well-defined "heart" or "core" based upon the overwhelming presence of a transportational route. The sample region selected for analysis is a residential suburb of Philadelphia—The Main Line District (Fig. 1).*

Imagine a large wheel the hub of which represents the city of Philadelphia and the spokes of which depict the arteries of transportation leading out of the metropolis; it is then possible to visualize the principal features of transportation in the Main Line Suburban Region. The area lies along the meridian of the spoke extending due west from the central hub (Fig. 2). Transportation moves along this meridian in an east-west direction. Furthermore this spoke of transportation is rather efficiently isolated from the other "spokes" to the north and south; it is practically impossible to travel conveniently to adjacent suburban areas on public conveyances without first going through the central city to make connections.

This east-west trend of transportation has been conspicuous since the earliest settlements in the area, but its persistence is the result of two major developments. First, the Main Line District has become in reality a residential "bedroom." It is comparable to a dormitory; people live there but depend upon the city for employment, shopping, pleasure, education, and the other conveniences offered by a large metropolis. This intimate relationship generates a daily stream of commuters. The second reason is that the Main Line District lies along one of the principal east-west continental routes, which creates additional heavy through traffic.

Early Routes of Transportation

Since the earliest days of recorded history in the region east-west transportation has been of prime importance to the area now known as the Main Line. At first the Indians had a trail leading from the Delaware River to the Conestoga River and thence to the Susquehanna. At that time the trail was referred to as the Kitanning Path.[1] Soon after William Penn and the colonists appeared, this trail became a bridle path. People traveled on horseback, and freight moved by pack train. The twenty-five or so miles at the eastern end of this trail lay within the original Welsh "Barony" (now known as the Main Line District). To reach this westward trail it was necessary to cross the Schuylkill

* For a detailed account of the determination of the boundaries of the Main Line Suburban Region see "Delimiting the Main Line District of Philadelphia," by George Langdon, *Economic Geography*, pp. 57–65, January, 1952.

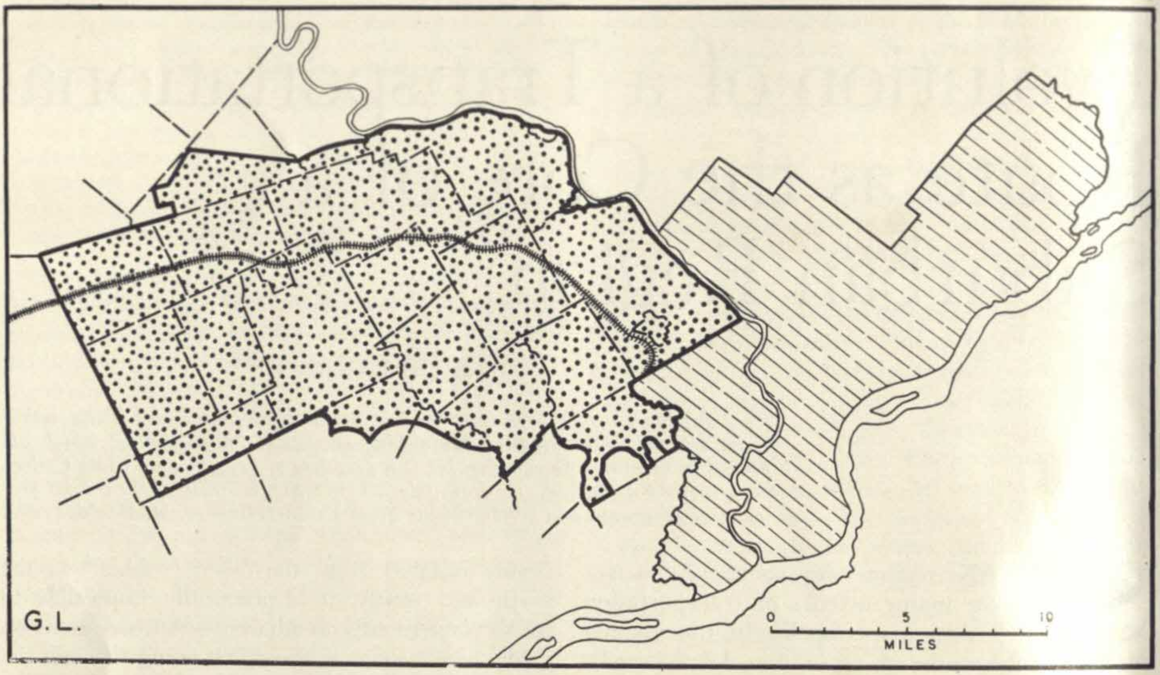


FIG. 1. The extent of the Philadelphia Main Line Suburban Region is shown by the dots superimposed upon the presently existing township lines. The line bisecting the Main Line area is the route of the Pennsylvania Railroad and illustrates the east-west trend of the transportational meridian. The area of diagonal lines represents the city of Philadelphia.

River, and, until ferry service was provided in 1682, it was difficult of access.[2]

In addition to this route to the west the residents made great use of the natural thoroughfare offered by the Schuylkill River. It flowed conveniently along the northern edges of their tract directly to the great, growing town of Philadelphia on the Delaware (Fig. 3).

The Old Lancaster or Conestoga Road

Within thirty years there was a road, of a sort, extending between Philadelphia and the Dutch settlements on the Conestoga River. It rather closely followed the old Indian trail. The portion between Philadelphia and the Welsh settlements west of the Schuylkill was laid out as early as 1690; it was one of the oldest roads on the continent. Efforts were made from time to time to open regular stage service, but the ruts, the mire, the snow, and the dust were victorious, and all attempts were futile.

In spite of these difficulties the Old Lancaster Road was a busy route to the interior. Traffic increased steadily, and there was great demand for a more commodious passage westward. Picturesque wayside inns began to appear along the route, and the pack trains were gradually replaced by the

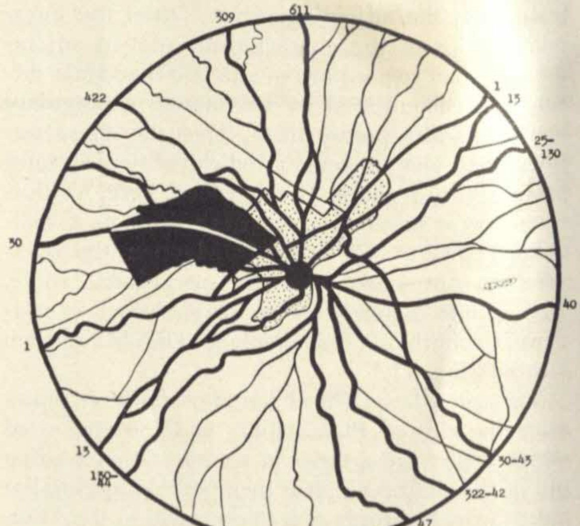


FIG. 2. The hub of the huge wheel of transportation around Philadelphia is indicated by the black circle in the commercial center of the city. The extent of the city is shown by the dots. Heavy lines represent the leading highways and thin lines the less important routes leading out of the city. The darkened area west of the city is the Main Line Suburban Region. The white line bisecting the area shows the location of the principal east-west route, the Lincoln Highway (U.S. 30) and the Pennsylvania Railroad's main line tracks. Numbers indicate the principal routes.

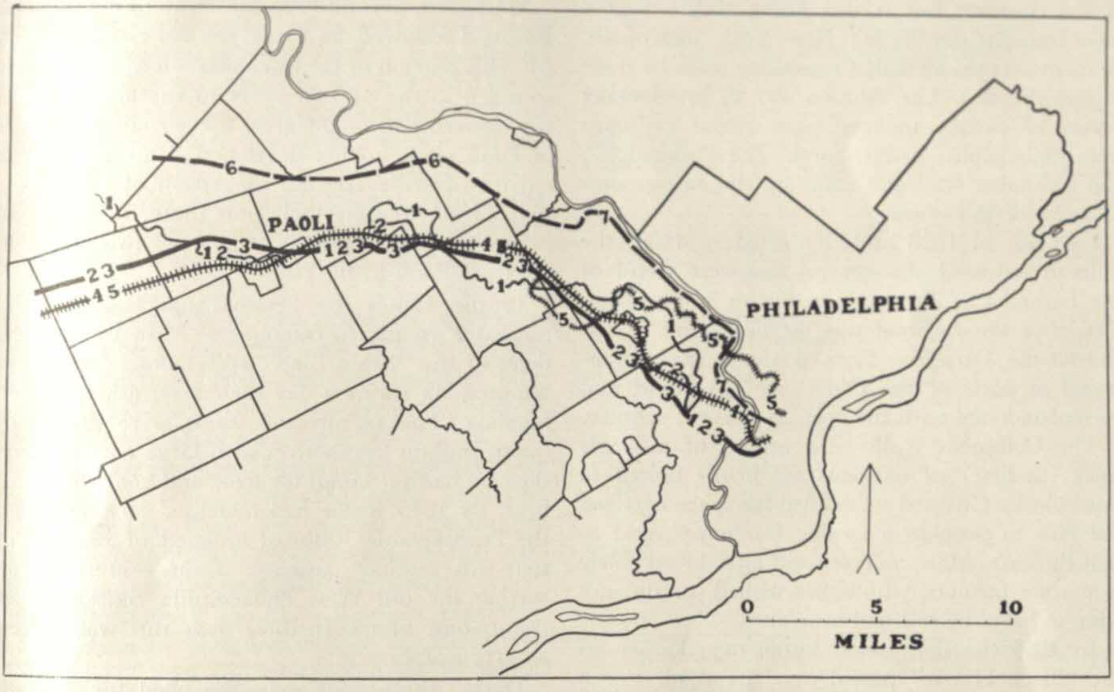


FIG. 3. Comparative approximate locations of the historical routes of transportation in the Philadelphia Main Line Suburban Region. The general east-west trend of the transportational meridian is evident. The routes are superimposed upon the existing township structure. The historical routes referred to in the text are numbered on the map as follows: (1) Old Lancaster or Conestoga Road, (2) Lancaster Turnpike, (3) Lincoln Highway, (4) The Pennsylvania Railroad's main line tracks, (5) Old Philadelphia and Columbia Railroad, (6) approximate location of the Pennsylvania Turnpike, (7) approximate proposed route of the Schuylkill Expressway.

famous Conestoga Wagons. In fact, these freighters became so popular along the route that the Old Lancaster Road was often called "The Great Conestoga Road" (Fig. 3). Portions of this road exist to this day and are still known by that name.

The Lancaster Turnpike

In 1791 the legislature authorized the construction of a new turnpike to Lancaster. The turnpike was completed in 1794.* It was 50 feet wide and over 62 miles in length; there was a center, paved with crushed stone, 24 feet wide and 18 inches deep. It was the first road of its kind in the country, and tolls were charged travelers making use of the route. Traffic increased greatly, and the turnpike was more than financially successful. This success inspired the construction of thousands of additional miles of turnpike roads throughout the country patterned after the Lancaster model. Unfortunately, because of the sudden introduction of steam power, most of these turnpikes were utter failures.

The new Lancaster Turnpike became congested with an almost unbroken procession of the ponderous Conestoga Wagons, regularly scheduled stagecoaches, large herds of animals being driven to market, and picturesque farmers' wagons. Travel was so extensive along this route that it was without parallel in the history of the country prior to the introduction of steam power.[3] Use of the turnpike increased apace until the 1830's; its period of usefulness was indeed the era of travel by wagon.

The famous Conestoga Wagons attained their greatest prominence along the route of this turnpike, and by the time of the War of 1812 they were in very general use. The wagons were well designed for the carrying of freight, because they had a curved bottom that prevented the cargo from slipping back and forth, and broad-rimmed wheels that prevented them from sinking into soft ground.

The Philadelphia and Columbia Railroad

In 1825 the State of New York very successfully initiated the canal-building period with the construction of the Erie Canal. Increasingly, westward traffic was moving along this new route to the interior. Philadelphia was then the largest city

* Some sources say it was authorized in 1792 and completed in 1796.

in the country, but it was losing traffic to the more rapidly developing New York metropolis. Pennsylvania decided that something must be done to stop this loss. The solution was an interlocking system of canals, inclined planes, and railroads from Philadelphia to Pittsburgh. The Philadelphia and Columbia Railroad made up the easternmost 82 miles of this route.

Laid out in 1828 and completed in 1834, the railroad followed the general east-west trend of the Turnpike as illustrated in Figure 3. The completion of the railroad was the beginning of the end for the Turnpike. Parts of it were even abandoned as parts of the Old Conestoga Road had been abandoned with the opening of the Turnpike.

The Columbia Railroad consisted of a single track (at first) of wooden rails firmly bolted to stone blocks. Crushed stone filled the space between the rails to provide a footing for horses used to pull the cars. Many curves were introduced solely to placate farmers, who either wished, or did not wish, to have the railroad near them.

In 1834 the first steam locomotive, known as the "Black Hawk," passed over the road. For a time attempts were made to use both horses and locomotives; another track was laid to provide one for horses and one for locomotives, but the experiment was a complete failure. Horsepower fought a losing battle, and by 1840 only locomotives were in use.

Although the Columbia Railroad augmented travel through this area, the complicated system of water and land routes to the west proved to be poor competition for the Erie Canal. By 1840 Philadelphia had lost the distinction of being the nation's largest city. It never regained that title.

Early Days of the Pennsylvania Railroad Company

The Pennsylvania Railroad Company was organized in 1846 to eliminate the disadvantages of the complicated rail-canal-inclined plane route over the mountains to the west. Within a few short years this through route became the "main line" for the exchange of commerce between east and west. It was during these years of successful expansion that the term "Main Line" was applied to the area along the tracks near western Philadelphia.

The era was heralded by improvements. The Old Columbia Railroad was straightened, particularly between the city terminal and the vicinity of Bryn Mawr (Fig. 3). The old inclined planes along the steep banks of the Schuylkill were replaced by gentle grades.

Westward, beyond Bryn Mawr, the Pennsylvania Railroad followed, as a rule, the old roadbed (Fig. 3). This portion of the track was not altered nearly so much as the portion closer to the city. Also, in the western end of the area, the terminal facilities of Paoli were further developed. Rail yards were enlarged to alleviate the congestion of local commuter trains ending their runs there. Before these improvements, it usually took over two hours to get from Paoli to the city.

In the 1860's the Pennsylvania Railroad did not cater greatly to commuters. Most of the residents of the "Main Line" were farmers, and they required six trains a day each way, and none on Sundays. The picturesque trains were drawn by queer, puffing locomotives with large cowcatchers; the cars had oil lamps for light and coal stoves for heat. By 1875 traffic had increased so much that the Pennsylvania Railroad bragged of its total of sixty-two regular passenger trains entering and leaving the old West Philadelphia Station every twenty-four hours. In those days this was indeed an accomplishment.

During these early years the need for through service from New York City became increasingly apparent. By 1871 Jersey City, New Jersey, had replaced Philadelphia as the eastern terminus, with ferry service to New York. It wasn't until 1910, however, that tunnels under the Hudson provided facilities for riding directly into New York City. The railroad also expanded westward to Chicago and St. Louis. These developments had a profound influence on Philadelphia's Main Line District. They became the basis for the through east-west traffic so characteristic of transportation within the core of this region.

Development after the Early 1900's

Although the relatively high elevation made the Main Line area cooler than the city and enticed many Philadelphians to spend summers at "hotels" and "boardinghouses," not until transportational facilities were improved did people consider establishing themselves as permanent residents. Many early immigrants from the congested city were officials, directors, financiers, and industrialists closely affiliated with the railroad. They used their influence in demanding rapid improvements in commuter service. Within a few years the section was given one of the best suburban railroad services in the world. After these changes the Main Line District experienced major influxes of resident population.

One of the most significant advancements to

influence the geographic background of the Main Line area was the introduction of electrically operated trains. Noisy, smoky steam locomotives were replaced by quiet, clean electric locomotives. Trains passing through no longer belched smoke and dirt.

Then a bypassing route was constructed just to the west of the Main Line District. This route, also electrified, was called the "Trenton-Cutoff," and it followed the Chester Valley to the north of the "main route." It rejoined the main tracks near Trenton, New Jersey. The existence of this bypassing route meant that only a relatively few noisy freight trains passed through the Main Line, and dirt, noise, and congestion were minimized in the core of the Main Line Suburban Region.

Electrically operated express trains speed through the Main Line area without stopping until they get to terminal Paoli. The powerful locomotives pulling such trains are 79½ feet long, weigh 460,000 pounds, and develop 4620 horsepower.

To a resident of the Main Line the commuter train is more familiar and vital. It is called "The Paoli Local," and, in striking contrast to the express trains, it jerks along the track, stopping and starting at 18 stations in 20 miles (Table 1). It has a queer honking whistle announcing its departure, and it hardly ever attains full speed before it is time to start braking for the next station.

TABLE 1

COMMUTER STOP CHARACTERISTICS ALONG THE MAIN LINE*

Miles from Philadelphia	Names of Station Stops on the Main Line	Total Number of Trains Stopping Daily
0.0	Philadelphia (all stations)	139†
4.1	52nd Street	58‡
5.6	Overbrook (City Line)	77
6.1	Merion	75
7.0	Narberth	79
7.6	Wynnewood	78
8.6	Ardmore	84
9.3	Haverford	85
10.3	Bryn Mawr	85
11.0	Rosemont	82
12.1	Villanova	83
13.1	Radnor	84
13.9	St. Davids	84
14.6	Wayne	85
15.5	Strafford	83
16.6	Devon	86
17.7	Berwyn	86
18.7	Daylesford	70§
20.0	Paoli	139

* Information from timetables of the Pennsylvania Railroad made effective March 2, 1947. These statistics include only trains operating at least 5 days per week.

† This figure (139) includes only those trains that run along the Main Line District. There is a grand total of over 600 trains in and out of Philadelphia daily.

‡ The figure 58 includes only trains originating or terminating at Paoli; other trains from Chestnut Hill, etc., stop here too. The daily total is 89 trains.

§ Most trains stop here only on signal to receive or discharge passengers.

Characteristics of Present-Day Commuter Service

Examination of Table 1 reveals several important developments. First, the Pennsylvania Railroad attempts to provide most frequent and efficient service to station stops lying some distance outside the city limits. Stations close to central city have fewer trains stopping daily, and many of them are expresses to the central city terminal. For example, the 52nd Street Station has only 58 inbound and outbound trains daily. Even Overbrook, which is at the city line, has only 77 trains. Farther out, beyond Ardmore (8.6 miles from central city), some station stops have as many as 86 inbound and outbound trains daily. Second, the commuter stops are closer together farther out on the route. There is only one stop (52nd Street) in the 5.6 miles between city line (Overbrook Station) and central city. Farther out, some of the stops are only 0.6 mile apart. It is apparent that commuters in or near the city depend more upon local bus and trolley service.

The stops at Overbrook (city line, 5.6 miles out), Merion (6.1 miles out), Narberth (7.0 miles out), and Ardmore (8.6 miles out) are more than significant because trains often operate as expresses to and from these points. For example, an outbound train is classified as an express as far as one of these stops and then becomes a "local" or a "skip-stop express" farther out. Inbound, the situation is similar, no stops being made between these stations and the central city terminal. Excluding the early morning hours, the average main line station has a total of one train in each direction every half an hour. During the "rush hours" trains may run only a few minutes apart. Most express trains and those that skip stops are operated during these "rush hours."

The terminal function of Paoli is apparent. A total of 139 trains stop there daily (Table 1). Fifty-three of them are through passenger trains. The other eighty-six are "locals" (74) and "skip-stop expresses" (12) to the city. Often the residents of the area take the "Paoli Local" to Paoli, where they can conveniently make connections with any through train and thus avoid the congestion of mid-city. The fact that all trains stop at terminal Paoli offers an additional advantage to commuters residing near there, 20 miles from Philadelphia. They can get into the city in less than 24 minutes on an "express" (Table 2).

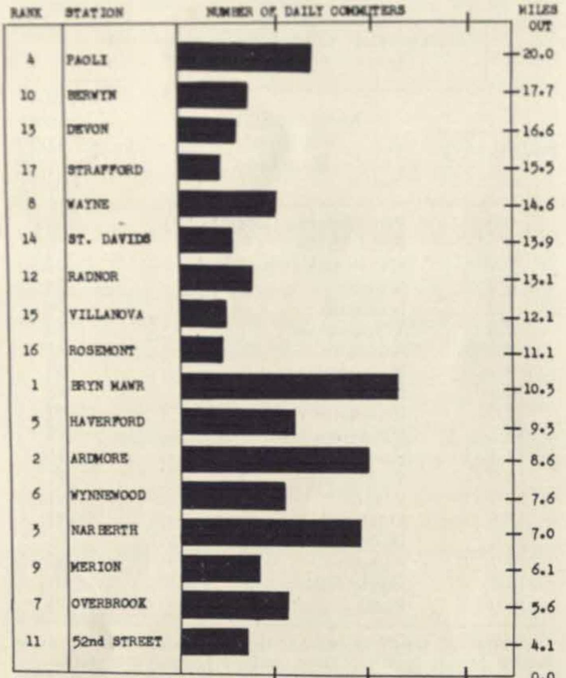


FIG. 4. Relative importance of the commuter stations according to the number of daily commuters utilizing each station stop.

Curtailed service to points in and near the city and extra service to points farther out make it possible to get to some parts of the Main Line District in a shorter period of time than it takes to get to outlying parts of the city proper by local bus and trolley service. For example, it takes about 17 minutes by the "local," 15 minutes by a "skip-stop express," and 12 minutes by an "express" to get to Ardmore, 8.6 miles from central city (Table 2).

Distribution of Commuters

To get an accurate picture of the distribution of commuters along the route of the "Paoli Local" it is necessary to compare Figures 4 and 5. The bar graph in Figure 4 illustrates the relative importance of commuter stations according to the number of daily commuters utilizing the stop. The graph is based on the number of daily passengers. The three most important stops along the main line are Bryn Mawr (2240 daily commuters), Ardmore (1950), and Narberth (1885). Paoli is in fourth place (1650), remaining important despite the great distance (20 miles) from the city because of its terminal function. The figures on the right vertical scale indicate the number of miles each stop is from the Philadelphia terminal. The most important station stops lie within the 3.3 miles between Narberth and Bryn Mawr. The only significant exception is Paoli. The least important station is a flag stop at Daylesford (18.7 miles out). It does not appear on the graph. The least important station with regularly scheduled stops is Strafford (320 daily passengers).

Figure 5 is a line graph showing the approximate volume of daily commuters passing through each station. The vertical scale indicates the relative distance between stops. The numbers show how many miles each station is from the Philadelphia terminal. The horizontal scale shows the number of daily commuters on the trains as they pass through each station. The line of the graph corresponds to the cumulative total. To get these figures it was necessary to add the number of daily commuters from each stop to the total number of passengers already on the trains.

The most significant characteristic of commutation brought out by Figure 5 is the location and distance from the city of the points at which most commuters board the trains. A significant number (about 1650) of the passengers originate at the Paoli terminus for the daily round trip to Philadelphia. Many others arrive at and depart from Paoli on express trains, some of them taking the "local" to some community down the "line." Although about 1650 commuters begin the daily trip at Paoli, relatively few passengers board the trains in the first 9 miles inbound. At Rosemont (11 miles from the city) the cumulative total has risen to only 5855 passengers. The figure quickly jumps to 8095 passengers at the next station, Bryn Mawr. From here on the increase is great at each stop to the city line at Overbrook Station. The cumulative total rises to over 16,360 daily com-

TABLE 2

RUNNING TIME OF COMMUTER TRAINS TO SELECTED STATIONS*

Miles from Philadelphia	Selected Stations on the "Main Line"	Time by Express, No Stops	Time by Skip-Stop Express	Time by Local, All Stops
5.6	Overbrook	8 minutes	—	9 minutes
7.0	Narberth	10 minutes	11 minutes	13 minutes
8.6	Ardmore	12 minutes	15 minutes	17 minutes
10.3	Bryn Mawr	—	17 minutes	21 minutes
14.6	Wayne	—	24 minutes	30 minutes
17.7	Berwyn	—	31 minutes	37 minutes
20.0	Paoli	24 minutes	36 minutes	41 minutes

* Time is computed to 30th Street Station (Pennsylvania Station). Add about 3 minutes for Broad Street Suburban Station. The stated times are approximate; some trains are a few minutes faster, others a few minutes slower. Information from timetables of the Pennsylvania Railroad made effective March 2, 1947.

muters at the 52nd Street Station, 4.1 miles from the central city terminal. It is evident that the greatest number of commuters boards the trains between Overbrook (City Line Avenue, 5.6 miles out) and Bryn Mawr (10.3 miles out). This is also noticeable when one rides the trains. Inbound trains often have only standing room for passengers boarding at stops near the city. Outbound trains are similarly crowded for the first few miles and then become noticeably "empty" beyond Bryn Mawr.

It must be understood that the "cumulative total" upon which Figure 5 is based is merely an estimate and does not include people riding *through* the area on express trains nor passengers who ride *between stops* within the Main Line District itself. Although no figures were available for Daylesford, it is listed because there is no doubt that it is the least important stop on the route.

Bus and Trolley Routes

Local electric trolley and motor bus services supplement the facilities of the Pennsylvania Railroad. Typical is the Philadelphia and Western Railway Company. This rapid-transit trolley line leads westward from the 69th Street Terminal, which is the end-of-the-line for the subway-elevated routes leading out of central Philadelphia. Its route almost parallels that of the Pennsylvania Railroad, and, in places, the two tracks are within sight of each other.

It is possible to visualize the importance of such service when one considers that a typical station stop like Villanova has a total of 118 inbound and 116 outbound trolleys daily.

Development of the Lincoln Highway

After the abandonment of the Lancaster Turnpike and the advent of the railroad, the people who had settled along the main line became thoroughly disgusted with the sadly neglected Turnpike. Furthermore the route offered by the Old Lancaster Road became a menace to the interests of the paralleling Pennsylvania Railroad. The railroad feared that street car lines would be extended from the city into the growing suburbs and cut into its local passenger business. Therefore, to protect itself from such opposition and at the same time to improve the deplorable conditions on the pike, it formed a subsidiary company, the Lancaster Avenue Improvement Company, to take over the road.

This company purchased the portion of the road extending from 52nd Street to Paoli. Its improvement and maintenance were supported by tolls. The portion in Montgomery County, renamed Montgomery Avenue, became the first macadamized road in the United States. Farther west the road was called Lancaster Avenue, a name it still retains in places.[4] In the early 1910's this stretch became a part of the famous transcontinental Lincoln Highway, which, by 1923, was considered the "Main Street" of the nation.

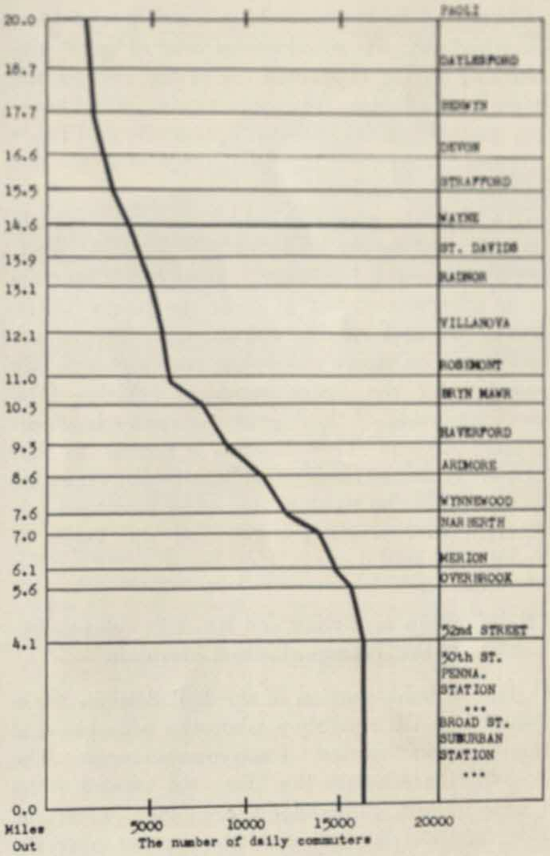


Fig. 5. The approximate volume of daily commuters passing through each station.

The development of the automobile led to the purchase of the route by the State Highway Department in 1917; this brought the end of tolls. By 1925 demands for an improved route were so great that the right-of-way was resurfaced, straightened, and widened. To accomplish this it was often necessary to shift sidewalks and slice properties. As a result sidewalks are very narrow and residences are but a few feet from the edge of the thoroughfare in many places in the core of the region today.

The present Lincoln Highway practically parallels the main line of the Pennsylvania Railroad,

as indicated in Figure 3. It is a well-paved four-lane highway. Its development was of great significance to the characteristics of the core of the Main Line District. It brought tourists, hotel keepers, garages, service stations, restaurants and made it more convenient for the commuter to live farther from a railroad station.

An observer along this highway today would be impressed by the heavy volume of traffic. During the daylight hours the passenger automobile is in prominence, but at night the heavy trailer trucks roll and rumble continuously. Trucks are prohibited on many paralleling routes, which are reserved for the faster passenger vehicles. The most important of these passenger routes is Montgomery Avenue. This situation is similar to that of the Trenton-Cutoff, that is, the objectionable freight traffic, by truck as well as by locomotive, is diverted over selected routes and the value of certain residential areas is enhanced.

Relationship of Urban and Rural Development to the Transportational Meridian

The "urban" portion of the Philadelphia Main Line District is roughly a triangular area bisected by the main east-west transportation route. The base of the triangle lies along the several miles of the western Philadelphia city limits, while the apex of the triangle extends to a point near the community of Paoli, 20 miles from central city. The remaining portions of the Main Line District are, generally, rural, and they lie farther away from the transportational meridian. With distance westward within the "urban" area, large tracts of "rural" country are wedged between the small towns. Beyond Paoli most of the countryside is of a rural nature. Within this triangular area lying along the east-west transportational meridian the distinguishing characteristics of the core of the Philadelphia Main Line Suburban Region are best displayed.

FIG. 6. A typical "gateway" leading into a hidden estate in the Main Line Suburban Region. The roadway characteristically leads uphill because many country homes are located on hilltops.

It is difficult to determine whether the residents of the Main Line are really "urban" or "rural." Perhaps it is safe to say that they are urbanites living in a rural area. As Wehrwein would probably say, they are living in the area of transition between well-recognized urban land use and that area devoted to agriculture, or in "the rural-urban fringe."

The most characteristic feature of the rural portions of the Main Line District is its park-like appearance. A ride along one of the many meandering hard-surfaced roads reveals stately trees, spacious lawns, well-kept fences, and the country estates of gentlemen farmers. "The large size of the houses and the beauty of the yards and grounds are out of proportion to the associated farming operations. The emphasis is upon the establishment of attractive homes, not upon making a living from agriculture."[5]

The traveler is often disappointed because many of the estates were planned for the purpose of providing complete privacy. From the road the "gateway" may be the only indication of the presence of a residence. The typical gateway is quite elaborate, and from it a roadway usually leads *up* to the hidden estate (Fig. 6). The paved road usually leads up because frequently the country estate is situated on the top of a hill. The inhabitants are obviously city commuters, not farmers.

Within the last few decades the tendency has been to break up the large land holdings into small building lots. The "rural country farms" and "estates" that were wedged between the "towns" have gradually become less prominent. Building lots were thus provided near transportational facilities in beautiful regions where the comforts of a rural home were available. Relatively modest homes (in comparison to the older estates) appeared.

Today it is no longer necessary for a town to be clustered about the commuter station. "Motor transport has released man from the necessity of living in places where mass transportation is available."[6] This development has made it possible for commuters to live farther from the transportational meridian. They can drive to the local station and park the car there all day. Always, however, access to the main east-west transportational route is all-important to the resident of the Main Line District.

Perhaps the difficulty of parking in mid-city and the fatigue caused by actual driving are sufficient reasons for leaving the family car out along the main line. The train ride offers an opportunity to read the newspaper and relax. This tendency to leave cars parked in stations has increased. The railroad has recognized this trend and has provided ample parking space for its patrons. Figure 7 shows a typical Main Line Station stop. The striking architectural design is found in most stations of the district.

One of the most recent urban developments has been the construction of large, beautiful apartment houses, particularly in the built-up area closest to the city. This is the climax of the trend to reduce the size of the household from the typical old estate with spacious acres to a small apartment with a few bushes or a windowbox for a landscape. Thousands of "Mainliners" today live in these apartment houses rather than in great rambling mansions. Figure 8 illustrates the typical suburban apartment house.

Relationship Between Industrial Development and Transportational Facilities

One of the outstanding characteristics of the core of the Philadelphia Main Line Region is the scarcity of industrial development. This lack of industry exists despite the fact that the region lies along a major artery of transportation where it would be possible to import raw materials and export finished products conveniently. The Main Line Suburban District has developed as an almost entirely residential area. Transportational facilities have been aimed at serving the commuter. There is a pronounced lack of sidings and other facilities needed by industrial plants.

It is rather remarkable that this area has had such a limited development of industry when one considers that it is surrounded by great industrial regions. Still more unusual is the fact that these surrounding industrial regions give relatively little employment to "Mainliners." In general, they ignore nearby areas; few residents of the Main Line are employed in industry. Instead, they are mainly businessmen, professional people, managers, officials, clerical workers, and the like employed in central city Philadelphia. The evolution of the transportational route here has certainly not fostered industry.

Influence of the Pennsylvania Turnpike

The latest influence brought to bear upon the Philadelphia Main Line District is the location of

FIG. 7. The Haverford Station on a sunny afternoon between trains. Note the parking facilities.

the route of the recently constructed Pennsylvania Turnpike. Because this highway tends to avoid all areas of congestion, it does not follow the old transportational meridian that forms the core of this suburban region. Instead, it approaches the Main Line District from the northwest and terminates in the vicinity of the community of King of Prussia within the area studied (Fig. 3).

An arterial highway is to be constructed (construction has already begun) from King of Prussia along the Schuylkill River Valley into the city of Philadelphia. Ironically enough, this route roughly follows one of the first passageways used by early pioneers in the area when they navigated the Schuylkill for transportation. Although the new highway does not pass through the congested core of the Main Line District, it continues to follow the persistent east-west trend of transportation. The route of the new Pennsylvania Turnpike reminds one of the Trenton-Cutoff of the Pennsylvania Railroad for, as it cuts through the hills and mountains of the state, it bypasses the congested areas of the towns and cities in the core of the region.

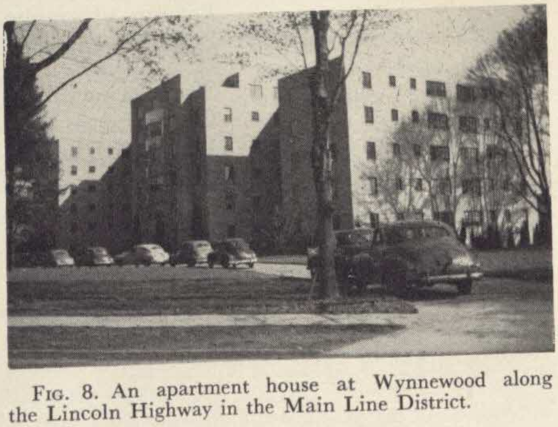


FIG. 8. An apartment house at Wynnewood along the Lincoln Highway in the Main Line District.

Summary and Suggestions

"This through route . . . exemplifies the evolution of transportation from Indian Path to pack horse trail, crude freight wagon road, stage coach turnpike, railroad and modern highway, probably better than any other single thorofare in the country today."[7] All the succeeding historical routes have, in general, followed the dominant east-west trend and have been the basis for developing the distinguishing characteristics of the core of the Philadelphia Main Line Suburban Region. Because all large cities have similar residential areas stretched along the major arteries of transportation, it is hoped that this study will inspire the continuation of research on the suburban fringe of metropolitan centers. All too often major study and emphasis are given to the political city or to the metropolitan area as a whole, and seldom are the specific characteristics of the city's suburbs considered. It is this type of overlooked area this study intends to describe.

It is suggested that studies be made of the evolution and distinguishing characteristics of the cores of other types of suburban districts. For example, in recent years there has been a tendency for industry to "decentralize" and move out of central city locations to the suburbs along some convenient transportation route. Certainly an industrial suburb would show a type of evolution different from the one portrayed here for a residential suburb located along a transportational route, and would result in the appearance of different distinguishing characteristics in its core. And what about the difference between the characteristics of the core of a suburban region with a dominantly educational function and those of a suburban area developing a commercial function with the construction of a modern shopping center in its midst? Here is a field of investigation that has been much neglected.

References

1. Faris, J. T. *Old Roads Out of Philadelphia.* Philadelphia: Lippincott, 110 (1917).
2. *Op. cit.*
3. ———. *Old Trails and Roads in Penn's Land.* Philadelphia: Lippincott, 981 (1927).
4. Browning, C. H. *Welsh Settlements in Pennsylvania.* 443–444 (1912); Townsend, J. E. *The Old Main Line,* 60–61 (1922); Faris, J. T. *Old Trails and Roads in Penn's Land,* 99 (1927).
5. Murphy, R. E., and Murphy, M. *Pennsylvania—A Regional Geography,* 201 (1937).
6. Wehrwein, G. S. In *Economic Geography,* 221, (1942).
7. Bruce, R. The Lincoln Highway in Pennsylvania (American Automobile Association, National Highways Association), Chap. 2, 29–41 (1920).

THE HILLS

The hills roll wide on every side
Like waves upon the sea,
Breakers of granite tossed through time,
Surf of eternity.

The heaving hills in dumb show shout
Of days when time began,
When earth was an unsteady star
And like quicksilver ran.

James Dillet Freeman

Lee's Summit, Missouri

What Scientists Look for in Their Jobs

THERESA R. SHAPIRO

The author received her training at Hunter College and Columbia University and has held a wide variety of government posts in the field of labor economics. This article is one of a group of studies conducted for the U. S. Bureau of Labor Statistics as part of its continuing program of studies of scientific personnel.

IN this period of personnel shortages in the sciences, when the rate of turnover among scientific personnel is unusually high, it is pertinent to inquire what it is that scientists ask of a job. Employers who need to attract and retain competent scientists have the most obvious interest in the answer to this inquiry, but it is of concern also to scientists themselves. Young scientists, in particular, who are at an early stage of their careers, must make what may prove to be critical decisions as to where to work. On what basis should they choose among the many offers they are likely to receive? Should they seek the highest salary, the greatest promise of security, the most interesting work? Which will in the long run contribute the most to a satisfactory working life? Some light is thrown on these questions by a study of job satisfactions and dissatisfactions among scientists, carried out by the Bureau of Labor Statistics of the United States Department of Labor, as part of its program of studies of scientific personnel.

It should be said at the outset that any study of employees' attitudes toward their jobs which, like this one and almost all others, is based on interviews has certain inherent limitations. For one thing, pleasure or displeasure in a job situation is usually the result of several interrelated factors. The particular factor a person happens to mention to an interviewer months or years later may actually have been of secondary importance in the bygone situation. Moreover, people are not always aware of all the factors influencing their decisions to accept or leave a position, and they tend to ignore those outside the recognized and socially acceptable categories of values. Even when an interviewee remembers and understands his motivations, there is danger that he will not transmit them to the interviewer. Confronted by a strange face, no matter how sympathetic, most of us find it easier to retreat into stereotyped phrases or vague generalities than to talk frankly.

The story of Mr. Brown, a chemist, illustrates these points. Mr. Brown worked as a research chemist under the supervision of Mr. P., whom he liked and respected. One day Mr. P. left the company. Mr. Brown, who had hopes of inheriting Mr. P.'s position, found Mr. P.'s successor uncongenial and the supervision he exercised chafing. His salary seemed inadequate, now it was clear that it would not be raised. His work began to appear trivial and dull. For the next six months, however, Mr. Brown did nothing about all this except grumble to his wife. Then, one day, he ran into a former classmate, Mr. K. Always an optimistic fellow, Mr. K. was very enthusiastic about his job, his firm, and his colleagues. He knew of an opening and suggested that Mr. Brown apply for it. They made an appointment. Mr. K. introduced Mr. Brown, not only to the chief but also to several other men. The chief reminded Mr. Brown of his old boss, Mr. P., and the other men seemed very friendly. When Mr. Brown was offered a job with the ABC Company, at an increase of only $300 a year, he was delighted to accept.

Why did Mr. Brown become so dissatisfied with his job that he moved over to the ABC Company? The explanation he gave the interviewer was that he considered Mr. P.'s successor inferior and that he wanted more money. Reading between the lines, we are inclined to believe that it was the disappointment of Mr. Brown's hopes for a promotion and his chance meeting with his old classmate which were most responsible for his transfer.

If, then, interviews cannot be relied on for a complete and an objectively accurate analysis of the reasons why people like a given job, of what value are they in studying job satisfactions? The answer is that they furnish information as to employees'

conscious motivations, particularly those that conform to the attitudes held in common by the group to which the workers belong. Since these attitudes have a decided influence on behavior, information concerning them is valuable to employers, personnel workers, and scientists themselves.

The insight into job satisfactions and dissatisfactions obtained through interviews is greatest when the group studied is homogeneous and cohesive. Scientists with Ph.D.'s, the subject of the present study, are such a group. They are knit together by a common background of training, often in the same schools, by membership in the same scientific societies, by a community of interest in science, and by many personal and professional relationships.

How the Study Was Conducted

This study is based on interviews conducted between December 1950 and June 1951 with 407 men physicists, biologists, and chemists, all of whom held doctorates. The interviews were arranged through the cooperation of various private firms, government agencies, and colleges and universities located on the eastern seaboard from New York City south to the District of Columbia. Some additional scientists were interviewed at the 1950 meeting of the American Association for the Advancement of Science, in Cleveland.*

Each scientist was asked to report his employment history from January 1, 1939, to the date of the interview. During this period, the 407 men in the study had made 574 job shifts, apart from those due to service in the Armed Forces. Since some of the men were still in college in 1939, the number of job entrances reported was greater (670) than the number of job exits (574). The average number of job entrances per scientist was 1.6, of exits 1.4. Most of the job shifts occurred while the scientists were still young; 85 per cent of the exits were made before their fortieth birthdays.

The approach used to determine the attitudes of these scientists toward their jobs was to inquire about the reasons for their job decisions. Each scientist who had changed employers since January 1939 was asked why he had left the jobs involved and why he had favored one offer over another in accepting a new position. Those who had not changed jobs since January 1, 1942, were asked why they had remained on the same jobs. It can be argued that these questions are an indirect way of studying job satisfactions. They have the value, however, of focusing on concrete situations and on attitudes which are, in a broad sense, operational.

The men were encouraged to talk freely, and the replies were coded after the interviews were completed. In most cases, the scientists felt that no one factor determined a particular decision. Some recalled as many as five different aspects of a job situation which had played some part in their decision to remain or to leave. In order to prevent the more talkative men from unduly influencing the statistical results, no more than three reasons were recorded or tabulated in connection with any job entrance or exit or for any scientist who had not changed positions.

Involuntary Job Shifts

The decision to stay on a job or leave it, as well as where to work, generally lay with the scientists themselves. Only 18 of the 155 scientists who had remained on their jobs for at least 8 years reported that they had had no other offer or none worth considering during this period. Only 67 of the 574 job exits were due to factors over which the scientists had no control, and 28 of these resulted from the termination of war projects. Furthermore, the scientists were rarely forced to accept a job for lack of another offer; this was the case for only 75 of the 670 job entrances covered by the study. Very likely, the fact that the scientists were able to choose between job offers was at least in part due to their practice of continuing in a position while shopping for a new one: they rarely left a job without having another one lined up.

The larger measure of job choice available to Ph.D. scientists than to many other occupational groups is suggested by a comparison of these findings with those from a study of a sample of the labor force in St. Paul, Minnesota, during about the same period (1940–50). In that study, close to a third of all the job terminations, and a fourth

* It was not practicable to select the scientists to be interviewed on a random basis. However, chemists, physicists, and biologists were represented in about the same proportion as among all the nation's Ph.D.'s in these three sciences. The age distribution of the respondents in each field also was similar to that of all Ph.D.'s in the given field. However, the relative numbers employed in colleges and universities were considerably lower and the proportions in private industry and government higher among the respondents than among all Ph.D.'s. Furthermore, few men in the study were employed outside the Middle Atlantic states. Despite these limitations, the attitudes expressed by the group of scientists interviewed are probably held by most Ph.D. chemists, biologists, and physicists, since Ph.D. scientists form a cohesive group, characterized by considerable shifting between university and other employment and by a high degree of geographic mobility, and since both these characteristics were found among the men in the study.

of those made by men in professional and kindred occupations, were found to be involuntary.*

Reasons for Job Decisions

The reasons given by the scientists for voluntarily leaving a job, for preferring one offered position to another, or for remaining on a particular job despite opportunities elsewhere are shown in the tables. In all three types of decisions, the considerations uppermost in the minds of most of these scientists were the interest of the work, the earnings and opportunities for advancement it provided, and the working conditions on the job. Less often mentioned in connection with these job decisions, but nevertheless determining in some situations, were such factors as security, personal considerations, the war effort, and the pursuit of graduate studies.

Job interest. This factor was the one most often given for continuing in a job for a long time (Table 1) and for choosing a particular job (Table 2). It ranked after earnings and opportunities for advancement, however, among the reasons cited for leaving jobs (Table 3).

What does a scientist mean when he says his job is interesting? What makes it "worth while and challenging"?

Many scientists stressed the importance of their work in extending the boundaries of science or in contributing to human welfare:

> "I have challenging problems. There is a chance for extending the whole field of chemistry. That is to say—does the theory built on the basis of small molecules apply to large molecules? Polymer chemistry is related to living matter; it is the riddle of life. Polymers have a tremendous technological significance; next to metals they are the most important materials."
>
> "The objectives are not generally as broad elsewhere. You can contribute more here. It means more to a larger number of people."

Others emphasized the opportunity for concrete achievement:

> "When you work for industry, you are at the forefront of research. Industrial research is done by a team possessing various skills. The team member can accomplish more than the individual scientist in the university."
>
> "Many universities don't do research except of the ivory tower type. I prefer to see the results of my research. It gives me a great thrill every time I see a package of 'Dot' on the grocery store shelf because I mixed the first batch of it."

For some men, it was the opportunity to develop and grow with a problem which was most important.

* University of Minnesota Industrial Relations Center, "The Voluntary Shifts of St. Paul Workers, 1940–44 and 1945–50," unpublished manuscript, p. 6.

TABLE 1

REASONS CITED BY SCIENTISTS FOR REMAINING ON SAME JOB SINCE JANUARY 1942 OR EARLIER

Reason	Reasons cited: Number	Reasons cited: Per cent distribution
Total reasons reported	**399**	**100.0**
Job interest	**132**	**33.1**
Worth-while and stimulating work	75	18.8
Work in scientist's field of specialization	18	4.5
Variety of problems	39	9.8
Working conditions	**128**	**32.1**
Total environment good	26	6.5
Freedom and independence	31	7.8
Good staff and supervisors	26	6.5
Good employer	28	7.0
Good research facilities	17	4.3
Earnings and advancement	**74**	**18.5**
Good salary or salary increase	36	9.0
Promotions	23	5.8
Pension program or other fringe benefit	15	3.7
Personal reasons	40	10.0
Security	16	4.0
Other reasons	9	2.3
Number of scientists reporting	137*	

* Excludes 13 scientists who did not report a reason for remaining and 18 who had no alternative offer worth considering.

> "I have been developing the same problem during my twelve years stay at ———. I have no desire to change problems as long as progress is being made."
>
> "It takes 10 years for a good research job to mature. The problems are such that it takes years to work out. You keep going till you have extracted the juice."

Another job interest factor, most often mentioned in connection with the choice of a new job, was the chance to work in one's specialty, to grow and develop in this field:

> "The work represented a continuation of the research I had been doing for my doctor's thesis."
>
> "I chose this job because it was a chance to utilize my previous experience."

Interest in a particular field of specialization is not incompatible with a wish for new problems and broader experience, mentioned fairly frequently as a reason for changing employment. Nevertheless, some scientists accepted a particular job or continued in it because they wanted the even broader experience of work in several specialties.

> "I have remained with the company because of its diversity of interest. I like to move from one field to another. With the X Company the opportunity to shift fields is combined with the advantages of remaining with the same firm."
>
> "The Blank Company interviewed me in regard to

many different specialties. I wanted to make myself flexible, so I chose their job."

Working conditions. In explaining their continued employment with a particular organization, the scientists who remained on the same job for many years mentioned the working environment almost as often as the work itself (Table 1). A considerable number of scientists also gave careful consideration to the prospective working atmosphere in choosing a job (Table 2). There were relatively few instances, however, in which active dissatisfaction with the working environment was mentioned as a reason for changing jobs. Like professional employees in general, scientists rarely work in physically unpleasant surroundings and are seldom subject to close supervision. This is undoubtedly one reason why unsatisfactory working conditions play a much smaller role in scientists' voluntary job exits than in the quits of manual workers.*

It is always difficult to single out the one element of the total job environment which pleases or disturbs. A number of scientists declared that many elements—freedom and independence, the research facilities, their supervisors and colleagues, the firm's policies and administration—together created a good or bad atmosphere for scientific work. The poor quality of the supervision and administration was the specific factor most often cited as a reason for leaving a job (Table 3). The caliber of their prospective co-workers and employer (either the individual organization or the type of organization) influenced more scientists in choosing a position than any other environmental factor (Table 2). In the case of scientists who had long remained with the same employer, freedom and independence led among the reasons for job satisfaction grouped under "working conditions" (Table 1).

Like all evocative words, freedom means different things to different people. For most of the government and industry scientists, it meant a voice in selecting their research problems:

> "One problem led to another, and I was allowed to follow my own inclinations. There has been more freedom than I could have anticipated elsewhere."
> "It is a new field and you pick your own topics. There is more freedom than there would be in old fields. No one can tell you what to do."

University teachers, who more often mentioned freedom and independence as sources of satisfaction than either government or private industry scientists, usually defined freedom more broadly:

> "University work offers many kinds of freedom—freedom of research, freedom of life, freedom from security investigations, and freedom to express oneself. It also provides freedom of time."

What these scientists stressed in assessing their supervisors and associates was competence in scientific work rather than administrative ability or humanity:

> "I like my associates. They are the top men in their scientific fields. It is a pleasure to work with them."
> "I admired and respected Dr. G., the head of the project. I consider him one of the most outstanding biochemists in the country."

In discussing their reasons for satisfaction with a particular university, business firm, or government agency, some scientists emphasized the organization's reputation for high-caliber scientific work, valued not only for the pride they could take in their work but also for the contribution the firm's

* The findings of the University of Minnesota Study cited above suggest that the proportion of job quits due to unsatisfactory working conditions increases as one goes down the occupational scale (Appendix tables A-9 and A-10).

TABLE 2

REASONS CITED BY SCIENTISTS FOR CHOOSING NEW JOBS, 1939–1951

Reason	Reasons cited: Number	Per cent distribution
Total reasons reported	**950**	100.0
Job interest	**308**	32.5
Worth-while and interesting work	50	5.3
Work in scientist's field of specialization	100	10.6
Interesting activity (teaching, research, or administration)	97	10.2
New or broader experience	61	6.4
Working conditions	**153**	16.1
Total environment attractive	22	2.3
Freedom and independence	29	3.1
Good staff and supervisors	44	4.6
Preferred employer	31	3.3
Good research facilities	27	2.8
Earnings and advancement	**229**	24.1
Salary increase or best salary offered	118	12.4
Good prospects for promotion	62	6.5
Earnings and chance for advancement generally good	49	5.2
Personal reasons	84	8.8
To study	84	8.8
Work close to defense effort	57	6.0
Security	10	1.1
Other reasons	25	2.6
Number of job entrances covered by table*	571	

* The 950 reasons for choosing jobs analyzed in this table applied to 571 voluntary entrances. The tabulation excludes 24 job entries for which no reason for change of job was given and 75 where the scientists had only one job offered them.

TABLE 3

Reasons Cited by Scientists for Leaving Jobs, 1939–1952

Reason	Reasons cited: Number	Reasons cited: Per cent distribution
Total reasons reported	**558**	**100.0**
Job interest	**118**	**21.1**
General dissatisfaction with work	12	2.1
Not interested in field of employment or function	29	5.2
Desire for new or broader experience	47	8.4
Insufficient opportunity for research	30	5.4
Working conditions	**62**	**11.1**
Dissatisfaction with total environment	22	3.9
Insufficient freedom	6	1.1
Poor supervision	13	2.3
Bad administrative policy	14	2.5
Inadequate research facilities	7	1.3
Earnings and advancement	**131**	**23.5**
Inadequate salary or better offer elsewhere	79	14.2
Insufficient opportunity for advancement or better opportunity offered	52	9.3
Personal reasons	50	9.0
To study	67	12.0
War-connected reasons	73	13.1
Insecurity	43	7.7
Other reasons	14	2.5
Number of job exits covered by table*	406	

* The 558 reasons for leaving jobs analyzed in this table applied to 406 voluntary exits. The tabulation excludes 67 involuntary job exits, 94 due to the completion of studies, and 7 for which no reasons were reported.

reputation was expected to make in furthering their later careers. Others said they enjoyed the atmosphere of an efficient organization. A few scientists who had experienced frustration working for a large organization, pointed out the advantages a small one has to offer—a high degree of personal responsibility for the work and close ties with other members of the staff.

Implicit in every evaluation of an organization as a "good employer" was satisfaction with the equipment and assistance provided the research scientist. Some scientists, however, singled out the research facilities as one of the most important aspects of a job. One man said, "I have turned down other offers, including an offer to head up a cancer laboratory, because I believe that the Blank Institute offers the best facilities for cancer research."

Earnings and advancement opportunities. These essentially economic considerations played a greater part in the scientists' decisions to quit jobs and in the selection of new ones than in their long-continued employment with the same employer. Of all the reasons cited for job exits and entrances, about one-fourth related to earnings and advancement opportunities.* However, less than one-fifth of the men who had held the same job for a long period mentioned these factors as reasons for their employment stability.

The amount regarded as a good salary depends in the case of scientists, as for all of us, on a variety of considerations. Prevailing salary rates, the scientist's standard of living and family obligations, his expectations in a particular organization, the long-term earnings outlook, the professional status connected with the salary, and kind of offers received from other firms, all entered into the definition of a good salary. As the following quotations suggest, the consideration stressed in a particular situation depended both on the situation and the individual scientist.

"The job I took paid $2000 a year—more than the going rate. That doesn't seem like much now, but some of my classmates made only $1600 or $1800.

"I have four children and need an industrial salary."

"I decided to leave the teaching field because the pay was poor."

"A promised increase wasn't granted. So I left."

"I didn't want to leave, but I was offered a thousand dollars more."

Past experience and future prospects, as the scientists saw them, also entered into their assessment of advancement opportunities. A number of men stayed on the same job despite offers elsewhere because they had been promoted rapidly: many left because they had not been advanced. In other instances, the decision hinged not on the past rate of promotion nor even on the outlook for the immediate future but rather on the long-run prospects. A scientist employed by a leading manufacturer of chemicals commented in explaining his transfer into that firm: "There was a chance for advancement because the company was already sold on research and was a big company."

Other factors. Besides the factors already discussed, the scientists mentioned job security, personal considerations, the war effort, and the pursuit of graduate studies as reasons for some of their shifts in employment.

The war was the most important cause of voluntary job exits between January 1, 1942, and August

* The younger men were particularly concerned with salary and/or advancement in choosing a job. After 45, other considerations were given greater weight.

31, 1945. During the war years, many scientists—for patriotic reasons, to avoid being drafted, or because their jobs were threatened by war-created circumstances—transferred into defense work. After VJ-Day, there was a counter-movement into normal civilian activities.

Although few job exits or entrances were due to personal concerns—such as home ownership, family obligations, and the educational and cultural facilities of a community—they were the principal cause of some job shifts. One young man, for example, accepted a job because it was the only one offered him in his home town and he wanted to be close to his family. Another scientist left a position at the first opportunity because he regarded the schools in the locality as inadequate. A third transferred out of a field of work which had been his main interest for many years because his employer moved to another city, which he found overcrowded and otherwise "intolerable." Personal concerns were somewhat more frequently mentioned as reasons for leaving jobs by the men over 35 years than by the younger ones.

Job exits for purposes of study were largely confined to men under 30 years. The pursuit of a doctorate was a central consideration in the lives of the young men, however, accounting for a fifth of all the job exits made under 30, and is therefore of considerable importance in explaining the relatively high mobility of young scientists.*

Some Concluding Remarks

In summary, the focal point in these scientists' attitudes toward a job was the wish for worth while and interesting work. Inevitably, standards as to what is worth while and interesting vary in accordance with individual temperaments. By and large, the men were most interested in attacking a challenging research problem, preferably in their own specialty, and in making a contribution either to knowledge or to humanity. This aim took for granted a certain degree of independence on the job and the equipment and assistance required to carry it out. The scientists also looked for an atmosphere conducive to scientific research—an atmosphere in which capable and sympathetic supervisors and colleagues and a management committed to the support of a research program all played a part.

All this is not to say that scientists are above ordinary human considerations. They sometimes chose or left jobs for family or personal reasons. In the war period, a number of the young men shifted into war work in order to avoid the draft. And at all times, the desire for better earnings and advancement was at least as important in the decision to leave a job as any other single factor.

Some of the emphasis on interesting work noted here arose from the fact that during most of the period under review there was a shortage of scientists. Jobs were plentiful and salaries kept rising. Hence, scientists were in a position to look for interesting work. In part, however, this concern with the nature of the work can be expected of scientists, regardless of economic circumstances. In a study of scientists and engineers who had voluntarily left their government jobs in 1948,† the respondents were asked to list the professional and personal conditions which make a position desirable. Four out of five mentioned the opportunity to do interesting and important work under conditions of freedom and responsibility. The proportion who singled out adequate compensation and opportunities for advancement as essential requirements of a good job was smaller (60 per cent). Like all professional people, scientists tend to identify themselves with their work and to evaluate themselves primarily in terms of progress and achievement in their field.

* A number of young scientists had held assistant teaching posts while graduate students. When they received their doctorates, they left these positions. The 94 job exits due to the completion of studies are, however, omitted from the tabulation of reasons for voluntary job terminations.

† Clark D. Ahlberg and John C. Honey, *Attitudes of Scientists and Engineers About Their Government Employment*, Vol. I. Syracuse, N. Y.: Syracuse University, p. 37, (July 1950).

Allegory Within Allegory

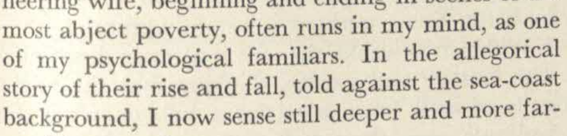
KARL P. SCHMIDT

Karl Patterson Schmidt is Chief Curator of Zoology at Chicago Natural History Museum, where he has been a member of the staff since 1922. A native of Illinois, he was educated at Lake Forest Academy and College and at Cornell University. His more significant education, he feels, came from a Wisconsin farm and from his museum career in New York and Chicago. He regards folklore as one of the proper spheres of interest of a naturalist.

FOLKLORE at its best seems to be the product of a characteristic environment that provides atmosphere and background for the story. The more intimately the dark forest, the robber society, or the sea coast are involved in a given tale, the greater will be its measure of poetic truths. In the Story of the Fisherman and His Wife (in Low German) the Brothers Grimm found an extraordinary combination of moral allegory with Arabian Nights fantasy, set forth against a background of the moods of the North Sea. The lives and fortunes of those who live by the sea and from it may depend so intimately on even the faint nuances of the relations of water and weather that it is appropriate and meaningful that these relations should appear in their regional folklore.

The story of the humble fisherman and his domineering wife, beginning and ending in scenes of the most abject poverty, often runs in my mind, as one of my psychological familiars. In the allegorical story of their rise and fall, told against the sea-coast background, I now sense still deeper and more far-reaching meanings.

The gentle shelving slopes of the coasts of the North Sea are uncovered by the retreating tides as flats that may be many miles in width, on which



the rich variety of animal life must adjust itself to being uncovered by the twice daily ebb of the tides. All the living creatures of the great marine habitat are then dependent on the fresh supplies of food brought in by the returning flood. Such broad tidal flats have been veritable theaters of evolution, and for these the great types of marine animals have all provided actors whose adjustment to their roles has become perfected in geologic eras of the order of a billion years. Here on the tide-flats there were, in ages past, forerunners of the backboned animals, and it is not impossible that the earliest of our ancestors to depart from the invertebrate type may


Von dem Fischer un syner Fru.

Da wör mal eens en Fischer un syne Fru, de waanten tosamen in'n Pißputt, dicht an der See, un de Fischer güng alle Dage hen un angeld: un he angeld un angeld.

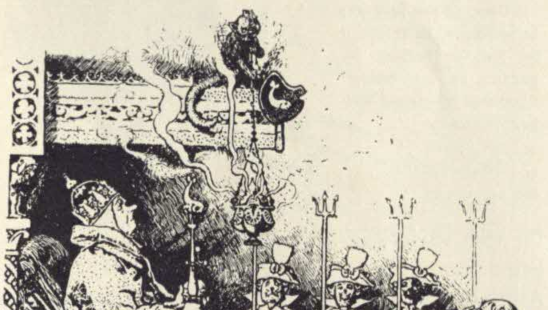
So seet he ok eens by de Angel und seeg jümmer in dat blanke Water henin: un he seet un seet.

have found the opportunity and the stimulus to do so in their progressive adjustment to the everlasting succession of tidal change.

The story to which I refer suffers a good deal in translation. In brief, it tells how a fisherman and his wife lived in such impoverished circumstances, close by the sea, that their hovel could only be described as resembling a chamber pot. How, in his daily angling, the simple and humble fisherman caught a halibut that spoke to him in Low German, begging for its life with the plea that it was no true halibut but a bewitched prince. The fisherman needed no further evidence than the halibut's command of language to convince him that such a creature should have its freedom. Parenthetically I must remark that the usual English version of the story is unhappily weakened by the substitution of the word flounder for halibut. The halibut is indeed a flatfish, with both its eyes on the same side of the head, like the flounder; but whereas the flounder is familiar and ordinary, the halibut is from much deeper waters and is ten times as large. Before the days of modern fishing the halibut must have seemed, by comparison, a veritable monster of the deep.

On his return to their hut at the end of the day, the fisherman told the story of the capture of the bewitched halibut to his wife. She is aghast at his simple generosity in giving so remarkable a creature its freedom without demanding a reward. Against his will, the fisherman is driven by her scolding to return to the sea, to call up the halibut, and to ask the exchange of their hovel for a decent cottage with a modest garden. When he had left the sea, it had been in its most peaceful mood, with little wavelets lapping the shore and a play of pleasing blue on the distant deeper water. When he unwillingly returns with his wife's demand, the sea is ominously calm, and the peaceful blue has given way to greens and yellows. The halibut responds to the fisherman's conjuring verse, and the request for better living quarters is instantly granted.

Within two weeks, the wife is already dissatisfied with cottage and garden, thinking that she might quite as well have asked for a nobleman's castle and rule over the whole countryside. When she drives her husband back with this new demand, he finds the sea all gray, the wind rising, and the waves capped with white as far as the eye can see. Again he calls up the halibut, and again the wish is granted. Now, however, inordinate ambition has seized the wife. She sends her husband back, first with the demand to be made king and ruler over the whole country, then to be made emperor, and at last even pope, with rule over all the temporal princes of the world. On each return the storm has risen to a higher pitch, and when the halibut establishes the wife as pope, the fisherman is barely able to make his way against the wind. One must refer to Hermann Vogel's interpretation of the fisherman's wife Isabel in the role of pope to appreciate how simply this incongruous situation is faced in the story.



After these triumphs, even rule over the most powerful princes of the world is no longer enough for Isabel. She again drives her husband back to

call up the halibut and demand that she be made like God himself, with rule over the sun and moon and stars.

At the coast our fisherman now finds the storm so violent that he can scarcely stand. Houses and trees are being blown over, and even the mountains shake with great rocks rolling into the sea. The sky is inky black, lit up with flashes of lightning that are followed by rolling crashes of thunder. The sea had risen into dark waves as high as church steeples, comparable even with mountains, every one capped with a vast white crest of foam. Against the screaming gale the words of the little conjuring verse are soundless, whipped from the fisherman's lips with the driving scud from the towering waves. Even so the halibut again appears; but now it is to send wife and fisherman alike back to their original chamber-pot hovel.

A grim and terrible thought has not escaped the minds of the very leaders in modern man's rise to godlike command over the forces of nature. They see, as if written in lightning flashes against a black sky, that still another backdrop remains to be rung down if the fisherman's story be brought up to date, and that the tide-flats might then appear as empty of vertebrate life as when they were washed by pre-Cambrian seas.

Perhaps, even, sea and life might be gone altogether, man's final success (as was long ago suggested) having been announced to the universe in the birth of a new star.

THE FISHERMEN

Both lie and love
    mark Peter's clan
They lie about the fish
                they've lost
    their faces scorched
    their aching backs
    their wet and weary feet
    their empty creels
        to hide their love
            and guilt of love
    of having lived
        in fleeting fellowship
            with flight
            of wing
            and wind
            and cloud
    of having known
        the passage of a breath
                no more
            the cadence of free water
            the grace of growth
            the singing of the spinning sphere

JOSEPH HIRSH

New York City

Self-Regulatory Behavior after Extirpation of Certain Endocrine Glands: A Review

FRANK B. STRANGE

The author was born in Portland, Oregon, and received A.B. and M.A. degrees from the University of Portland, at which university he was later instructor in psychology and education. He is completing his requirements for a Ph.D. degree in clinical psychology at Washington University where he holds the appointment of Fellow in Medical Psychology in the Department of Neuropsychiatry of the School of Medicine.

IN RECENT YEARS much attention has been directed toward the self-regulatory functions of the body. Probably the most outstanding worker in this area has been Walter B. Cannon, who suggested the term "homeostasis" to signify the constant conditions maintained by the body, conditions which may vary but which are relatively constant. Although the term is new, Cannon points out that the ability of living beings to maintain their own constancy has long impressed biologists, and he lists several scientists who have given expression to this thought: Hippocrates (460–377 B.C.), Pflüger (1877), Leon Fredericq (1885), and Charles Richet (1900). Cannon has directed his research in this matter, as summarized in *The Wisdom of the Body,*[1] mainly to study of the relation of the autonomic system to the self-regulation of physiological processes.

Another investigator who has contributed to the knowledge of self-regulatory functions is Curt P. Richter, who has approached the field in a different manner, through the study of endocrine glands. Writing in 1941,[2] Richter pointed out that only recently was it realized that the ability of animals to satisfy internal needs under a great variety of normal and pathological conditions by appropriate activity could be understood in terms of a broad biological concept first stated in 1859 by Claude Bernard—the concept of constant internal environment. Richter indicated that Bernard and Cannon have laid stress upon some of the single factors which help to maintain a constant internal environment. "In addition," he states, "we believe that the ability of animals to maintain a constant internal environment, after certain single organs or physiological regulators have been removed, clearly establishes the existence of behavior or total organism regulators."[2]

It is the purpose here to review research related to the self-regulatory behavior of animals following removal of certain endocrine glands. Since the great majority of research in this area has utilized the rat as the experimental animal, and for the sake of unity, only experiments using the rat will be reported. Studies concerned with the following will be considered: (*a*) the relationship between aspects of behavior and body temperature following hypophysectomy; (*b*) the relationship between aspects of behavior and water balance after extirpation of the posterior lobe of the pituitary; (*c*) the relationship between aspects of behavior and the sodium concentration of the blood following adrenalectomy; (*d*) the relationship between aspects of behavior and the calcium concentration of the blood after parathyroidectomy; (*e*) the relationship between aspects of behavior and diabetic symptoms following pancreatectomy.

Body Temperature

The chief regulators of body temperature are the thyroid and the anterior lobe of the pituitary. Death will result within a few days if animals from which either of these glands has been removed remain in even slightly lowered external temperature.[2] Kinder[3] has reported that nest building is inversely related to environmental temperature. Animals that were given access to paper used little and made small and loosely constructed nests at room temperature. Nest building activity was greatly increased as was the quality of nest con-

struction when the temperature was lowered. The behavior of the rats served apparently to maintain a constant body temperature.

Employing a modification of cages used earlier by himself[4] and by Kinder for studying nest building in the rat, Richter[5] found that hypophysectomy produced a marked change in nest building activity. The animals were kept in individual compartments measuring 12 by 11 inches with a meshed wire screen covering the front. A device was attached to the outside of the cage which consisted of an axle with a roll of soft tissue paper one-half inch wide by 500 feet long and a cyclometer for measuring the length of paper taken from the roll The end of the paper hung freely inside the cage within easy reach of the rat. The cyclometer was read at noon each day at which time the paper pulled in by the animal during the preceding 24-hour period was removed. Even though the paper was taken from the cage, normal rats, both male and female, built nests each day. Under constant conditions the amount of paper used from day to day remained "amazingly" constant for males and females alike.

It was found that hypophysectomy regularly produced a large and permanent increase in the nest building activity. Effects on nest building of gonadectomy, adrenalectomy, and thyroidectomy were also studied. Small increases were produced by adrenalectomy and gonadectomy, whereas large and permanent increase equal to that resulting from hypophysectomy was regularly produced by thyroidectomy. It was thought that the increased nest building activity after extirpation of the pituitary was due to the atrophy of the thyroid which it caused. Richter states that the hypophysectomized rats build larger nests in an effort to keep up their normal metabolism.

Referring to this study, Richter comments at a later date, "Thus, after thyroidectomy or anterior lobectomy and the resultant decrease in heat production has threatened the maintenance of a constant body temperature, the rats themselves made an effort to maintain a constant body temperature by building large nests around their bodies, thereby reducing heat loss."[2]

Water Balance

One of the main regulators of water balance of the body is the antidiuretic hormone which is secreted by the posterior lobe of the pituitary gland. Extirpation of the posterior lobe results in the displaying by the animal of the symptoms of diabetes insipidus, i.e., abnormally severe thirst (polydipsia) and excessive urination (polyuria).[6] These difficulties are relieved promptly by injection of an extract from the posterior pituitary.[7] Dehydration and death will follow, however, if the animals receive neither more than the normal amount of water nor hormonal injections. When dehydrating factors operate on the intact animal, an increased amount of the antidiuretic hormone is secreted which causes the kidney to excrete less urine, thus the body loses less water. On the other hand, when the body has an oversupply of water, hormonal secretion is decreased resulting in greater excretion of liquid.[8]

Drawing from a previous experiment,[9] Richter notes that if rats from which the posterior lobe has been surgically removed are allowed free access to an unlimited supply of water, they will ingest sufficiently large amounts to keep themselves alive and in good health. "The increased water intake," summarizes Richter, "represents to us an effort of the animal to maintain a constant internal environment, compensating for polyuria, which is primary."[2]

In a more recent report, Richter[8] indicates that experiments in his laboratories have shown that when rats from which the posterior lobes have been removed were not given access to water, they lost up to one-sixth of their body weight within eight hours after the operation and died much sooner than did intact animals that were deprived of water. When the operated rats were given access to water they consumed very large amounts, some drinking every day nearly double their body weight, and as a result kept themselves alive and in good health indefinitely.

Sodium Balance

Adrenalectomized animals show an increase in the urinary excretion of sodium and a decrease of the sodium concentration of the blood.[10] Young[11] has noted that rats usually die from excessive loss of salt in 10 to 15 days after adrenalectomy. It has been shown by Gaunt, Tobin, and Gaunt[12] that the length of survival of adrenalectomized animals is increased if they are forced to ingest a sufficient amount of salt. And Rali[13] has demonstrated that survival following adrenalectomy is enhanced by feeding sodium chloride

In an experiment by Richter,[14] 13 adrenalectomized rats were put on a saltless diet following the operation. The survival rate was found to be zero per cent and the average length of life after adrenalectomy to be 11 days. With 26 animals continued on the standard McCollum diet (approximately 0.145 gram of salt per day) after adrenalectomy, the survival rate was 39 per cent and the average duration of life of the animals that died was 17 days. A third group of 13 rats was kept on the

standard diet but in addition was given the choice of tap water or 1 per cent salt solution. These animals ingested a larger quantity of the salt solution after adrenalectomy, and the survival rate was increased to 69 per cent. Yet another group, made up of 5 rats, was kept on a saltless diet but was given a choice between tap water and a 3 per cent salt solution. Six times as much salt solution was ingested after adrenalectomy, and a survival rate of 80 per cent was shown. Richter concludes that the salt appetite is greatly increased by adrenalectomy and that the survival rate is greatly increased by virtue of the appetite.

In another study with adrenalectomized rats, using the single choice method, Richter and Eckert[15] found that the mortality, in general, varied inversely with the intake of sodium, with a reduction from 100 per cent to zero per cent. This study and also one by Young and Chaplin[16] have strongly supported the view that the adrenal gland regulates a specific hunger for sodium rather than sodium chloride. Bare[17] has also reported an increase in the strength of salt drinking behavior of adrenalectomized rats, as have Richter,[18, 19] Clark and Clausen,[20] and Mark.[21] In the general conclusions of a rather recent article, Young and Chaplin state, "If adrenalectomized rats are given unlimited access to salt solutions at the optimal or near optimal concentration, they can survive indefinitely and in seemingly good health and activity."[16]

Calcium Balance

A hormone (parathormone) secreted by the parathyroid controls the concentration of ionic calcium in the blood. Removal of the parathyroid stops the flow of that hormone which in turn causes the calcium concentration of the blood to drop below normal. This affects the central nervous system, produces increased irritability of the peripheral nerves, and results in tetany which can be cured by an extract of parathyroid glands.[10] The conclusive state can also be checked by injection of calcium salts, and if administration of calcium is in sufficient quantities, all the symptoms of tetany can be held in check.[7] However, if no treatment is given, the parathyroidectomized rat ordinarily loses weight, develops symptoms of tetany, and dies.[2, 11] The parathyroid maintains the phosphorus as well as the calcium balance in the blood, but there is a rise in the level of phosphorus in the blood and, if administered in the diet of animals after removal of the parathyroids, results in or increases tetany as the case may be.[22] It is also known that parathyroid tetany improves under treatment with strontium[23] and magnesium[24] salts.

In a study to determine the level of calcium appetite in rats before and after parathyroidectomy, Richter and Eckert[25] allowed the animals free access to a 2.4 per cent calcium lactate solution. The investigators report that the appetite for calcium lactate was definitely increased by removal of the parathyroid in 17 or 18 rats. The animals seemed to make an effort to satisfy their increased need for calcium by ingesting large quantities of the liquid. Although it seemed quite likely from the data that parathroidectomized rats have a craving for calcium, it was felt that such a conclusion was unwarranted since only calcium lactate was employed. So the same investigators conducted another experiment in which rats were allowed to select from several different solutions.[22] A markedly increased appetite for calcium solution, which included lactate, acetate, gluconate, and nitrate, and an aversion toward dibasic sodium phosphate were shown by the rats after parathyroidectomy. After the operation the rats also showed an increased appetite for strontium and magnesium salt solutions. When the operated animals were given free access to the calcium solutions, they made selections which reduced their mortality to zero, greatly improved the tetany, and either eliminated or reduced the loss of weight which had occurred. It was concluded that the decreased mortality and alleviation of deficiency symptoms of the parathyroidectomized rats given access to calcium solutions furnished additional evidence that rats have an ability to make selections conducive to their well-being.

Wilens and Waller[26] found that when completely parathyroidectomized rats on low calcium diets were given a choice of calcium lactate or sodium phosphate solution, they showed an increase in the amount of calcium and a decrease in the amount of phosphorus ingested. Young[11] comments that not only do parathyroidectomized rats select more calcium, but they also exhibit an aversion to phosphate. Such animals demand a diet high in calcium and low in phosphorus, and they refuse to partake of diets low in calcium and high in phosphorus.

Carbohydrate Balance

That the lack of the pancreas causes diabetes was discovered in 1890, and it was later demonstrated that the pancreatic hormone is carried in the blood stream and that pancreatectomized animals can be kept normal by injection of the pancreas hormone, insulin.[7] Insulin is important in carbohydrate metabolism and without it the concentration of blood sugar increases and polyuria and polydipsia develop.[8]

A rat kept on a standard diet after pancreatec-

tomy will develop all the symptoms of diabetes.[6] (It is noted by Harrow[10] that pancreatectomy is fatal to the dog, with the death of the animal occurring in one to two weeks and that the length of survival of cats after removal of the pancreas is about five to six days.) Of 28 rats kept on the standard McCollum diet by Richter and E. C. H. Schmidt[27] in a study of behavioral and anatomical changes produced by pancreatectomy, only 10 of the animals survived the 40-day observation period following the operation. Pancreatectomized rats, this study found, became quite inactive. It was concluded that the inactivity was due largely to the inability of the animals to metabolize carbohydrate.

A later study by Richter and C. H. Schmidt, Jr.,[28] was concerned with the questions of whether pancreatectomized rats seek fat or carbohydrate and of whether their food selections actually help to maintain the constancy of the internal environment. Seven early pancreatectomized rats were kept on the standard McCollum diet after operation, and all developed marked diabetic symptoms—polydipsia, increased food intake, decreased rate of growth, and hyperglycemia. Several weeks after these symptoms had developed the animals were placed on the self-selection diet. It was found that a marked appetite for fat and olive oil was shown by the rats and they ate little or no carbohydrate, which in this experiment was sucrose. An increased appetite was manifested by all 7 animals for yeast. On the self-selection diet the diabetic symptoms of all the rats either disappeared or were greatly reduced. Upon the return to the McCollum diet, diabetic symptoms were again shown by 4 of the 7 experimental animals.

In the same study another group consisting of 8 rats was kept on the self-selection diet for at least 40 days before pancreatectomy and for an equal period of time following the operation, at which time they were put on the McCollum diet. After pancreatectomy all the animals selected more fat and little or no carbohydrate. Measured in calories, the intake of carbohydrates changed from 36 to 9 per cent; fat from 38 to 68 per cent; and protein remained unchanged, 26 to 23 per cent. Diabetic symptoms appeared in none of the pancreatectomized rats as long as they were allowed to select their own diet. After being placed on the McCollum diet, 5 of the 8 animals showed polydipsia, increased food intake, reduced rate of gain of weight, and hyperglycemia.

From these experiments Richter and Schmidt conclude that the rats helped correct their diabetic symptoms by their selections of food and that their selections were in close agreement with the known needs of human diabetics. The authors conclude also that the various responses of the diabetic rats can be explained in terms of Claude Bernard's principle of the maintenance of the internal environment.

A later study by Richter, Schmidt, and Malone[29] dealt with the dietary selections of rats made diabetic by pancreatectomy. After removal of the pancreas, rats maintained on the standard diet developed diabetic symptoms. The animals were then placed on a self-selection diet, and they made dietary selections which caused all the diabetic symptoms to disappear. In general, the results confirm the finding of Richter and Schmidt,[28] reviewed above, that the rats tend to make dietary selections which favor health.

Summary

Much attention has been devoted in recent years to the self-regulatory functions of the body. The outstanding work of Cannon in this field is probably the most widely known. Whereas his studies have been primarily concerned with the physiological regulations of the internal environment, much of the work of Richter has dealt with the maintenance of the constancy of the internal environment through the operation of behavior regulators. Research pertaining to self-regulatory behavior in rats after extirpation of certain endocrine glands is reviewed.

Body temperature is regulated chiefly by the thyroid and the anterior lobe of the pituitary. Animals from which either of these glands has been removed will die within a few days if the external temperature is even slightly lowered. Nest building activity in normal rats is inversely related to environmental temperature. Thyroidectomized and hypophysectomized rats display greatly increased nest building behavior, producing larger and better constructed nests. This is interpreted as an attempt by the animals to compensate for reduced metabolism and to maintain a constant body temperature.

The water balance of the body is regulated primarily by a secretion of the posterior lobe of the pituitary. Surgical removal of this gland results in polyuria and other symptoms of diabetes insipidus which will be followed by dehydration and death. After such an operation rats who are allowed free access to sufficient quantities of water will ingest enough to keep themselves alive and in good health indefinitely. This may be viewed as an attempt by the rat to maintain a constant internal environment.

Adrenalectomy causes a decrease in the sodium concentration of the blood and in rats will result in

death from excessive loss of sodium in 10 to 15 days. Adrenalectomized rats that are put on a self-selection diet including a sodium solution of optimal or near optimal concentration will ingest a sufficient amount of it to survive indefinitely in apparently good health. This is also viewed as an effort on the part of the animal to compensate for an internal deficiency and thus maintain a constant internal environment.

Calcium concentration of the blood is controlled by parathormone, a secretion of the parathyroid. Extirpation of this gland results in tetany and death. If parathyroidectomized rats are allowed free access to calcium solutions, they will consume a quantity sufficient to either eliminate or reduce the tetany and avoid death. This may likewise be seen as an attempt by the animal to restore the constancy of the internal environment.

Pancreatectomy stops the flow of insulin which results in diabetes. Such animals are unable to metabolize carbohydrates and should avoid them. Pancreatectomized rats develop diabetic symptoms while on the standard diet, but when placed on a self-selection diet, these animals ingest little or no carbohydrate and larger amounts of fat, thus either eliminating entirely or greatly reducing the diabetic symptoms. This is interpreted as an effort by the animal to maintain a constant internal environment.

It is concluded that rats have a tendency toward and the ability to induce, in a permissive situation, behavior which will maintain the constancy of the internal environment. It is felt that this principle of the constancy of the internal environment may be of value in understanding activities of the total organism.

References

1. Cannon, W. B. *The Wisdom of the Body,* Rev. Ed. New York: Norton (1939).
2. Richter, C. P. *Endocrinology,* **28,** 193 (1941).
3. Kinder, E. F. *J. Exptl. Zool.,* **47,** 117 (1927).
4. Richter, C. P. *Comp. Psychol. Monogr.,* 1, 1 (1922).
5. ———. *Cold Spring Harbor Symposia Quant. Biol.,* **5,** 258 (1937).
6. Morgan, C. T., and Stellar, E. *Physiological Psychology,* 2nd Ed. New York: McGraw-Hill (1950).
7. Moss, F. A. *Comparative Psychology,* Rev. Ed. New York: Prentice-Hall (1942).
8. Richter, C. P. *J. Comp. Physiol. Psychol.,* 40, 129 (1947).
9. ———. *Proc. Assoc. Research Nervous Mental Disease,* **17,** 392 (1936).
10. Harrow, B. *Textbook of Biochemistry,* 3rd Ed. Philadelphia: Saunders (1944).
11. Young, P. T. *Psychol. Bull.,* **38,** 129 (1951).
12. Gaunt, R., Tobin, C. E., and Gaunt, J. H. *Am. J. Physiol.,* **111,** 321 (1935).
13. Rali, E. P. *Endocrinology,* **39,** 225 (1946).
14. Richter, C. P. *Am. J. Physiol.,* **115,** 155 (1936).
15. Richter, C. P., and Eckert, J. F. *Endocrinology,* **22,** 214 (1938).
16. Young, P. T., and Chaplin, J. P. *Comp. Psychol. Monogr.,* **19,** 45 (1949).
17. Bare, J. K. *J. Comp. Physiol. Psychol.,* 42, 242 (1949).
18. Richter, C. P. *Endocrinology,* **24,** 367 (1939).
19. *Ibid.,* **29,** 115 (1941).
20. Clark, W. G., and Clausen, D. F. *Am. J. Physiol.,* **139,** 70 (1943).
21. Mark, J. *Endocrinology,* **31,** 582 (1942).
22. Richter, C. P., and Eckert, J. F. *Am. J. Med. Sci.,* **198,** 9 (1939).
23. Swingle, W. W., and Wenner, W. F. *Am. J. Physiol.,* **75,** 378 (1926).
24. Luckhardt, A. B., Waud, R. O., and Brannon, L. *Ibid.,* **76,** 228 (1926).
25. Richter, C. P., and Eckert, J. F. *Endocrinology,* **21,** 50 (1937).
26. Wilens, S. L., and Waller, R. K. *Ibid.,* 28, 828 (1941).
27. Richter, C. P., and Schmidt, E. C. H. *Ibid.,* **25,** 698 (1939).
28. Richter, C. P., and Schmidt, C. H., Jr. *Ibid.,* **28,** 179 (1941).
29. Richter, C. P., Schmidt, E. C. H., and Malone, P. D. *Bull. Johns Hopkins Hosp.,* **76,** 192 (1945).

The Sleep-Wakefulness Pattern in the Arctic

NATHANIEL KLEITMAN and HORTENSE KLEITMAN*

Nathaniel Kleitman, professor of physiology at the University of Chicago, has been investigating sleep for over thirty years. To determine the influence of unusual environmental conditions on the diurnal sleep-wakefulness rhythm, he has made studies in Mammoth Cave, Kentucky, in submerged submarines, and more recently in the Norwegian Arctic. Hortense Kleitman, a graduate of the University of Michigan, where she majored in sociology, is with the Chicago Research Department of the advertising firm of Young and Rubicam. She has served her father as a "guinea pig" since infancy; this marks her debut as his co-worker.

THE consolidation of the several irregularly distributed daily sleep fractions, characteristic of the newborn infant, into one continuous nightly sleep period, in older children and adults, represents an acculturation of the individual to the family and community routine of living. This routine, in turn, is an adaptation to the astronomical alternation of darkness and light, resulting from the 24-hour periodicity of the rotation of the earth about its axis.[1] In the two polar areas, however, there is a season of continuous darkness and one of continuous light. How do these seasonal extremes affect the sleep-wakefulness pattern of the residents of these far-away regions of the earth?

The Antarctic is practically uninhabited, and in the Arctic of the Western Hemisphere there are no communities of any size. In the Eastern Hemisphere, however, there are cities of about 10,000 inhabitants, with a highly organized community life, north of the Arctic Circle, in both Sweden (Kiruna) and Norway (Narvik and Tromsø). Information gleaned from travelogues and occasional books dealing with Scandinavian countries points to a profound effect of continuous summer daylight upon the sleep routine of the denizens of the North. Travelers who usually visit these parts in the summer invariably report that people can be seen in the streets of arctic towns at all hours of the night. Dudley V. Talcott,[2] a sculptor and sailing enthusiast, who spent several summers cruising in arctic waters, related that he "often heard the people down in the south of Norway say that everyone in Nordland and Finmarken is crazy. They say that no one who eats so much fish and who gets no sun at all for three months in the winter and so much sun that he doesn't go to bed at all during the summer can help but be a little cuckoo." Talcott "would not be surprised if a scientist should some day come out with the discovery that the sun actually creates a tide within the substance of one's brain." Also, every spring, the advent of the touring season brings forth a number of articles on the attractiveness of the Arctic and the peculiarities of its people. In March, 1952, in the *New York Times,* Oden and Olivia Meeker,[3] referring to Lapland (Norwegian-Swedish-Finnish Arctic), declared that "people are apt to sleep only three or four hours a day during the summertime here."

As anecdotal information on other aspects of the diurnal routine of living has proved to be, on close examination, highly inaccurate, and as a sojourn in Tromsø, in the summer of 1951, was made for a study of a purely physiological nature, it was decided to venture into sociological research of the community sleep-wakefulness pattern. This was to be done by direct count of the number of persons that passed a particular central point of Tromsø at different hours of "day" and "night," as well as by intensive interviews with a sample of the population of the town.

* We extend our thanks to Sigurd Winther-Hansen, chief of the Weather Forecasting Establishment for Northern Norway, and to his charming wife, for the hospitality they extended to our family during our stay in their home; to Dagfinn Sivertsen, of the University of Oslo, for his invaluable aid in collecting the material upon which this report was based; to Dr. Herbert Hyman, Columbia University, for helping us to secure Mr. Sivertsen's services; to Dr. Erling Skouge, Physician-in-Chief, Medical Division, Tromsø Hospital, for placing at our disposal his analysis of medical history records; and to Shirley A. Starr, Senior Study Director of the National Opinion Research Center, University of Chicago, for codeing and tabulating the data. The study was made possible by a grant from the Dr. Wallace C. and Clara A. Abbott Memorial Fund of the University of Chicago.

General view of Tromsø, Norway.

Tromsø is located at 69°39′ N, 18°58′ E, and occupies a portion of the east shore of the small, hilly, oblong island, Troms (Tromsøya), six miles long (N-S), and two miles at its widest. The Tromsø sound, at the site of the town, is only about half a mile wide, and a ferry boat furnishes communication with the mainland at half hourly intervals. To the west there are outcroppings of one very large island (Kvaløya) and several smaller ones. A couple of miles west of the southern tip of Tromsøya lies the wreck of the German battleship Tirpitz, which turned turtle and sank in shallow water from the effects of near misses from RAF bombers. Because of the latitude, the mountains of the mainland and nearby Kvaløya, though only two to three thousand feet in height, are covered with snow for the greater part of the year. Tromsø therefore appears to be surrounded by truly magnificent alpine scenery.

The town is about 200 miles north of the Arctic Circle and 1400 miles from the North Pole. The sun describes a daily circle in the sky, but remains above the horizon, from May 21 to July 23, daylight thereafter decreasing unevenly to 12 hours per day at autumn equinox and down to nothing by November 21, when the period of complete darkness begins. From about January 23, when the sun reappears, the length of the day increases until continuous daylight is reached by May 21. The climate in that part of Norway is relatively mild, due to the nearby passage of the Gulf Stream. The winters are quite stormy, but the mean air temperatures from December to March are only 25–27° F, whereas in July and August they are about 52° F. Because of continuous daylight, the growth of vegetation during the short summer is quite lush. Trees are covered with leaves and flowers bloom in the open, while cattle browse in the meadows surrounding the town. This part of Norway is the only region in the world where cereals (rye and barley) can be raised so far north.

Tromsø is called the Capital of the Arctic. Administratively, it is the capital of Troms county and bishopric. It has a large hospital, staffed with an imposing roster of full-time physicians and surgeons. A state broadcasting station, a daily newspaper, a weather forecasting establishment (one of three in the whole of Norway, the other two being at Oslo and Bergen), a Northern Lights Observatory (built on a grant from the Rockefeller Foundation), consulates and consular agencies of several countries, banks, primary, secondary, and professional schools—all these contribute toward making the town something of an intellectual center also. Commercially and industrially, Tromsø's eminence rests upon the possession of three fine harbors and its location on the main north-south sea route. Sealing, whaling, and Arctic exploratory expeditions start from here; fish, seals, and whales are brought in for processing, oil extraction, and manufacture of fertilizer; and there is a thriving shipbuilding and outfitting industry. A daily passenger-mail-freight service is maintained in both south and north directions, and there are also bus connections with Kirkenes, on the Russian border, and with Lønsdal, the northern railhead of the Norwegian system, just above the Arctic Circle. In addition, during the summer, a seaplane line operates on a five times a week schedule to and from Oslo, only five hours away, from an air harbor on the Troms island, about two miles north of the town limits.

The pace of life in Tromsø, during the summer months, at least, is leisurely, people strolling through the streets, looking into store windows, stopping to chat with friends. Occasional Lapps, in their picturesque multicolored costumes, add brightness to the otherwise drab street scene. The daily work stint in stores and offices is from 8–9 A.M. to 3–4 P.M., with a break of a few minutes at noon, for coffee and cake or a sandwich, usually brought from home. The principal family meal of the day (*middag*) is served shortly after the conclusion of the day's work, between 3:30 and 4:30 P.M. Thereafter, until bedtime, the several members of the family are entirely on their own with respect to activity, as supper (*aftens*), in the form of a light snack, is usually eaten individually some time in the course of the evening. Municipal electricity is time-rationed, and current is not available after 9 P.M. This is of no consequence in the summer, but in the other seasons the current shut-off becomes an obligatory bedtime hour for those who have provided no other means of illumination (candles, kerosene lamps, electricity from home-

size gasoline or diesel engine dynamos). Hospitals and other institutions operating around the clock have rather powerful auxiliary electric stations.

A cinema showing some of the latest American and British films runs on a fixed schedule of single shows, one in the late afternoon and another in the evening. There are evening band concerts in the city hall plaza during the summer. Informal parties, with endless sampling of good things to eat and drink, and enlivened by group singing, are likely to continue way past midnight, as it seems to be late afternoon throughout the whole period of conviviality. Outdoors, in the summer, it is not unusual to see people engaged in mowing the lawn or playing a game of tennis around 11 P.M. In the streets there is considerable circulation, on foot and by bicycle, in what corresponds to late evening hours in lower latitudes.

Direct observations of "night life" in Tromsø, during the summer of 1951, revealed a sharp drop in street traffic shortly after midnight, except in the vicinity of the harbor. As in other rather isolated arctic towns, the big events of the day were the dockings of the regular mail-freight-passenger steamers, northbound for Kirkenes and southbound for Trondheim and Bergen. At Tromsø the north-going boats usually arrived at about 5 P.M. and departed three hours later, and they brought many passengers, tourists from southern localities or Tromsøites returning from their vacations. Relatively few persons departed from Tromsø for towns farther north in the steamers' itinerary. Just the opposite was true of the south-going boats, arriving at about 11 P.M. and departing at 2 A.M. Great numbers of passengers, southbound on business or pleasure, with accompanying relatives and friends, would gather at the pier prior to the arrival of the steamer. The prevailing animation of this expectant crowd makes it easy to understand why cruise passengers, making their first and only round trip from Bergen to Kirkenes, get the impression (and later relate it in articles and books) of feverish activity in Tromsø and other arctic towns, no matter what the hour of the docking of the steamers. If they ventured into neighboring streets during the boat stopover, they would also see a good deal of movement. Had they wandered farther from the pier, they would notice a progressive thinning out of traffic and also that it invariably radiated away from or toward the harbor. What the travelers could not record, however, was the *complete* cessation of vehicular and pedestrian movement in the whole city half-an-hour to an hour after the departure of the boat. Aside from an occasional police motorcycle with side-car, or a baker or post-office worker coming out for a breath of fresh air, the streets of Tromsø were absolutely deserted. The broad daylight and red sun hugging the horizon accentuated the eerie ghost town aspect of the Capital of the Arctic in these early hours of the morning.

The authors, with their young friend, Erik Winther-Hansen, on a visit to a whale-processing yard, on the outskirts of Tromsø. (Photo by Sigurd Winther-Hansen.)

There was only one night during which circulation through the streets of Tromsø was considerable, continuous, and rather boisterous, and that was Saturday, June 23, "Midsummer Night," when the midnight sun was highest above the northern horizon. In more southerly latitudes great bonfires are lit in the fields or on mountain tops during that night, but in the Arctic the sun would make these fires practically invisible. The Tromsø celebrants therefore used smoke-producing materials for fuel, and billows of smoke rising from the midst of clumps of human beings, as well as the sustained hilarity of the revelers, testified to the carnival nature of these nocturnal gatherings.

As our original intention of making an accurate hour-by-hour count of the number of persons crossing the central square of the city, around the clock, was defeated through the complete desertion of streets during the night, it became necessary to turn to the indirect method of personal interview for establishing the diurnal activity pattern of the inhabitants of Tromsø. Luckily, it was possible to secure the services of a trained Norwegian interviewer, a sociology student at the University of Oslo, with considerable experience in assisting several U.S. sociologists in studies they carried out in Norway, under the Fulbright Act. This interviewer had a very good command of English, especially of the technical terminology of the sociological interview, and he cooperated in the preparation, pretesting, and subsequent improvement of the questionnaire, interviewed the subjects in Norwegian, and

translated the data obtained into serviceable English. Further treatment of information, such as coding and tabulating the replies, was carried out by the staff of the National Opinion Research Center of Chicago.

Each "depth" interview lasted about one hour and took the form of a conversation with the subject. The order in which the questions were asked was not always the same, nor was it possible to get clear-cut answers to all questions from each subject. It was found best not to limit the interview to the topic of sleep, but rather to make it a general attempt to detect seasonal variations in the routine of living, including meals, work, and recreational activities.*

So far as they could be elicited, answers to each item of the questionnaire were obtained with respect to (1) the day of the interview, or the preceding night, (2) the summer months in general, and (3) the winter months. As the study was made in the summer, (1) and (2) were compared for consistency, and (2) and (3) for seasonal agreement or variation. The interviewer was a "good mixer" and made friends easily. At no time was information sought without an adequate introduction and an explanation of the purpose of the study. Although some persons refused to be interviewed, the majority of those approached seemed eager to cooperate and, as a rule, volunteered additional information, not always relevant. The problem of proper sampling came up during the pilot study, but after an attempt to secure a scientifically random sample on the basis of census figures, it was finally decided to aim for individuals who were articulate enough to furnish the desired information. Thus, the better educated groups were represented out of proportion to their numbers in the general population. The subjects were recruited through personal contacts and the recommendation of others, but as the investigation progressed, a deliberate attempt was made to overcome early bias in sampling by extending the interviews into age, education, occupation, and residence areas that would ensure a more balanced distribution This required "door-bell ringing" in a considerable number of cases. Because of the time consumed by the interviews, and the subsequent translation of the answers into English, the size of the modified "quota" sample was limited to 100, or about one per cent of the population of Tromsø.

As already indicated, it was not always possible to ask all the questions. Also, some of the answers were so ambiguous as to defy proper translation and coding. It was therefore necessary, for some items in the questionnaire, to confine the analysis to a portion of the answers, rarely, however, to less than fifty. Where some of the replies could be checked against others—as, for instance, the hours that elapsed between going to bed and getting up against the respondent's own estimate of the duration of his sleep—the figures did not always agree. The majority of the subjects tended to underestimate the duration of their sleep. However, where the differences between numerical means of several variables were treated statistically, their significance, if any, was easily established, even when the numbers involved were small.

Of the 100 subjects, 49 were males, and 51, females. Eight, students in the local secondary school and in a college of navigation, were under 20 years of age; 34 were between 20 and 39; 39, between 40 and 59; and the remaining 19, 60 or over. Their scholastic attainments could not always be determined by direct questioning and, at times, had to be inferred from their occupation or status. Definite data on 52 subjects (31 men and 21 women) revealed that six (students) had completed primary school; 22 (9 men and 13 women), secondary school; and 24 (19 men and 5 women) had higher, usually technical, education. It is safe to assume that the unlisted 48 subjects had little or no formal schooling. No pretense is made that one-quarter of the population of Tromsø had college or university training, and, as would be expected, our sample was also biased with respect to occupation. About one-third were professional or business people; another third, housewives; and the remainder, skilled or unskilled manual workers, students, and retired. Of the 100, 64 were married and 36, single or not living with spouse. The number of individuals per family unit varied from two to seven; 37 included children, usually one or two per family. Four-fifths of the subjects had lived in Tromsø more than five years.

The respondents' daily routine of living was about the same as that of the general population of northern Norwegian towns. A breakdown of the coded replies revealed that the prevailing mode of awakening in the morning was "spontaneous," with alarm clocks not a very close second, and arousal by another member of the family, third. Breakfast was eaten anywhere from 5 to 10 A.M., mostly at 8–8:30, almost immediately upon getting up. Coffee was by far the favorite beverage at breakfast, but some drank milk, and a few, tea. Dinner time stretched from noon to 5 P.M., with

* Data were thus gathered on several aspects of living, omitted from this report, such as the length of the working day, group activity outside the home, hours of meals, habits of other members of the respondents' families, etc.

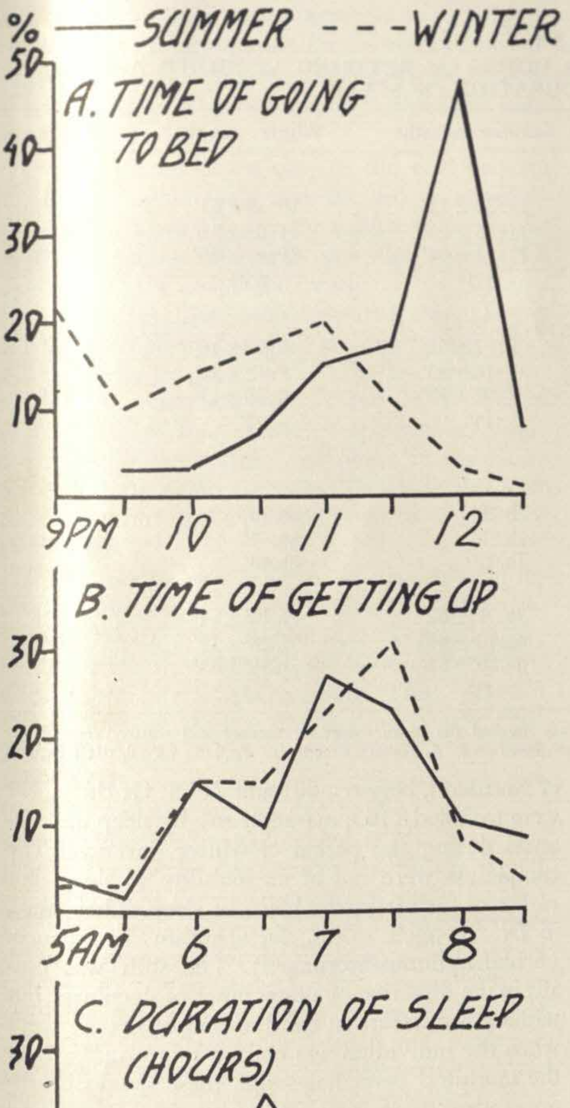


FIG. 1. Frequency distribution curves of the hours of going to bed and getting up, as well as the duration of sleep.

distinct modes at 12:30–1 P.M. for 'stay-at-homes' and 3–4 P.M. for those who came home at the end of the working day. Supper hours ran from 5 to 9 P.M., but the majority ate between 7 and 9. The choice of beverage at dinner and supper was fairly equally divided among coffee, milk, and tea. Some drank coffee later in the evening.

Although 33 persons reported having had naps the day before they were interviewed (12 naps lasting less than 45 minutes, and 11, an hour or longer), very few admitted that they took after-dinner naps regularly, especially in the summer. Leisure-time activities were the same in the summer as in the winter for one third of the group; the other two thirds engaged in different activities, mostly outdoor in the summer, indoor in the winter. Friends and relatives were visited somewhat more frequently in the winter than in the summer.

As previously stated, the shutting off of electricity could have no effect on the time of retiring in the summer, but in the winter nearly one-quarter of the subjects were in bed by 9 P.M. Few persons had individual bedrooms, the great majority sharing theirs, as a rule, with one other member of the family, some, with two or three others. More people left their bedrooms undarkened during the night in the summer than bothered to pull down the shades. Indeed, many bedrooms had no shades. The town was very quiet at night, and there were no noises to disturb the sleepers in any season of the year. Three-quarters of the subjects used no sedatives or hypnotics at any time, but the few who occasionally resorted to them did so more often in the winter than in the summer.

Going to bed. The percentage frequency distribution of the hours of retiring for the night is shown in Figure 1A. During the summer, the respondents went to bed as early as 9:30 P.M. and as late as 12:30 A.M., but 78 per cent retired from 11 P.M. to midnight. The distinct mode at midnight represented the highest percentage for any of the six frequency distribution curves in the figure and, on the breakdown of the data by sex, age, and occupation, was found to apply to men as well as women; to those under and those over 40 years of age; and to housewives, professional persons, and manual workers. The mean going-to-bed time was 11:36 P.M. (Table 1, A). Although the range of the hours of retiring was about the same in the winter as in the summer, the percentage distribution curve in the former reveals no sharp mode at any hour, but a gradual increase till 11 P.M. In the winter, the mean group retiring hour was 10:32 P.M., or over one hour earlier than in the summer. This seasonal difference was statistically highly significant (P less than 0.000001). Furthermore, in both seasons, men seemed to retire later than women, and older persons, later than younger ones. These sex and age disparities were probably connected with

TABLE 1

MEAN VALUES, AND STANDARD DEVIATIONS, FOR HOURS OF RETIRING AT NIGHT, ARISING IN THE MORNING, AND DURATION OF SLEEP

	Summer months	Winter months	Difference
A. Hours of Retiring at Night (P.M.)			
Entire Group (75 & 88)	11:36 ± 41′	10:32 ± 61′	64′†
Professionals (28 & 28)	11:53 ± 23′	11:05 ± 31′	48′†
Housewives (26 & 29)	11:31 ± 41′	10:02 ± 56′	89′†
Difference	22′*	63′†	
B. Hours of Arising in the Morning (A.M.)			
Entire Group (48 & 39)	7:02 ± 52′	6:57 ± 46′	5′
Professionals (18 & 15)	7:16 ± 33′	7:12 ± 30′	4′
Housewives (15 & 12)	7:02 ± 53′	6:50 ± 41′	12′
Difference	14′	22′	
C. Duration of Sleep			
1. Based on A. and B.			
Entire Group	7h.26′	8h.25′	
Professionals	7h.23′	8h. 7′	
Housewives	7h.31′	8h.48′	
2. Based on subjects' estimates			
Entire Group (87 & 71)	7h. 1′ ± 62′	7h.48′ ± 62′	47′†
Professionals (28 & 27)	6h.40′ ± 49′	7h.26′ ± 51′	46′‡
Housewives (27 & 17)	6h.58′ ± 64′	7h.56′ ± 65′	58′‡
Difference	18′	30′	

Figures in parentheses indicate the numbers of subjects whose data supplied the mean values for summer and winter, respectively. Degrees of statistical significance of the differences between the means, based on R. A. Fisher's *t* method: * $P<0.02$; † $P<0.001$; ‡ $P<0.01$.

occupational factors: housewives went to bed earlier than did the professional people, mostly men. Even in the summer, when, as stated, all subgroups showed a retiring mode at midnight, the mean retiring time for the housewives was 11:31 P.M. and for professionals, 11:53 P.M., the 22 minutes' difference being on the borderline of statistical significance. In the winter, however, the mean retiring times for the two subgroups were 10:02 P.M. and 11:05 P.M., and the difference, nearly three times as great as in the summer, was of definite statistical significance.

Three-fifths of the 90 respondents who supplied information on the subject stated that, as a rule, they found it easy to fall asleep in all seasons, but 48 persons noted a seasonal difference in this respect. Of the latter, 34 could go to sleep with greater ease in the winter, 14 in the summer. It will be recalled, however, that the few who had to use hypnotic drugs did so predominantly in the winter.

Among the reports concerning seasonal differences in sleep in the northern regions of Norway and Sweden were personal communications from local physicians citing complaints of patients. Perhaps the most extensive study of this subject was made by Dr. Erling Skouge, who analyzed medical history records of 1000 patients (including 90 children six years of age or younger) who resided in the counties of Troms, Finnmark, or the northern half of Nordland, between 68° and 71°N. Of these, 471 were troubled (163, markedly so) by sleep disturbances during the period of winter darkness. The complaints were not of an inability to sleep, but rather of a shift in the hours of sleep. They were, in Dr. Skouge's words, sleep-rhythm disturbances (Schlafrhythmus-Störungen). The shift was usually in the direction of a later onset of sleepiness, but without a decrease in the duration of sleep, except when the individual was obliged to get up early in the morning. Percentage-wise, these sleep disturbances were more frequent in men than in women; in adults than in children; in "immigrants" from southern counties than in the indigenous population. On the other hand, an analysis of 514 medical history records from residents of the counties of Vestfold, Agder, or Rogaland, located in the extreme south of Norway, between 58° and 59.5° N, showed only seven complaints pertaining to sleep in the winter.

Getting up. By contrast with the seasonal difference in the time of retiring for the night, the percentage distribution curves for the hours of arising in the morning (Fig. 1B) are practically superimposable. With ranges of 5 to 8:30 A.M. in both seasons, the mode in the summer (22 per cent) was at 7 and that in the winter (24 per cent), at 7:30. The corresponding means for the two seasons were 7:02 and 6:57 (Table 1, B). It can also be seen

that there were only small differences in the time of arising of the subgroups, and these were of no statistical significance. However, housewives who went to bed earlier also got up earlier, both in the summer and in the winter.

Over one-third of the subjects did not wake up during the night; the remainder did so occasionally, but no more often in the summer than in the winter. The mood on getting up in the morning did show a slight seasonal difference: in the summer 32 per cent of those who answered this question felt "tired or sleepy" and 42 per cent were "wide-awake, alert, in good spirits"; in the winter the respective percentages were 41 and 24. It appears that, in spite of much longer sleeping hours, fewer of the respondents were wide-awake in the morning during the winter months. This is in line with the winter shift in the hours of sleep, as reported by Dr. Skouge.

Duration of sleep. The mean figures were derived in two ways, showing consistent differences. First, the hours *spent in bed* were obtained by determining the time that elapsed between retiring at night and arising in the morning. Thus reckoned, the duration of sleep in the summer was 7 hours 26 minutes, and in the winter, 8 hours 25 minutes. Professional persons slept less than housewives, particularly in the winter (Table 1, C1). Second, the subjects' own estimates of how many hours they *slept* per night, in the summer and in the winter, were tabulated and their means obtained (Table 1, C2). A glance at Figure 1C, showing the frequency distribution curves for the duration of sleep in the two seasons, as estimated by the respondents, reveals a distinct mode at 7 hours in the summer, and an undulating plateau for 7 hours and longer, in the winter. The two seasonal curves cross between 7½ and 8 hours, with 56 per cent sleeping 8 hours or more in the winter, but only 22 per cent sleeping that much in the summer. The mean values, and seasonal and occupational variations, from the subjects' estimates, are consistently lower than the figures derived by the first method. This discrepancy may be due, in part, to the fact that (*a*) the estimates were of hours slept, which were shorter than hours spent in bed, and that (*b*) in determining the mean values for the duration of sleep from respondents' estimates, "9 hours or more" was treated as 9 hours, and "5½ hours or less," as 5½ hours, and, with a much larger percentage of the former in the winter and the latter in the summer, the winter means are probably smaller and the summer means larger than they should be. In any case, the mean of 47 minutes for the increase in the duration of sleep in the winter, relative to the summer, though smaller than the 59-minute increase in the time spent in bed, is statistically highly significant. So are the increases in the mean duration of sleep of the professional people and housewives, but the differentials for the subgroups, in either season, though consistent with the corresponding differences in time spent in bed, are not statistically significant.

Discussion of results. The data furnished by the questionnaires confirmed the information obtained by direct observation and were totally at variance with the reports of travelers. The citizens of Tromsø, and undoubtedly those of other towns in the Scandinavian Arctic, follow the same diurnal sleep-wakefulness pattern as does the general population of civilized communities in other parts of the world. As elsewhere, the customary routine of living sets the hour of getting up in the morning. Although the range is considerable (5 to 8:30 A.M.), it is the same throughout the year, and the percentage distribution of the hours of arising is practically identical for summer and winter. Professional people do get up a few minutes later than the other subgroups, but they sleep less. Real and statistically significant seasonal and subgroup differentials were found, however, in the hours of retiring for the night. After-work hours afford individual choices in activity everywhere, but more so in Norway, where community business, by and large, is over in the middle of the afternoon. As could be predicted, advantageous light and climatic conditions prompt Tromsøites to stay up later in the summer than in the winter. "Wakefulness-of-choice," the concept developed in the evolutionary theory of sleep and wakefulness,[1] applies especially to the utilization of leisure time. The onset of sleep has long been looked upon as an "escape from boredom," and there is no doubt that there must be something of interest to keep one up late at night. This factor is revealed with particular clarity in the variation in the retiring hours of the subgroups. Whereas the group, as a whole, retired, on the average, a full hour later in the summer than in the winter, professional people, with their greater ability or eagerness to follow leisure-hour pursuits, consistently stayed up later than did the housewives. The 22-minute mean disparity in the going-to-bed times of these two subgroups in the summer, barely significant statistically, was increased to 63 minutes in the winter, when cold and darkness discouraged outdoor activities, and the "inner stimulus" to the maintenance of the waking state met its greatest challenge in the sleep-inducing seasonal gloom. Again, it will be recalled that the sleep-rhythm disturbances, revealed by Dr. Skouge,

were more prevalent among "immigrants" from the South, with a large proportion of professional people.

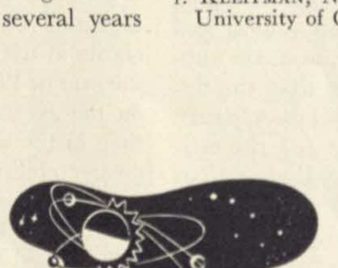
With the community getting-up time fixed the year around, and a later hour of retiring in the summer, it is inevitable that the duration of sleep should be shorter in the summer than in the winter. But how to interpret this seasonal differential? Did the subjects *need* more sleep in the winter? Or, did they sleep longer simply because they had no strong incentive to stay up and therefore retired earlier? The latter alternative appears to be the more plausible one, since, as will be recalled, in spite of a greater duration of sleep, only 24 per cent were "wide-awake" on getting up in the morning in the winter, compared to 42 per cent in the summer. Furthermore, the distinct mode at 7 hours in the frequency distribution curve for the duration of sleep in the summer (Fig. 1C), and the general uniformity of the means for the length of sleep in the summer for the subgroups suggest that 7 hours is perhaps a physiological modicum, sufficient for rest and recuperation. A longer night's sleep represents "luxury consumption," to borrow a dietetic term, or "escape from boredom," or some other combination of extraneous nonphysiological factors. In other words, given something of interest to occupy oneself, it should be possible for the "average" adult to remain awake for 17 hours out of 24. Of course, this does not preclude individual variations, as the spread even in the small sample studied was from less than 5½ to more than 9 hours of sleep per night, both summer and winter. The results of the survey, however, challenge the proverbial recommendation of 8 hours of sleep for the average normal adult.

That the mean duration of sleep of human beings is under 8 hours was demonstrated in figures obtained on 31 subjects, in Chicago, several years ago.[4] These persons, all adults, also slept least in the summer, but the seasonal variation was rather small. Disregarding seasons, the mean duration of their nightly sleep was 7 hours 32 minutes. It increased, on the average, to 8 hours 7 minutes, when they retired earlier, and fell to 6 hours 48 minutes, when they stayed up later than usual. Thus, a mean differential of 79 minutes in the duration of sleep was effected through a variation, by choice or necessity, in the time of retiring for the night. Their getting-up time was also influenced by choice or necessity, and it was found that they slept, on the average, 7 hours 44 minutes when they awoke spontaneously, but only 7 hours 13 minutes when they were awakened by an alarm clock or some other artificial means. For these subjects, as for the Tromsøites, the duration of sleep was increased from 7½ to over 8 hours, as a form of luxury, but necessity brought it down to around 7 hours.

To summarize, the data on the sample of the inhabitants of Tromsø, Norway, where the sun does not set during the summer and where continuous darkness prevails in the winter, show only a slight deviation from the sleep-wakefulness pattern that characterizes the diurnal routine of living in civilized communities in middle latitudes, and incidentally reveal that an average of 7 hours of sleep per night is probably compatible with the maintenance of well-being in human adults.

References

1. Kleitman, N. *Sleep and Wakefulness.* Chicago: University of Chicago Press (1939).
2. Talcott, D. V. *Report of the Company.* New York: Random House, 53, 287 (1936).
3. Meeker, O., and Meeker, O. Summertime Side Trip through Lapland. In the *New York Times,* Section II, p. 20X, Sunday, March 23, 1952.
4. Kleitman, N., *et al. Sleep Characteristics.* Chicago: University of Chicago Press, 52 (1937).



BOOK REVIEWS

STUDY OF MAN

Anthropology Today; An Encyclopedic Inventory. A. L. Kroeber, Chairman. Chicago: University of Chicago Press, 1953. xv + 966 pp. $9.00.

THERE can be no doubt that this volume is one of the most important books ever published in the anthropological field. Certainly it has no peer as the "encyclopedic inventory" it claims to be, and for years to come it will remain one of the most consulted and cited works bearing on the study of man. Nothing like it has ever been published.

To appreciate the scope of this book one should learn how it came to be planned and brought to fruition. In 1951, on its tenth anniversary, the Wenner-Gren Foundation for Anthropological Research, sponsor and supporter of the anthropological sciences, decided that it would need some sort of guide in the formulation of its future philanthropic policies. It felt that developments in the field had been snowballing too fast for it to evaluate its own efforts, and it hit upon the idea of getting its bearings through an international symposium. After elaborate planning, begun in 1951, with Alfred L. Kroeber, dean of American anthropologists, at the helm, such a meeting was held in New York on June 1952. As bases of discussion, fifty-two contributors prepared fifty so-called inventory papers, two of which were joint efforts. A total of eighty scholars from the United States, Europe, Latin America, Japan, India, Thailand, and Australia met and discussed for a fortnight the problems raised in these papers.

The numerous items in the present volume cover the whole range of anthropology and could not be reviewed as individual units even in a more ambitious review than this. Suffice it to say that the first group of articles deals with the problems of the historical approach and is made up of three papers on method, thirteen on results, and five on theory. The second group deals with problems of process and includes five papers on method, eleven on results, and three on theory. The third and last group deals with problems of application and includes nine papers. Appended is a special paper surveying the history of the technical aids in anthropology. Perhaps a more informative way of describing the content of these writings is to say that they deal with the present status of physical anthropology, ethnology, social anthropology, archaeology, linguistics, and applied anthropology. Their scope is, of course, enormous and can be justified and understood only by bearing in mind that the *raison d'être* of anthropology is its special way of looking at man-study. That way is the comparative method. Anthropology dares to invade the fields of history, economics, psychology, sociology, government, jurisprudence, genetics, and the other disciplines dealing with man because it examines him in all his diversified manifestations and reduces them to common denominators. The comparative method is anthropology's substitute for experimentation.

What does the inventory show? Alas! This is impossible to state, both because of the limitations of space and the enormity of the task. As a matter of fact a companion piece, entitled *An Appraisal of Anthropology Today* (University of Chicago Press, 1953), has been published on this very subject and contains the verbatim comments of the eighty participants in the symposium; but even it does not succeed in making an over-all estimate.

Although, in general, each paper in the present volume has been prepared by the most competent man in the field, there are puzzling exceptions. These, however, are in the minority and may be excused as resulting from practical coansiderations rather than bad judgment. The quality of *Anthropology Today* is indisputably superior, and if names need to be mentioned there are those of such scholars as Caso, Chapple, Childe, Grahame Clark, Forde, Kluckhohn, Lévi-Strauss, Mead, Redfield, Teilhard de Chardin, Stith Thompson, Vallois, and Weinert.

WILLIAM A. LESSA
Department of Anthropology and Sociology
University of California at Los Angeles

MAN AND MEDICINE

The Infirmities of Genius. W. R. Bett. New York: Philosophical Library, 1952. 192 pp. $4.75.

THE giant redwoods of the Pacific slope are noted for the ugly burn scars that mar the trunks of many of them. The huge holes, sometimes extending entirely through the trunk, make one wonder how the trees survived such extensive injury. And yet there the redwoods stand, majestically dwarfing all other life around them.

We might postulate upon the possible reflections of some early explorer who first gazed upon a grove of such scarred redwoods. Perhaps he wondered whether the ugly scars were not something more than a manifestation of the unquenchable life potential of the trees; that the injuries in some way even stimulated the growth, or were the instruments for selection of the fittest of the species. Or perhaps, he noted that under foot was lesser vegetation that was withstanding privations relatively as great, but causing no wonder because of its insignificance. And, upon continuing his exploration, he would have come to another grove, with redwoods just as tall and broad, but devoid of any injury. Then perhaps the explorer would have sighed, discarded his reflections, and remained satis-

fied with the mere privilege of beholding their beauty.

Similar reflections are inevitable upon reading the slim, delightful essays Dr. Bett has put together about the infirmities, real or imagined, of thirteen English and two French giants of literature. Without dwelling upon the indefinable and yet recognizable features of genius, there would be little disagreement on whether the fifteen subjects selected fulfill the criteria. Except for this feature, however, there is little in common in regard to their physical or psychological troubles. And one can only wish that there had been included for comparison a few perfectly normal literary geniuses—if there be such specimens and if there be such a state as normalcy.

In some of the case histories, the relationship between the disease and the art is only too clear, although even here it is impossible to distinguish between the cause and the effect. The alcoholic Poe, the addicts De Quincey and Baudelaire, reveal their frustrations and insecurities in their writings. But neither the psychological infirmities, nor the drugs by which they sought to alleviate them, can explain the nature and the extent of their genius. These factors merely exteriorized and perhaps gave certain directions to their genius. The malevolent Pope and the debauched Byron certainly were influenced by their physical deformities, and this influence is revealed in their writings. Yet the deformities offer no clue to the source of their genius. The tuberculosis of Keats and the rheumatic heart disease of Burns appear as accidental incidents without apparent influence on the art, despite Dr. Bett's perpetuation of the old fable regarding some more direct biochemical connection between the tubercle bacillus and literary brilliance. And at least two examples are given in which the literary achievements seem to be antithetical to the man and his environment: the lusty songs of Whitman, and his inversion, and the gentle work of Lamb, and his insane household.

Even among the fifteen selected cases, is the incidence of psychopathology any higher than could be found among fifteen individuals selected for their mediocrity? The bellyaches of Carlyle, recorded in his Gargantuan style, attract attention through his descriptions and because Carlyle was involved, and not because they were any more severe or more personally influential than the peptic ulcer of a businessman detailed by an interne in a hospital chart. The sexual peccadilloes of Burns, Shelley, or Byron are interesting and presumably important because they were involved, whereas those of more common citizens are either their unrecorded private affairs or mere statistics on Kinsey's punch cards.

Whether the defects of these men have as causal connections to their genius as are implied in the essays is, at least to this reviewer, a matter of opinion and unestablished conjecture. It may be, in reverse, a demonstration that genius cannot be thwarted even by fatal disease, disabling psychological quirks, or the most hostile of environments. Perhaps—and this is pure heresy in the era of psychosomatics and psychoanalysis—perhaps here we are given a glimmer that there is something bigger and better than man, although through man it is manifest.

M.B.S.

Great Adventures in Medicine. S. Rapport and H. Wright. New York: Dial Press, 1952. 874 pp. $5.00.

THIS is really quite an amazing potpourri of medical excerpts, articles, statements, and other selections, arranged in six approximately historical parts, with a short synopsis for each part. There seems to be no central focus or criterion for the choice of the items. Accounts of their discoveries by Koch and by Pasteur are followed by selections from Dickens and Conan Doyle. There are some pages of ancient and medieval remedies; Francastoro's poem on syphilis; twenty-two pages about Johns Hopkins and Osler, including a humorous poem; excellent, concise reviews of the histories of yellow fever, pellagra, and other diseases; and looks into the future and beyond.

The unevenness of the compendium certainly contributes to its informality, and it is not a doctor's book about doctors. The description of the plague by Boccaccio can be compared with a description of trichinosis from the *New Yorker,* not only from the standpoint of the advance in medicine in six hundred years, but as examples of literary and journalistic styles of the fourteenth and of the twentieth centuries.

The authors must have had a lot of fun gathering the material, and it is a lot of fun to read, no matter where one starts. One might wish for the inclusion of more selections from the historical giants such as Galileo, Newton, and Francis Bacon, for better representation beyond the boundaries of the United States, England, and France (neither Virchow nor Pavlov is present), and for fewer contemporary papers that obviously are going to fade in the stretch.

This is just the book to present to a nephew in college who is considering medicine as a career, or to take on a summer vacation. For that matter, it would also serve as a good jumping-off place for a seminar-type course in medical history. But for any purpose, it should be read under the best illumination, for the publishers have crammed every page with as many lines as the small print would allow.

MICHAEL B. SHIMKIN

University of California School of Medicine, San Francisco

FIFTY-YEAR SUMMATION

Diseases in Plants. Neil E. Stevens and Russell B. Stevens. Waltham, Mass.: Chronica Botanica, 1952. xx + 219 pp. Illus. $4.75.

THIS introduction to general plant pathology departs from the handbook type of presentation often followed in college texts. Instead of describing an array of plant diseases, with excursions into elementary bacteriology and mycology, the authors give a general ac-

count of the science—drawing upon the entire field for their evidence and illustrative material. Because of the great quantity of information on plant diseases to be covered, the book is very much condensed, almost to syllabus form. With a good teacher, it should be both a stimulating and a challenging text for students.

The preface indicates that much of the manuscript was essentially complete at the time of death of the senior author. To his son, also a professor of plant pathology, fell the task of coordinating and arranging the abundant material. But many of the topics that are stressed—plant disease survey, epidemiology, plant disease losses, plant quarantines, *inter alia*; the fascinating phytopathological lore and curiosa included here or there; the references to cranberry diseases; the insistence on the strong relationship of plant pathology to national and world affairs—were encompassed in the thinking and lifework of the dynamic senior author, who speaks again in this fine book.

The authors deal with general pathology under four main subdivisions: an introduction, a section on causal agents of plant diseases, a section on development of pathogenic diseases, and the final one on disease control. In the introductory section, the world food problem is briefly outlined, and then the losses from plant diseases are considered in this frame of reference. The authors sum up the matter thus: "This, then, is the real problem which faces plant pathologists in relation to the public welfare; that of protecting a vital source of foods from serious and in large part preventable destruction." In a seven-page chapter of this section, the effects of disease on the plant body and on the root, stem, and leaf are briefly sketched. The authors' concept of plant disease is any impairment of structure or process sufficient "to noticeably or permanently affect the normal development of the plant." Normalcy, however, is not defined.

In the second section of the book, under the general heading "Causal Agents of Plant Disease," the titles of the chapters are: The Viruses; Bacteria and Fungi; Flowering Plants, Nematodes, and Insects (*sic*); and Nutritional Factors, Climatic Effects, Chemical Injury. The third section of the book discusses the host plant, the pathogen, the effects that weather and soil have upon disease development, the relationships of insects to plant disease, and variation and physiological specialization in plant pathogens. This section closes with a chapter entitled Introduced Hosts and Pathogens. The final section of the book, devoted to disease control, considers the economics of disease control; fungicidal treatments; seed and soil treatments; sanitation, eradication, and quarantine; and breeding for disease resistance. There is a chapter on Market Pathology. The book closes with a nondescript chapter entitled Handling Plant Disease Problems, whose topics in any later edition could well be reassigned to other sections. From this brief scheduling, it will be seen that the authors have achieved a very satisfactory organization of subject matter. By virtue of two indexes, the one general and the other for persons, information is readily accessible.

Many years ago, in their preface to the famous University of Chicago *Textbook of Botany*, Coulter, Barnes, and Cowles disclaimed that the volumes were written for their professional colleagues; all teleological implications were expressed disavowed, and it was pointed out that the authors sought to develop certain general concepts thought to be fundamental rather than to present an encyclopedic collection of facts: "This purpose," they said, "has demanded occasionally also a greater apparent rigidity of form in general statements than is absolutely consistent with all the facts." So, too, in this book written for students—probably undergraduates—we must not always expect the critical professional viewpoint, or the stressing of exceptions and alternative hypotheses, but rather a certain positiveness of statement. We also must expect what to the professional may seem glaring omissions, together with occasional overemphasis of new and intriguing phases.

With such guidelines, the book is certainly very acceptable. It has few errors of consequence. The attention given to the various topics often seems disproportionate to their relative significance in phytopathology. Thus the viruses, as causal agents, receive more discussion than the bacteria and fungi. Market pathology is discussed in greater detail than breeding for disease resistance. The map entitled "Agricultural Research in United States" (Fig. 60) contributes little. The size of virus particles is given in microns where millimicrons are meant. There is a tendency to include speculative and untested information where the subject matter had special appeal to the authors. In many places the diction is jerky; in a few places the statements are clumsy, almost as if too sweeping a comment had originally been written and then, upon review, this was modified by some saving phrase or qualifying word. All these relatively minor shortcomings can be ironed out in any later edition.

The most serious criticism this reviewer would make deals with the references to phytopathological literature. In considerable part, these seem to be readily available sources for checking purposes rather than to the original researches. As a result, many of the great names in plant pathological science are not put before the student; often he is not directed to the most important contributions. The literature references occasionally are to nonscientific or ephemeral publications. A more scholarly bibliography would, in the reviewer's opinion, enhance the usefulness of the book.

With purpose, the authors chose the same title for their book as was used by H. Marshall Ward, the great English plant pathologist, a half-century before. The first book was an authoritative discussion of the principles of plant pathology, and in the early days of the science had great formative influence. It is therefore not without interest to contrast the precursor volume with the present summarization, now that plant pathology has taken its place beside plant physiology as an independent botanical discipline.

Ward had available a relatively meager store of phytopathological information. He could discuss the

necrotic diseases, drawing chiefly upon their mycological characteristics. Similarly, the smuts and rusts could be described as fungus entities, but the rusts showed heteroecism and evidence of specialization into races. As additional subject matter, Ward dealt also with nutritional effects and nonpathogenic diseases, with late blight of potato, club root of crucifers, and with the various galls, excrescences, malformations, and injuries of trees and other plants. Bacterial diseases of plants were not included, although this field of research had been opened by T. J. Burrill, J. C. Arthur, and E. F. Smith. Nor does Ward mention virus diseases, although tobacco mosaic and other virus diseases had long been known. M. W. Beijerinck's classic work was published in 1898, but, as we know, it remained largely unapplied until H. A. Allard's researches in 1914 broke with accepted theories and opened the entire field for investigation. The full development of topics possible in Ward's book, as contrasted with the epitomization and omissions required in the later volume, dramatically portrays the enormous increase in our knowledge of plant pathogens.

Ward used nearly one third of his book to acquaint the reader with the elements of plant physiology, stressing plant nutrition and photosynthesis—chlorophyll was not then a household word. He needed to correct certain fallacies as to gas exchange in plants and the circulation of sap. He needed to bring to his readers the new studies on cellular mechanics and physiology. As a student of Julius Sachs and Anton de Bary, this he was well fitted to do. In discussing hybridization and selection, only a four-line mention was made of Mendel's law, just rediscovered, attention being devoted chiefly to W. Rimpau's experiments with wheat. The authors of the book under review very properly saved space by assuming that botanical fundamentals are now common knowledge with students. Ward devoted a chapter to breeding for disease resistance. His approach was chiefly theoretical. He was hopeful of possible accomplishments, but he assessed properly the difficulties that would arise because of specialized races of the pathogens. Not a single "disease-proof" variety was named by Ward. The succeeding half-century witnessed the application of disease-resistance breeding as a most effective control measure for plant disease. From a wealth of such material, the Stevenses have space for only a few striking examples.

As Ward presents it, disease is related to the plant's response to impinging environmental factors, especially if these go beyond its capacity for prompt adjustment. This concept of the equilibrium that the plant tends to maintain with its surroundings focuses attention on the disease factor as the disturber of this equilibrium. Our present viewpoint on plant disease causation, which, of course, Ward helped to mold, does not differ essentially from this interaction concept. Ward stressed that disease was a cell reaction brought about by lack of nutrition, water, or other environmental factor, or by the presence of a disturbing agent. The disease should not be confused with agent or the symptoms with the maladies—obvious, of course, but a thing that each new crop of phytopathologists apparently must rediscover. Ward stressed the living cell as the center of interest; he described the healing processes of tissues after wounding occurs; the stimulation of cell activity as a function of the natural irritability of protoplasm. The parasite may produce its effects through toxic substances—as, for example, the predator fungi—and through enzymes, among which he lists cytases, the diastases, and proteolytic enzymes. Chemotropism is the effective force directing the parasite. Parasitism, as Ward sees it, is a stage beyond epiphytism in that the parasite invests and invades the cells, establishing food and water relations with the host. These generalizations of the pioneer volume find no counterpart in the present book except as a detail here or there can be pieced together to form a philosophy on the nature of disease and its causation. We must not charge this solely to the authors but include the investigators themselves, who seem to have been so preoccupied with factual details they have not found time to ponder or even to speculate.

George H. Coons

Division of Sugar Plant Investigations
Bureau of Plant Industry, Soils, and Agricultural Engineering, USDA
Beltsville, Maryland

BRIEFLY REVIEWED

Ultraviolet Radiation. Lewis R. Koller. New York: Wiley; London: Chapman & Hall, 1952. 270 pp. $6.50.

ULTRAVIOLET radiation nowadays plays a great role not only in physics, but also in biophysics, biology, chemistry, climatology, and medicine. The literature about the subject is dispersed over many journals, and sometimes it is very difficult to find quick and reliable information. A monograph on ultraviolet radiation, written by a competent research man in this field, therefore will be welcomed by all scientists specializing in one of the fields mentioned above.

An introductory chapter covering historical facts, fundamental definitions, and concepts is followed by three chapters dealing with the ultraviolet radiation of arcs, incandescent sources, and solar radiation. Two additional sections are devoted to transmission and reflection properties of numerous materials, wherein most of the data are presented in graphical manner. A chapter on applications and effects of ultraviolet radiation covers biological aspects as well as problems of illumination. A chapter on detectors of ultraviolet radiation concludes the book, which indeed fills a gap in the literature on radiation.

K. W. Meissner

Department of Physics
Purdue University

French Inventions of the Eighteenth Century. Shelby T. McCloy. Lexington: University of Kentucky Press, 1952. viii + 212 pp. Illus. $4.50.

IN ATTEMPTING to write the history of technology on a national basis, the author has not been able to avoid the pitfalls inherent in this method. Science and technology have an international history, and French technology of the eighteenth century can be assessed only as a part of European technology of the period. The author is well aware of the fact, and in several chapters, such as those dealing with the balloon, steam transportation, the telegraph, lighting, papermaking, and textiles, he has managed to inform us of eighteenth-century technology and the special part France played in this development. In other chapters, such as those on chemical inventions, automata and other mechanical devices, military inventions, medicine, and surgery, his method has produced a selection of French improvements or variants rather than basic inventions and an anecdotal arrangement of material that leaves the general reader in the dark about the true place of these inventions in their field of technology or of science.

It is a great pity that McCloy tried to cover so many fields instead of selecting a few specific ones to illustrate why France, with her brilliant thinkers and inventors, did not experience an industrial revolution such as marked eighteenth-century Britain. Neither do we hear all the reasons why France failed in the mechanization of her industry. The apathy of the French bankers and of the bourgeoisie, the state-imposed "manufactures" as a result of Colbert's measures, and several other such factors are not sufficiently stressed. This is a great pity, the more so because the author has unearthed much original French evidence and in his excellent bibliography has given us access to many more sources. We hope that in the next edition of a book very useful for the many data it presents the author will be able to recast his material and, by selecting a few prominent examples only, will prove the thesis he set out to discuss —the failure of eighteenth-century France to industrialize as Britain did.

R. J. Forbes

University of Amsterdam

Phosphoric Acid, Phosphates and Phosphatic Fertilizers. 2nd ed. William H. Waggaman. New York: Reinhold, 1952. 683 pp. Illus. $15.00.

IN THE second edition of his book, the author offers a very complete and up-to-date coverage of all aspects of the phosphate mining and processing industries. This makes an ideal handbook for those engaged in the phosphate industry; it will also serve as an excellent reference book for those engaged in related industries.

The first part has individual chapters describing each of the main phosphate deposits of the world. Where the author was not thoroughly familiar with the deposit he has asked those who were to write the chapter. The known extent of each deposit, together with the characteristics of the rock, are adequately presented.

A large section of the book deals with the phosphate recovery processes and the processes involved in the manufacture of the various phosphates. The manufacture of phosphate fertilizers is the main use which is made of phosphate rock. Separate chapters deal with each of the various types of phosphate fertilizers; for each the author shows the different processes involved and the kinds of machinery used. For each process also, he has given a breakdown of the cost of production.

Although the major portion of the book deals with the phosphate industry, the other uses of phosphate are considered. The methods of production for the use of phosphates in water softening and cleansing, baking, sugar refining, and fireproofing are presented. The effectiveness of the phosphate compounds produced for these purposes is discussed.

The appendix contains a complete listing of the U. S. patents pertaining to the manufacture and use of phosphorus compounds. This enables anyone to become acquainted with the latest developments and also to determine what still lies open for further invention.

The book is written in a clear, direct, and interest-holding style. It will be valuable as a reference book for teachers and researchers in both chemistry and agriculture, and will make an excellent reference book on phosphate fertilizers for courses in fertilizers.

S. A. Barber

Department of Agronomy
Purdue University

The Immaculate Forest. W. R. Philipson. New York: Philosophical Library, 1952. 223 pp. Illus. $4.50.

THE Macarena Mountains, a range east of the Andes in Colombia, isolated, uninhabited, and practically unknown, were the goal of W. R. Philipson and his English and Colombian associates. The author, himself an eminent botanist, was interested chiefly in the flora, but made a great many observations on other phases of natural history.

All forms of travel were used—an airplane to get the layout of the land, mules, launches, canoes—but the actual journey to the mountains was made on foot, hacking a passage through the forest, wading streams populated with electric eels, sting rays, and caimans—far different from paddling in less-infested waters.

The Immaculate Forest is a detailed day-by-day journal of the trip, which followed the range until opposite the peak. Camp after camp was made, packing food and hammocks, and sorties were made from each camp, felling trees and collecting specimens. In a tropical jungle, as the author points out, plants, like the monkeys, have taken to the trees, and the branches take the place of solid earth, for on them sometimes more herbaceous plants grow than in the soil itself.

There were encounters with ants, wasps, and the

ubiquitous sweat bees, but many encounters were more pleasant. Game was plentiful and so completely unafraid it practically walked into the camps. Wild pigs, curassow, and tinamou supplied excellent meat, and the zoological collectors brought in such rarities as the giant armadillo and the spectacled bear, as well as an enormous jaguar.

Eventually, after several attempts, the party actually reached the peak, about 5500 feet high, and made extensive collections there. Other naturalists will look forward to the scientific results of this work.

In addition to the fight to ascend the mountain, other trips were made; an interval was spent in semi-luxury at El Mico Ranch, where the party lived the life of typical *lanneros;* and they flew in a Catalina plane to Leticia, the Colombian outpost on the Amazon itself.

There are a number of excellent photographs, a good index—all in all, an excellent picture of dense jungle teeming with tame wild life, as seen through the eyes of a capable botanist.

W. M. Mann

National Zoological Park
Washington, D. C.

Essentials of Microwaves. Robert B. Muchmore. New York: Wiley, 1952. vi + 236 pp. Illus. $4.50.

THIS book starts with an introductory chapter that contains principally a brief discussion of the place of microwaves in the electromagnetic spectrum, in engineering, and in physics. The second chapter treats the general laws of electromagnetism, and electromagnetic waves. The contents of the succeeding chapters are indicated by the following list of subjects taken from either chapter or section headings: characteristic waves, wave-guides, resonators, filters, antennas, grid-controlled tubes, klystrons, traveling-wave tubes, magnetrons, noise, crystal rectifiers, microwave relays, radar, linear accelerators, spectroscopy, atomic clocks, measurements.

The exposition is nonmathematical; there are but 23 equations, and these have generally been given only as shorthand expressions for relations stated verbally. Derivations are not carried out in the usual sense, although the logic underlying particular derivations is indicated. The text is very well illustrated by some two hundred appropriate figures—an average of nearly one per page.

In summary, we have a compact review of the entire field of microwaves, with emphasis on physical principles and rigid de-emphasis of mathematical manipulations. An exposition of this type has entailed compromises, and it is probable that many readers will not be completely satisfied with all the explanations. The difficulty could hardly be avoided, but the same cannot be said for the occasional evidences of offhand writing. For example, one finds on page 100 the statement: "These natural substances can be employed as dielectrics to very high frequencies (until the wavelength becomes small compared to the molecules or atoms), e.g., light-wave or ultra-violet frequencies." Actually, of course, even for the near ultraviolet the wavelengths are hundreds of times greater than the dimensions of atoms or simple molecules.

Neither of the above criticisms should be taken to mean that the book falls short of fulfilling the purpose for which it is intended—to convey to the reader a feeling for the behavior of microwaves and an appreciation of the range of their application. The book reads easily, and provides a rapid and enjoyable means of becoming acquainted with the broad outlines of the subject.

F. F. Rieke

Department of Physics
Chicago Midway Laboratories

Associated Measurements. M. H. Quenouille. New York: Academic Press; London: Butterworths, 1952. x + 242 pp. Illus. $5.80.

THIS book should be of aid to research workers who wish to demonstrate or test for the dependence of a variable on one or more other variables. Various tests, both graphical and analytical, are presented for determining whether dependence (or association) exists and, if so, what kind and what degree. Several tests of existence of association between two variables are given—such as Tukey's corner tests—which are not readily available in other books. The author approaches the general problem analytically from the analysis of variance and covariance standpoint.

A chapter on time series includes topics on serial correlation, elimination of trend, and various computing and testing devices. Multivariate analysis, the probit transformation, and many other topics are also treated. The book contains thirteen tables giving significance levels of various statistics, including the coefficient of medial correlation, number of pairs of points falling on the same side of the medial line, orthogonal polynomials up to the third order for $n = 3 - 12$, etc.

One should have had some contact with statistical methods in order to appreciate this book. It is written on an elementary level, and no derivations are given, but the book still contains a large amount of solid and valuable material. Throughout the book examples are solved which help to make the ideas presented concrete and understandable. A great deal of this content is not available in other books presuming the small background for the reader that this book does.

Teachers of elementary statistics might well use the book to present some ideas not ordinarily included in the beginning courses—ideas that are relatively simple, and certainly useful in application. The lack of problems for students to solve may well hinder its general use in the classroom, however.

Donald B. Owen

Statistical Laboratory
Purdue University

Historic Researches. Chapters in the History of Physical and Chemical Discovery. T. W. Chalmers. New York: Scribner's, 1953. 288 pp. Illus. $5.00.

THIS collection of essays published in the British periodical *The Engineer* between 1944 and 1948 covers a wide range of subjects, such as friction, the mechanical equivalent of heat, electrodynamics, the ether drift experiments, heat, chemical elements and atoms, the classification of atoms, molecular physics, the conduction of electricity through liquids and gases, X-rays, positive rays, and isotopes. The choice was not accidental; the author explains that he chose these subjects because they had a direct bearing on modern engineering. He chose the historical approach because it had distinctive merits as a practical educative method apart from its intrinsic interest.

The author never indulges in journalese; his expositions are always scientific and hence one should not read this book lightly but study it carefully. The reader will be well rewarded for his efforts. One misses the usual bibliographical notes or bibliography. This is a pity, because many would like to follow up the author's exposition by reading the original treatises and handbooks. It is suggested that this defect be repaired in the next edition. The author has provided extensive biographical and mathematical notes, which give much information, rightly included as an appendix and not in the essays themselves. The book seems well worth its price and forms a welcome addition to the college bookshelf as an interesting historical introduction to aspects of modern physics.

R. J. FORBES

University of Amsterdam

New Books Received

Acanthocephala of North American Mammals. Illinois Biological Monographs: Vol. XXIII, Nos. 1–2. Harley J. Van Cleave. x + 179 pp. Illus. $5.00, cloth; $4.00, paper. University of Illinois Press, Urbana, Ill. 1953.

Child Training and Personality: A Cross-Cultural Study. John W. M. Whiting and Irvin L. Child. vi + 353 pp. $5.00. Yale University Press, New Haven, Conn. 1953.

Manual of Comparative Anatomy (2nd ed.). Osmond P. Breland. xi + 256 pp. $4.50. McGraw-Hill, New York. 1953.

Basic Mechanics of Fluids. Hunter Rouse and J. W. Howe. x + 245 pp. Illus. $4.50. Wiley, New York. 1953.

The Yields of a Crop. Based on an analysis of cotton-growing by irrigation in Egypt. W. Lawrence Balls. xv + 144 pp. Illus. + plates + charts. 21s. Spon Ltd., London. 1953.

Adventures in Artificial Respiration. Peter V. Karpovich. xvi + 303 pp. Illus. $7.50. Association Press, New York. 1953.

Contributions to the Theory of Games, Vol. II. H. W. Kuhn and A. W. Tucker, Eds. viii + 395 pp. Illus. $4.00. Princeton University Press, Princeton, N. J. 1953.

The Bile Pigments. C. H. Gray. xv + 142 pp. Illus. $1.75. Wiley, New York. 1953. (Printed in England.)

Conferences on Drug Addiction Among Adolescents. Sponsored by the Committee on Public Health Relations of the New York Academy of Medicine, Nov. 30, 1951 and March 13–14, 1952. xvi + 320 pp. $4.00. Blakiston, New York. 1953.

Our Physical Environment: A Problem Approach. Leonard W. Gaddum and Harold L. Knowles. ix + 625 pp. Illus. $5.50. Houghton Mifflin, Boston, Mass. 1953.

A Textbook of General Botany (5th ed.). Gilbert M. Smith *et al.* x + 606 pp. Illus. $6.25. Macmillan, New York. 1953.

The Roots of Psychotherapy. Carl A. Whitaker and Thomas P. Malone. xvii + 236 pp. Illus. $4.50. Blakiston, New York. 1953.

Plant Morphology. Arthur W. Haupt. ix + 464 pp. Illus. $8.00. McGraw-Hill, New York. 1953.

Between the Tides. Philip Street. 175 pp. Illus. + plates. $4.75. Philosophical Library, New York. 1953.

Science in Daily Life. Francis D. Curtis and George G. Mallinson. xii + 570 pp. Illus. $3.96. Ginn, Boston, Mass. 1953.

College Chemistry: A Systematic Approach. Harry H. Sisler, Calvin A. VanderWerf, and Arthur W. Davidson. x + 623 pp. Illus. $5.25. Macmillan, New York. 1953.

Logic and Language (2nd series). A. G. N. Flew, Ed. vii + 242 pp. $4.75. Philosophical Library, New York. 1953.

The Epidemiology of Health. A New York Academy of Medicine book. Iago Galdston, Ed. ix + 197 pp. $4.00. Health Education Council, New York. 1953.

Introduction to the Theory of Statistics. Victor Goedicke. xii + 286 pp. Illus. $4.50. Harper, New York. 1953.

A Herd of Mule Deer. A record of observations made on the Hastings Natural History Reservation. Jean M. Linsdale and P. Quentin Tomich. xiii + 567 pp. Illus. $8.50. University of California Press, Berkeley. 1953.

Introduction to Geometrical and Physical Optics. Joseph Morgan. xi + 450 pp. Illus. $6.50. McGraw-Hill, New York. 1953.

Atoms, Men and God. Paul E. Sabine. x + 226 pp. $3.75. Philosophical Library, New York. 1953.

Science in Synthesis. A dialetical approach to the integration of the physical and natural sciences. William H. Kane *et al.* xii + 289 pp. $3.50. Dominican House of Studies, River Forest, Ill. 1953.

Vorstellungen vom Aufbau der Materie im Wandel der Zeiten. Fritz Lieben. x + 384 pp. $7.20. Franz Deuticke, Vienna. 1953.

Safer Smoking. Clarence William Lieb. 106 pp. Chart. $2.50. Exposition Press, New York. 1953.

Physics. Noel C. Little. viii + 648 pp. Illus. $6.00. Heath, Boston, Mass. 1953.

The Road to Abundance. Jacob Rosin and Max Eastman. vii + 166 pp. $3.50. McGraw-Hill, New York. 1953.

Plant Diseases in Orchard, Nursery and Garden Crops. Ernst Gram and Anna Weber. Evelyn Ramsden, Trans. 618 pp. Illus. + plates. $18.50. Philosophical Library, New York. 1953. (Printed in England.)

Cardano the Gambling Scholar. Oystein Ore. xiv + 249 pp. Illus. + plates. $4.00. Princeton University Press, Princeton, N. J. 1953.

INDEX TO VOLUME LXXVI (JANUARY–JUNE 1953)

Title Index of Articles

Subject Index

Contributors

Books Reviewed

Book Reviewers

Verse

Letters

Founded 1848

THE
SCIENTIFIC MONTHLY

JULY 1953

Alaska: Progress and Problems*

ERNEST GRUENING

Dr. Gruening was governor of Alaska from 1939 to 1953. He was born in New York City and received both his B.A. and M.D. degrees from Harvard University. In 1933 President Roosevelt appointed him general adviser to the U.S. delegation at the Pan American Conference in Montevideo. He was director of the Division of Territories and Island Possessions of the Department of the Interior 1934–1939 and commander-in-chief of the Alaska National Guard 1942–1947. He has been a member of the Board of Governors of the Arctic Institute of North America since 1948. Dr. Gruening has also been the editor of various newspapers and magazines and wrote Mexico and Its Heritage *(1928) and* The Public Pays *(1931).*

IT IS difficult to dissociate a discussion of Alaska's progress and problems from its previous history. Perhaps the word "progress" should be defined first, because there may be divergent views about what constitutes progress.

There was, and perhaps still is, a school of thought in the United States that believed the best use of Alaska was to keep it a wilderness area and lock up its resources indefinitely, reserving them for use in some far-distant future. There is still another that, concerned with the preservation of a particular resource, fears the advancing march of modernity and would hold it in check to safeguard that resource at any price. There was until very recently a school of thought in the Office of Indian Affairs that believed it desirable to preserve aboriginal custom and manners unchanged, to try to enclose its human exemplars as hermetically as possible, and to insulate them from the penetration and contamination of the white man's civilization. Such ideas are perhaps entitled to respectful attention, but for the purpose of this discussion I shall try to outline my conception of progress for Alaska.

World population is increasing at an unprecedented rate. Many areas are overcrowded, and sparsely settled areas, if habitable, naturally invite settlement. Considerations of special importance relating to national defense call for the populating of Alaska and for the creation of conditions there that will tend to insure the permanence of such population. Apart from all this, the westward trek of peoples, in search of greater freedom and greater economic opportunity, is undoubtedly the oldest American tradition. It is a tradition that antedated the founding of our republic and indeed was inseparably connected with its origins. It brought the Jamestown colonists, the Pilgrims, the Dutch, and the great variety of European emigrants across the Atlantic in the seventeenth and eighteenth centuries. It led them to cross the Appalachians toward the close of the eighteenth century. It carried them across the plains, over the Rockies and to the

* Based on an address presented at a general session of the Third Alaskan Science Conference, Mt. McKinley National Park, September 22–27, 1952, sponsored by the Alaska Division of the American Association for the Advancement of Science.

Pacific coast in the nineteenth century. It is bringing them to Alaska *now* in the middle of the twentieth century. This contemporary westward migration of Americans to Alaska is in a sense a final chapter in a great episode. This chapter gives new vitality and meaning to the phrase long used to describe Alaska, "the last frontier."

If we accept this interpretation of what is now taking place, it follows naturally that, to whatever extent immigration to Alaska has been achieved, it may be considered progress, particularly if the evidence indicates that the settlement is probably permanent. So we may set down the following as the first item under the head of progress: The population of Alaska *is* increasing very rapidly, and the increase has aspects of greater permanence than previous migrations.

The first Alaska census, taken in 1880, showed a population of 33,426. The next census, 1890, showed the population to be 32,052. This slight decrease need not be considered significant, as it is known that the first census in 1880, taken under great difficulties, was not too reliable. But certain it is that in the ninth decade of the nineteenth century, when the population of the United States increased by over 12,000,000, the population of Alaska did not increase. The census of 1900, reflecting the great gold rush, showed a population of 63,592, or an increase of almost 100 per cent. This impulse carried over to 1910, with the gold strikes in the Tanana Valley, the Iditarod, and Nome following those in the Klondike. The somewhat temporary nature of this influx, coinciding with the diminishing productivity of the gold fields, was reflected in the drop in population to 55,036 in 1920. World War I certainly contributed to that decrease because there was little in the way of economic opportunity in Alaska to induce the young men who had gone to war to return to Alaska. The increase in the third decade to 59,278, or some 7 per cent, indicated no great change. Actually, if Alaska's population figures for the first three decades of this century are contrasted with those of the United States, it will be plain that Alaska, viewed politically, economically, and socially, was static and stagnant. In those same thirty years the population of the United States increased by 45,000,000. But in Alaska there was a decrease, from 1900 to 1930, of 4314, or over 6 per cent. The accidental discovery of gold that brought a rush of prospectors to Alaska at the turn of the century had doubled the population, and by and large that figure was retained for the next thirty years. But throughout that first third of this century Alaska experienced none of the normal growth, either through excess of births over deaths, or through immigration, which was characteristic of the United States as a whole, and which was particularly related to the westward movement of population.

In Alaska the real change began in the 1930's. The 1940 census showed that Alaska's population had reached 72,524. This was an all-time high for Alaska; it showed an increase of 22.3 per cent, a percentage of increase in the preceding decade that was exceeded in that same decade by only two states in the Union. Thus Alaska in the fourth decade of this century—from 1930 to 1940—showed a percentage of increase greater than that shown by forty-six states. The Alaskan increase of 22.3 per cent contrasted with a 7.2 per cent increase for the United States as a whole.

In the fifth decade, however, the increase and the acceleration were even greater. The 1950 census showed a population of 128,643, or approximately 77 per cent in the last decade, an increase greater, percentagewise, than that of any state or territory, and a population which had more than doubled in 20 years. Today Alaska's population may be conservatively estimated at over 165,000, and its increase is not only continuing but continuing at an accelerating rate. We may conservatively forecast a population of 200,000 in 1955, and of 300,000 in 1960. The really important aspect of this growth is that it contains many elements of permanence formerly lacking. Until recently, until this present growth got under way, Alaska differed substantially from the previous frontiers in American history. Whereas the westward march from the Atlantic to the Pacific contained a large proportion of would-be settlers, individuals who hoped to establish themselves permanently in new surroundings, no such purpose actuated the overwhelming majority of those who came to Alaska from the time of the first population increase in the late 1890's until the early 1930's. They came rather as the Spanish and Portuguese came to Central and South America, not to take up permanent residence in the New World but to get gold and take it back with them to the mother country. Obviously, with that motivation there could be no permanence but only transience. Desire to improve the environment was lacking, and the resulting lack of improvements perpetuated the transience. It was a kind of vicious cycle. Those who came to get rich quickly—if possible—and get out found little here to make permanent residence attractive, and, their intentions being what they were, they did little to make the environment better. This, of course, was by no means true of a small minority who stayed on, but by and large

the pattern was set by those whose stay was purposefully temporary. This attitude was reflected in the relative inaction of successive territorial legislatures. And the Federal Government continued its course of indifference and lack of interest.

What, then, are the factors that have contributed to this population increase and to its presumed permanence? The answer lies in part in a fundamental change in Federal attitude which has been reflected in Federal policy and action. An important motivation for this change may be found in international events. In the first three decades only two significant *Federal* actions contributed to Alaska's development. One was the construction of the Alaska Railroad, begun in 1915 by the Wilson administration and completed in 1924. The second was the establishment of a land-grant college—the Alaska Agricultural College and School of Mines, authorized in 1915 and opened in 1922, which in 1935 became the University of Alaska.

In the 1930's there were several such actions. First was the increase in September, 1933, of the price of gold from $20.67 to $35 an ounce. This gave Alaska's second industry, mining, a great lift, which continued for the rest of that decade. Second was the Matanuska colonization project in 1935, which brought 200 families into the Valley, gave agriculture in Alaska an impetus, and widely publicized Alaska as an area of potential settlement. Third was the extension of social security to Alaska in 1937 by action of the Alaska legislature after a request from the Federal authorities.

In the 1940's international events and the Federal actions resulting therefrom stimulated population growth. Preparations for defense, totally lacking previously, brought in thousands of construction workers and G.I.'s. Not a few of the G.I.'s settled in Alaska at the end of hostilities.

The defense program for World War II brought the first major airports and radio range stations to Alaska. They made possible commercial air service between the States and Alaska, which was initiated in 1940 and amplified in 1945 by certification of additional carriers and additional routes by the Civil Aeronautics Board.

The defense program also brought the Alaska Highway in 1942 (although a highway over a different route had been recommended three years earlier by an American commission), and the Glenn Highway and with it the beginnings of a territorial highway system.

The World War II defense program established three permanent bases in Alaska (not counting the Aleutians) whose existence has benefited the economies of their areas: Fort Richardson near Anchorage, Ladd Field near Fairbanks, the naval station at Kodiak. It established a scattering of CAA stations throughout Alaska. It made a Coast Guard District of Alaska.

These were the material contributions incidental to defense when hostilities ended in 1945. They were substantial. They aided the economy of Alaska, created some permanent and needed improvements, and established a great new interest in Alaska in the States as a new frontier of promise and opportunity. The last contribution was of great importance.

The years since 1945 have seen a great amplification and intensification of the process. The "Cold War" and defensive preparations to avert a third world war have brought extensive housing, the first highway program in Alaska in the interior (though not in southeastern Alaska), great airports at Anchorage and Fairbanks; a public works program on a fifty-fifty matching basis which is helping to provide schools and utilities for our towns; the beginnings of an electrification program by the United States Reclamation Service at Eklutna; a program of agricultural research; a new interest in the mining of strategic minerals; the erection of the Geophysical Institute at the University of Alaska.

No less significant, however, have been the territorial changes in attitude and performance. The Territory was becoming conscious of its responsibilities and its destiny. It established a full-time health program in 1945; a territorial housing authority; a territorial Department of Agriculture; a development board. In 1946 it passed a Veterans' Act under which several thousand Alaska veterans were loaned funds to buy or build homes or to acquire farms or businesses.

In 1949 the legislature established a territorial Department of Fisheries. It set up a Department of Aviation, and under it, with funds from aviation gasoline and some Federal matching, brought about the construction or improvement of a hundred airports and seaplane facilities. In 1951 the legislature enacted legislation to promote the tourist industry on a fifty-fifty matching basis with private subscriptions. A most constructive step was also taken in banning billboard advertising.

Most significant of all was the enactment in 1949 of a comprehensive revenue program. Previously taxes had been negligible, and whole categories of businesses and individuals deriving substantial profits and income from Alaska had paid no taxes whatever. The result was that the revenues were insufficient to support essential public services. Great wealth was extracted from the Territory, but next to none stayed here for its development. That

has now been fully remedied by an inclusive yet moderate tax structure. The Territory will not only balance its budget at the end of this biennium but will also emerge with a surplus of several million dollars. The Territory has no indebtedness. By being financially strong and sound, it is able, and will increasingly be able, to render the services—schooling, health, welfare—which the American people properly expect as part of their way of life.

So far the record of progress relates chiefly to governmental action, Federal and territorial. But private industry is coming to Alaska. In this very year the first pulp plant in Alaska is under construction near Ketchikan to utilize the long-unutilized forests; a plywood mill is under way at Juneau; the Aluminum Company of America is making gigantic plans for processing at Dyea; extensive private oil drilling is scheduled for the coming season in the Katalla-Yakataga area; new tin and coal mines have been opened—all undertakings of private capital, which is entitled to every legitimate encouragement and assistance, particularly since so large a part of Alaska's economy has been, and will for some time continue to be, based on government spending.

These, then, are some of the factors that account for Alaska's recent and accelerating growth and progress.

At this point the above rather limited identification of *progress* with *growth of population* should be supplemented by suggesting the goal to which that progress is or should be directed. This goal, in the process of its attainment, predicates a highly important role for Alaska. Given its unique geographical position—its location in far northern latitudes, its fronting on the Arctic Ocean, its extension into the Eastern Hemisphere, its juxtaposition to the Soviet imperialism—all characteristics which no other area under our flag possesses, Alaska's destiny must be envisioned and shaped on the cosmic scale to which its great size, position, and potentialities invite our national enterprise.

In short, the destiny of Alaska is to be not merely militarily a bulwark of defense (and, if necessary, of offense) for this continent—and hence for the free world—the strategic assignment Billy Mitchell so brilliantly forecast, but, concomitantly, a no less vital outpost for the American idea and therefore a living demonstration of all that is best in an American society. That is a major role in national and world affairs, but it requires no great imagination to appreciate its desirability and value. It is a challenge only to our powers of performance.

To date, the cultures of only two peoples have established themselves permanently this far north. They are the Scandinavians (and Finns), who have erected a high civilization and a stable economy in these latitudes and whose concepts of freedom are similar to ours; and they are the Russians who, under the present rulers in the Kremlin, have established at our doors a militant society whose concepts are antithetical to ours and menace freedom wherever it may be found or hoped for on earth. Therefore the *national* stake in the attainment of Alaska's destiny is emphasized here, and the suggestion is made that it be constantly borne in mind as we discuss Alaska's problems.

The most essential step toward this major objective is statehood. This is not the place for a detailed presentation of the cogent arguments for it, but here are a few salient points:

1) The people of Alaska asked for statehood in a referendum six years ago.

2) Public opinion in the United States overwhelmingly supports statehood for Alaska.

3) Every Congressional committee to which the issue has been referred has, after study, reported favorably upon it.

4) No arguments have been advanced against statehood which are not in essence identical with those presented against the admission of states previously admitted.

5) The minority opposition in Alaska has dwindled almost wholly to contention about the statehood bill itself—introduced in the last Congress—with regard to whether it was generous enough.

6) The national interest will be greatly served by statehood and disserved by its continuing denial.

Statehood will go far to provide a solution, or at the very least will facilitate a solution, of many of Alaska's problems. All these problems are related directly or indirectly to the issue of continuing growth in population and of its permanence.

Among the most pressing are land problems. Alaska is a very sparsely settled part of the world. It has about one person for every 3½ square miles, in contrast to the United States with about fifty per square mile. Among the obstacles to settlement are the Federal land laws. In general, these are the laws adopted decades ago for the forty-eight states. They are obsolete, inappropriate for Alaska, and to a degree unworkable. Poor as they are, their application is further handicapped by the failure of Congress to appropriate adequate funds for surveys, and likewise by red tape in their administration. This is a field in which Alaskans are estopped by law from helping themselves.

Over 99 per cent of Alaska is Federal land—either public domain controlled by the Department of the Interior, or reserved or withdrawn by some other

Chilkoot Barracks and Haines, Alaska. Terminus of a new road to the interior (U. S. Forest Service).

Federal department. The Organic Act for Alaska passed by Congress in 1912, likewise an obsolete and outworn instrument—against whose limitations of territorial autonomy every governor since its enactment has protested—leaves the business of basic land legislation in the hands of Congress. Congress has done little to improve the situation.

The first Alaska legislature created by the Organic Act, meeting in 1913, memorialized Congress on various basic issues which the Organic Act specifically forbade the territorial legislators to do anything about. Its memorials on the subject of land were fourfold. *One* asked that the land laws be revised and simplified to promote settlement. A *second* asked that an end be put to reservations and withdrawals, and that some of those already made be restored for public entry. A *third* asked that an end be put to shore-space reservations. A *fourth* dealt with still other aspects. Not only has Congress paid no heed, but the reservation and withdrawal policy has continued without let or hindrance so that today approximately one-fourth of Alaska, some 94,000,000 acres or 147,000 square miles, is in some form of withdrawal or other. This is an area larger than our third largest state, Montana. It is an area larger than the total areas of eleven smaller states—the six New England States and New Jersey, Delaware, Maryland, South Carolina, and West Virginia. This does not mean that some of these withdrawals have not been for a good purpose. But the withdrawal process has become so tangled, haphazard, and confused, with withdrawals overlapping and with their original purpose long since forgotten and invalid, that it has taken the Department of the Interior two years to find out what and where these withdrawals are. The question of returning such withdrawals, whose present use is not clearly essential, still lies ahead. A subcommittee of the House Committee on Interior and Insular Affairs is coming to Alaska this week to address itself particularly to the problems of the revision of our land laws. But experience has shown that there is often a long gap between Congressional investigation and favorable action.

What is needed is a thorough overhauling of our land laws relating to Alaska. Their objective should

be to promote settlement. One of the arguments repeatedly used by the Congressional opponents of statehood is that only a small fraction of land in Alaska has passed from Federal to private ownership. Yet Congress itself has held the keys and has persistently refused to unlock the door.

The statehood bill, which lost by a margin of one vote in the last Senate, would give the State of Alaska some 23,000,000 acres of land to be chosen by the authorities of the new state. Reserved land is, of course, excluded from this offer which would, nevertheless, give the State of Alaska an area as large as New England or, to put it in another way, give Alaska more land for its use or disposal than is now possessed by four public land states, namely, Arizona, Idaho, Utah, and Nevada which have, respectively, 19,327,927, 19,269,006, 14,803,363, and 8,894,920 acres not in public domain. The *percentage* of this proposed grant to the new state of Alaska is, of course, smaller than that given to the public land states, leaving some 93 per cent of Alaska's total area in public lands, as compared with Nevada's 87 per cent, Arizona's 73 per cent, Utah's 72 per cent, and Idaho's 67 per cent. But here the percentages are less important than the actual amount of land—the best unreserved land—the new state would receive.

But statehood is introduced here again in this discussion merely to point out its remedial benefits, which are probably not otherwise obtainable. The process of revision of the land laws—as distinct from the question of land grant to the new state—would be speeded by statehood to the extent that the addition of two senators and a representative with a vote would aid the process.

There is another aspect of land law revision which requires attention. Two important geographic areas are now in national forest reserves. They are the Tongass National Forest, which roughly coincides with all southeastern Alaska, an area of some 16,000,000 acres, and the Chugach National Forest of some 4,000,000 acres, which includes the next habitable coastal area to the westward. These two areas include seven of the fourteen principal towns in Alaska: the capital, Juneau, and Ketchikan, which ranked respectively second and third in the last census, Sitka, Petersburg, Wrangell, Cordova, and Seward.

The forestry resource is extremely important to Alaska. Its utilization on a major scale is just beginning with the construction of Alaska's first pulp plant near Ketchikan. Its functioning should in no wise be impaired. But there is an aspect of these two national forest reserves, unrelated to the forestry function, which is unique. Nowhere else under the flag do national forests blanket a whole economic area or include and circumscribe a state's principal urban centers. The situation would be analogous to having the national forests occupy the total western fourth of Washington and Oregon and surround the cities of Seattle, Tacoma, and Portland. Recently a small beginning has been made in southeastern Alaska by excluding from the forest reserve, and returning to public domain for disposal under laws and regulations applicable to it, limited areas surrounding these towns. The objective is to enable these towns to develop suburbs and to achieve the normal development of other American cities. But, apart from the fact that even this move, while in the right direction, has only begun, uncertain as to issue and insufficient in scope, one great obstacle to normal development remains. That is that the highway construction policies in these important areas, which include half the principal urban centers of Alaska, are not in the hands of road-building agencies whose mandate and purpose it is to develop highway communications. The control rests in the hands of the regional forester, whose mandate is forest conservation and utilization, and not commercial, industrial, or urban development. The consequence of this anomaly is that, while the interior of Alaska is developing a fine highway *system,* linked to the continental highway system, southeastern Alaska's insignificant road mileage consists of mere short stretches of highway leading only a very slight distance out of each town and connecting with no other community.

The remedy is to exclude from the forest areas the *rights-of-way* for proposed and needed through highways and permit such arteries to be included in the construction program which is rapidly speeding the development of interior Alaska. Such a reform would in no wise interfere with the forestry function but would promote growth, settlement, and other development. Desirable sites within the forest areas should likewise be made easily available on a fee simple basis for the development of tourist lodges.

So much for the basic land problem.

Since its discussion has brought us to the subject of highways, it will be well to continue with it. Highways are indispensable to development and settlement. One has only to observe the springing up of lodges, tourist cabins, and homes taking place in Alaska whenever and wherever a highway cuts through the wilderness to be aware of this visible result. Among the memorials to Congress adopted by the first legislature in 1913 was a request for highway construction. Except for the Richardson

Highway built in the second decade as a low standard road, Congress did nothing (until three years ago), but discriminated grossly and uniquely against Alaska by failing to include Alaska in the Federal Highway Act—except for a limited participation in forest areas. No other territory suffered this discrimination. Alaska's share of Federal funds in the twenty-nine years since the passage of this act has been estimated at $350,000,000. Fractional matching on some formula would have been required, but Congress declined to sanction any formula. World War II's exigencies, however, brought the Glenn Highway, connecting Anchorage with Fairbanks and Valdez by means of the Richardson Highway, and also that part of the Alaska Highway between the Yukon boundary and Fairbanks as well as the Tok cut-off. Three years ago, in consequence of the Soviet threat, Congress authorized a road-building program with an annual appropriation of between 20 and 25 million dollars which includes black-topping the principal highways. Under this program a highway starting at Homer and connecting the Kenai Peninsula has been completed; Seward has been connected with the system. A road into the Forty-mile and to the Yukon at Eagle near the Canadian boundary and a connection with Dawson have been completed. Two other major projects have been begun, namely, a road to Mt. McKinley Park from the Richardson Highway and a highway from Cordova to Chitina over the old Copper River and Northwestern Railway right-of-way.

In addition, numerous shorter farm and access roads have been built. This is a fine beginning, but it should be considered only a beginning. It is fair to consider that Alaska's road-building program came a quarter of a century after the Federal Government had started its cooperative highway aid program with the forty-eight states and other territories. Indeed, the present Federal highway program, although most ably administered in Alaska, is already demonstrating its insufficiency, for surfacing and maintenance are consuming an increasing share of the budget. Thus the Paxson-McKinley Park Highway, begun two years ago, has had its current fiscal year allocation cut to $500,000, and the Cordova-Copper River Highway, which would link the one as yet unconnected city in central Alaska with the territorial and continental highways system, starts with an appropriation of only $650,000, although the cost of the project is estimated at $12,000,000. If the road program is to be effective in building up Alaska and serving its pressing needs, it must expand, and the annual Congressional apropriation—some $20,000,000 for the coming fiscal year—must at least be doubled for the next ten years; at the end of that period this would approximate in appropriation what would have been Alaska's rightful share had not Congress after Congress discriminated against Alaska by failure to include it in the Federal Highway Act. But more pertinent than the argument based on atonement is the fact that Alaska will require, at the very least, the mileage that an annual appropriation of $40,000,000 for the next ten years will construct. A hundred-million-dollar annual program would be even sounder. This is clearly a Federal function, although the Territory which now builds and maintains some smaller road and harbor projects could properly be expected to bring its present two-cent gasoline tax up to the national average, which is about six cents.

The need for harbor projects presents another problem. For years this has been the function of the Army Engineers, but in recent years they have all but ceased to construct in Alaska. The responsibility is, of course, the Congress's. The engineers have studied, surveyed, and recommended many needed projects for small boat harbors to take care of Alaska's fishing fleets, for flood control and much else. These projects remain pigeon-holed. In recent years there has been a tendency to stigmatize such river and harbor projects as pork-barrel projects. That is not the case in Alaska. These are sorely needed projects that have hitherto been beyond the financial means of the Territory and local communities. But the need remains. The failure to provide them checks growth that would otherwise take place.

In a related category is power development. The recent announcement of Alcoa's plan to harness the upper Yukon and to drop some of its waters onto Alaskan territory at Dyea in order to process alumina is epoch-making. Its coming will be the most important single event in the history of Alaska's development. It has several unique aspects.

First is its magnitude. It will call for an initial investment of $400,000,000. It was reported recently in a national magazine that this is the second largest investment ever made in a single plant, being topped only by a steel plant in Pennsylvania which cost $421,000,000. Actually Alcoa's investment will go well beyond $421,000,000 and will be, not the second largest, but the largest investment by a private industry under the flag. It will employ some 4000 people 365 days a year and will have an annual payroll of about $25,000,000. These people will require a town housing 20,000 persons to be built in the Dyea Valley. At least 10,000 more can be expected in neighboring Skagway. The beneficial economic consequences, direct and indirect, to

Alaska stagger the imagination. In itself it will increase Alaska's present population by nearly 20 percent. By itself this plant will supply the most important ingredient hitherto missing from Alaska's economy—a large year-round non-governmental payroll.

Second is the fact that the power to be utilized originates in Canada in the headwaters of the Yukon and adjacent lakes. It is now running to waste and will continue to do so unless harnessed by the force of gravity which the 2500-foot drop to sea level on the Alaskan side of the boundary provides.

Third, the project utilizes a raw material, bauxite, which as far as is now known does not exist in Alaska but will be brought here from Surinam in South America and be partly processed in the States before arrival here. The land on which this development will take place has not been found of any value and has likewise been unutilized, although known and accessible since the days of '98. Thus this project is unique on several counts, and it represents the antithesis of exploitation and wastage of natural resources. The hydroelectric development is essential to this industry, for large quantities of cheap power are indispensable to the manufacture of aluminum.

This very fact calls attention to the urgent need of developing cheap power elsewhere in Alaska to attract industry. Alaska and adjacent Canada have in all probability the greatest undeveloped power potentials on the North American continent. But private industry is seldom in a position to finance such power development. In the diversification of Alaska's economy, which has hitherto been highly limited by seasonal factors, its only principal industries, fisheries and placer mining, operated only in the summer months, and more year-round payrolls are needed. Cheap and abundant power alone will attract them.

The answer—in the absence of private enterprise, able, willing, and ready to undertake the great cost of power production—is governmental development. One project, the harnessing of Lake Eklutna to supply the greater Anchorage area, is under development. It will cost about $30,000,000, supply 30,000 kilowatts. All this is, of course not a grant or a gift, but a loan fully repayable with interest by the users—the City of Anchorage, the Chugach and Matanuska electric cooperative, and the Army. It is an interesting commentary on Alaska's great power needs that some two years before this project's completion the entire output is already preempted. When completed, it will not satisfy the current demand—to say nothing of the expanding demand of the greater Anchorage area. The need is for more power projects, the most promising of which is now the harnessing of the Susitna River. This project, which has been under study by the Bureau of Reclamation, will supply an estimated 400,000 kilowatts, or thirteen times as much as Eklutna. It would generate power at less than ten mills and make it available for 200 miles on either side of the rail belt. The problem is how to get authorization and appropriation for such a project, which, let it be emphasized again, is not a gift or grant but the soundest kind of investment by the government in behalf of private enterprise, and repayable with interest. But how to get it? Congress was, with the greatest of difficulty, even with the potent cooperation of the military and the support of numerous public and private agencies, persuaded to authorize the Eklutna project. A reactionary administration would in all likelihood view other public power developments coldly, even though private capital was unavailable for the purpose. Both the executive and the legislative branches will have to be persuaded. The benefits, let it be ever borne in mind, would be national as well as Alaskan. The representation of two senators and a voting representative would greatly improve the chances of realizing such a project.

It will be seen at this point, and it becomes increasingly clear, that Alaska is largely dependent on the vision and understanding of the Congress, on the whole a distant body with little firsthand knowledge of Alaska's needs and problems and of their intimate relation to the national interest.

And that brings us to another problem—or rather a whole series of problems—one of our greatest problems in Alaska, lack of knowledge. Very little is really known about Alaska and its resources. Research—intensified research, enlarged research, continuing research—is essential.

Yet it is an ironic fact that, while private industry has learned the importance of research,. government, on the level where the means for research are provided, has not. When Congress engages in its periodic retrenchment drives, research is likely to be the first victim. Possibly an exception is made of research in the field of national defense, where Congress has responded well. But, in Alaska, defense and economic development, defense and population growth are almost inseparable.

It would be superfluous to stress the importance of agriculture. It is the most basic of all economic pursuits. It is the greatest stabilizer of population. Yet, despite the fact that thirty years ago a land-grant college was established in Alaska, as a result of which it was entitled to certain annual appro-

priations for research and extension work under a variety of Congressional enactments, Congress persistently refused to appropriate the legally authorized funds. Year after year our voteless delegates would plead eloquently but in vain. The appropriations that were made were negligible, far below the sums legally Alaska's, and wholly inadequate. It should have been clear that Alaskan latitudes, with their unstudied climatic, soil, and entomological factors, required specific research of their own. Nothing was done about it until—as with so many recent Alaskan developments—word of intensive and successful agricultural activity in comparable latitudes in Soviet Siberia reached the Congress. Then for the first time a cooperative program of research under the joint auspices of the federal Department of Agriculture, the University of Alaska, and the Territory was provided. It was begun most auspiciously three years ago, even though on a relatively modest scale. But even now it is already menaced by Congressional retrenchment, which will force suspension of some important research already undertaken. Such curtailment is a constantly impending threat which in itself impairs the morale and effectiveness of those engaged in this essential undertaking.

In the field of fisheries, which involves Alaska's greatest natural resource, the Pacific salmon—a field Congress and the Department of the Interior insist on retaining under Federal control—appropriations for research have been almost totally lacking, and funds for enforcement of presumably sound conservation measures always insufficient. In consequence the annual salmon runs are slowly but surely diminishing. The Territory has attempted to alleviate the situation by setting up its own Department of Fisheries, engaging in research, experimenting in the expansion of salmon spawning areas, and even contributing stream guards; but the problem basically is lack of adequate sustenance by long-range governmental planning.

In the field of health it has long been clear that adequate knowledge of arctic and subarctic physiology and pathology were lacking. The question is intimately related to national defense in Alaska. Soviet knowledge in this field, based on long experience in arctic and subarctic latitudes, appears greatly to exceed ours, although since the lowering of iron curtains our information on the subject is largely presumptive. For years the establishment of an Arctic Institute of Health at the University of Alaska or elsewhere in our Territory has been urged upon the United States Public Health Service, the Bureau of the Budget, and the Congress. Nothing has resulted thus far.

In many of Alaska's other problems the Federal Government plays a controlling part. The question of aboriginal or possessory rights or claims requires settlement. Five years ago Congress finally gave evidence of its recognition of the problem by passing the Alaska Timber Sales Act, which froze the fees collectable for stumpage in escrow pending legislation to dispose of the problem. An excellent bill sponsored by Alaska's delegate and endorsed by the Office of Indian Affairs in Washington still awaits action by the Congress.

The judiciary and law enforcement were specifically retained for the Federal authorities under the Organic Act of 1912—except within municipalities. The Organic Act of 1884 provided one Federal judge and four lower court judges, called United States commissioners. Congress provided no salaries for them but arranged that they subsist on the fees they collect for probate and other services from the public. The number of commissioners has grown, and there are now more than fifty, but they are still unsalaried despite repeated efforts to get Congress to remedy this disgraceful state of affairs. Except in the four or five principal cities, these fees, which are expected to furnish the commissioners' livelihood, are insufficient to keep body and soul together. The four Federal district judges who appoint the commissioners have a serious problem in finding competent and worthy commissioners in the smaller communities. The administration of justice—cornerstone of the free society—suffers. Under statehood Alaska would have its own judiciary, and, needless to say it, it would be paid.

The police power—outside the incorporated towns—is likewise vested by the Federal Government in the four United States marshals and their deputies. But appropriation to provide an adequate number of deputies is never made. Congress has grossly failed in carrying out its responsibility for law enforcement in the Territory which nevertheless it has insisted on retaining. A visible symbol of the Federal failure in this field is the Anchorage jail—a Federal institution—whose foulness has been the subject of repeated protests.

The Territory, however, has made an excellent entry into the field of law enforcement by creating an efficient highway patrol, now numbering over forty officers and men, in an endeavor not merely to enforce traffic regulations but also to supplement the Federal Government's feeble efforts. The gap, however, has not yet closed, and the responsibility, as long as Alaska remains a Territory shackled to the Organic Act of 1912, is still the Federal Government's.

Progress in Alaska has, of course, brought its

problems. It is gratifying that they are the problems of growth and not of shrinkage. But basic to an adequate solution of these problems should be the understanding that Alaska's growth and development are and should be of great national concern, and that their rapid attainment is in the national interest. The need for better and fuller Federal cooperation is emphasized not merely by that concept, but also by the long years of neglect, by the continuation of many Federal practices of omission and commission that are unjustifiable, and by the vital importance of Alaska in the face of international events.

If it be urged that Congress raise its sights and increase its appropriations for essential objectives in Alaska, there should be no disposition to evade any responsibility or participation that can properly be borne or shared by the 165,000 people now resident in Alaska. As of today they are, by and large, doing their part. They can without difficulty today support *a* state. But Alaska, from the standpoint of national interest, should be viewed as something more than just another state, be it the forty-ninth or the fiftieth.

Federal policy should keep as its objective not merely economic development of Alaska, but also parallel political development and social development which require the fullest measure of self-government obtainable under our American system.

However, given the need of fulfilling Alaska's destiny as a *national* objective, statehood should not and cannot write *finis* to substantial federal assistance in achieving that goal. Nor is this postulate inconsistent with the demand for the full political equality of statehood. Actually statehood may be considered a helpful and indeed an indispensable instrument in helping the nation obtain its goal in Alaska. Politically, Alaska may be just another state when Congress acts and the necessary preliminaries have been complied with by Alaskans. But materially and ideologically the nation can have incorporated in its design and resource a great northern domain, in essence a new domain in its developed potency, militancy, and resourcefulness. That domain can embody all that is best in the American way of life and advance the front of democracy farther west and farther north than it ever has been.

SPACE ROCKET

Now the velocity of speed,
    As scientists have reckoned,
By gravity has been decreed
    At seven miles a second.

Take off from earth! No longer bound
    To either time or place;
No longer tethered to the ground,
    But free to wander space.

What worlds await us, once we dare
    To leave the earth behind,
Piloted through realms of air
    In rockets of the mind.

MAE WINKLER GOODMAN

Cleveland, Ohio

Experimental Discoveries Announced at the Meeting of the American Physical Society Fifty Years Ago

GORDON FERRIE HULL

Professor Gordon Ferrie Hull is professor emeritus of physics of Dartmouth College. He was born in the province of Ontario, Canada, and was graduated from the University of Toronto. He received his Ph.D. degree from the University of Chicago and also studied at Cambridge and Berlin. Only a few of the facets of his interests are mentioned here. He has made outstanding contributions to the study of light and ballistics; his teaching influence has been felt in many colleges and universities; his devotion to the field of physics has been shown by active participation in the programs of the American Physical Society, the Association of Physics Teachers, and as Vice President, AAAS Section B.

IN THESE DAYS of almost frenzied activity in research in realms beyond the ken of the farthest gazing physicist at the beginning of this century, it may seem wasteful of your time to deal with the program of the American Physical Society fifty years ago. However, the Society's secretary having invited the paper, to him belongs the onus or the credit. In seeking for one to fill this role, our secretary was unfortunately limited in his choice. Of those who presented papers at the Denver meeting of 1901 and the Washington meeting of 1902–1903, I am the only one now living. Time marches on. Our secretary also desired to emphasize the fact that the papers by E. F. Nichols and myself on the pressure of radiation were presented at those meetings. Indeed, he asked that I give an address on "Fifty Years of Light Pressure." I was reluctant to do so for the reason that there were other papers presented at the Washington meeting of Christmas week, 1902, which were of very great importance, an importance not recognized then or even now. I asked that I be permitted to give a paper on the above title. The experimental discoveries referred to were the pressure of light, the identification of the alpha particle and cosmic rays. I shall deal with these in detail later.

Discovery of the Pressure of Light

I first met E. F. Nichols at the dedication of the Yerkes observatory in 1897, at which function—though neither of us was an astronomer—both were delegates. He had been working on methods of extending the spectrum of an incandescent body toward longer waves, and he had devised instruments for detecting and measuring the energy. I had published an article on the measurement of very short electric waves. Thus according to Maxwell's marvelous electromagnetic theory, still quite new, we had been working in neighboring realms.

A large part of Maxwell's theory had developed during the years 1861 to 1873 and had been experimentally verified by Heinrich Hertz in the discovery of electric waves. But there was one item not yet experimentally verified—it was that a beam of light falling upon a surface should exert a pressure equal to the energy density of the radiation in front of that surface. (This theoretical result had also been derived by A. Bartoli in 1876 from thermodynamical considerations.) At the Yerkes meeting Nichols and I had numerous discussions about all the extraordinary conclusions of the Maxwell theory.

Mr. Nichols was called to Dartmouth in 1898, I in 1899. By the summer of 1900 it was perfectly clear that we were detecting the pressure of light as contrasted with the very disturbing effects due to gas action. No publication of our work was made at that time except locally. Our first paper was read in Denver in August, 1901. In it we showed that we had measured all the quantities involved so as to give an agreement of 20 per cent between

the experimental and theoretical values. But there was omitted from that paper the almost exact agreement between the two values when the energy density had been measured by a bolometer method which we had reason to believe was faulty. Our more complete paper was presented at the Washington meeting in December, 1902. This gave an agreement within 1 per cent between the two values, but there was an error in that paper of about 6 per cent which was not detected for about thirty years. Our more exact work, together with the confirmation by Lebedew, established the reality of light pressure and verified the theoretical result set forth by Maxwell in 1873 and by Bartoli in 1876. A beam of light possesses momentum; and the pressure of light became a phenomenon of nature as universal as gravitation.

Before going on to consider the consequences of this phenomenon, let me call attention to one feature of our work which was of great importance. We plotted the positions of the torsion system at quarter period intervals after exposure to light. Thus we noted that the initial motion of the vane, whatever the pressure of the air in the bell jar, was always away from the light. That initial motion was due to light pressure. Had William Crookes done this, had he made his work quantitive, he might have found that light itself produces a force on a surface on which it falls. For Crookes labored continuously for about six years, from 1873 to 1879, on work which resulted in the Crookes radiometer. He and his assistant were extremely ingenious in designing light mills, but they did not have the technique of using quartz fibers for suspensions and for producing a balancing and restoring force against the action of light. Let me say that quartz fibers are still the supreme materials for this purpose. Yet though Crookes failed to establish the existence of light pressure, his keen intuition led him to say in his Bakerian Lecture of 1879, "The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter may exist in a fourth state and where the corpuscular theory of light may be true."

I have said that there were three important experimental discoveries announced at the meeting of 1902–1903. Of these, that of light pressure was perhaps of the least importance, yet it is probable that it produced more excitement among the members present than any other paper of that session, simply because we gave an experimental demonstration that was completely convincing. The demonstration was the more exciting on account of the fact that various prominent physicists had predicted that, though theory required that light should produce pressure, its amount was so small that it probably never would be detected. This statement was made, for example, by Drude, one of the foremost physicists of that time, in his excellent textbook on optics, the English translation of which appeared in 1902.

After the reality of the pressure of light had been established, we had to assume that not only is there an attractive force between masses, there is also a repulsive force. Both follow the law of the inverse square of the distance between the masses, but there the similarity ends. The repulsive force depends on the temperature of the masses, their surfaces and absorption coefficients, and the ionized condition of their vapors. The gravitational action of two masses depends only on the masses and distances. It cannot be shielded in any way. The repulsive forces depend on all the variables above, but not on the masses. They can be completely shielded. Consequently, the repulsive forces frequently cannot be computed. If the bodies are receding, the pressure is lessened. Does gravitation depend upon the radial speed? I asked one of the foremost philosophical mathematical physicists of our society that question, and he replied that if there is an answer he does not know it. One of the younger physicists here should try to obtain an answer.

For a spherical particle of the mean density of the earth, with a totally absorbing surface, and a diameter of 2×10^{-5} cm, the radiation pressure and the gravitational pull by the sun would be equal. Here the solar radiation constant has been taken as 1.92 calories cm^{-2} min^{-1}. Particles of the same density but of smaller diameter would be driven away from the sun. For a sphere of water the critical diameter would be 5.5 times the above value, or 1.1×10^{-4} cm. Particles the wavelength of the edge of the infrared, just smaller than the above, would be gradually forced away from the sun, while those just larger need not move quite as fast as the earth to be in equilibrium in the earth's orbit.

There are limitations to these arguments. If the diameter of a particle is small compared with the wavelength of light, the pressure of radiation becomes very small.

There is a further effect which may enter. To show how baffling it is, I give its history. Let us consider a spherical mass, a few centimeters in diameter, at the earth's distance from the sun. Presumably its surface would have the same temperature as that of the earth, and it would radiate energy at the same rate. But on account of its speed forward there would be a greater pressure on the front than on the rear surface, since the

energy density in front is greater than behind. Let us call the excess force F. In 1903 J. H. Poynting computed F to have a certain value, $Rv/3C^2$, where R is the rate at which the particle is radiating energy, v is its velocity, and c that of light. Ten years later Sir Joseph Larmor took up this problem and arrived at a value of F three times that of Poynting. A year later he revised his value to $3/2\ F$. Then in 1918 Leigh Page of Yale—whose recent death is mourned by all members of our Society—by making use of relativity operations computed that F was exactly zero. Twenty years later another of our members, H. P. Robertson, a mathematical physicist of the highest order, now of California Institute of Technology, then of Princeton, under the stimulation of that seer in astronomy and physics, our own Henry Norris Russell, made an elaborate analysis of the problem, using the methods of relativity. Neglecting small quantities of the second order, he arrived at the value of $3F$, that first obtained by Larmor on classical grounds.

On account of this retarding force, a particle 1 cm in diameter and of the density of the earth 5.5 g/cm² at the earth's distance from the sun would be slowed down and would very gradually spiral into the sun. Smaller particles, but larger than the critical size, would spiral in correspondingly faster.

Long before the experimental proof of the reality of the pressure of light, that property had been applied to account in part for various phenomena, such as the solar corona, zodiacal light, comets' tails, and northern lights. That there were fine particles in the tails of comets which were driven away from the sun by light pressure was suggested by Kepler in 1619, by Euler in 1740, and was discussed by Fresnel in 1825. In the 1890's, Arrhenius discussed at great length the possibility of this application. Now Kepler and most of those who followed him had regarded light as corpuscular, but when the wave theory of light was apparently established it was not evident that there would be any pressure. Even after the theoretical statement by Maxwell, it was rejected by some of the foremost astronomers of the closing decades of the nineteenth century, for example, by C. A. Young and Simon Newcomb. But there seemed to be no question whatever as to there being a repulsion of the particles of the tails by the sun, and the repulsive force had different values, even for parts of the tail of one comet.

However, we thought that we could make a laboratory comet's tail. Into a glass tube shaped like an hourglass we placed some puffball spore, lycopodium powder from which the oil had been driven by heat. The tube was exhausted. When a beam of light was focused on the particles after they had fallen through the constriction, the particles, at least many of them, were blown away from the light source. To us at that time it appeared that our demonstration in Washington was too successful. The force driving the particles away was of the order of the earth's gravitational force, which would make it 160 times the sun's gravitational force. We thought it should be small compared with the earth's gravitational force. The repulsive force is large compared with solar gravitation on many comets' tails, but we didn't know how large. Here are some data collected since 1902. For the Moorehouse comet of 1908 the ratio of repulsion force to the sun's gravitational force was computed by different observers to be 62, 72, 162, 105, 151, 88, 156. For the Halley comet of 1909–1910 the ratios were 194, 70, 90. Now the tail of a comet consists usually of a number of tails, all with a fleeting existence. A comet is constantly losing and replenishing its tails. These tails are frequently self-luminous ionized hydrocarbon molecules, and Karl Wurm of Potsdam in 1935 estimated that the repulsive force in an ionized carbon monoxide molecule would be between 60 and 120 times the solar gravitational force.

No two comets' tails are alike, and in that respect at least our laboratory comet tails closely followed nature; no two of our tubes gave the same result. But comets' tails are self-luminous, whereas our comets' tails were black particles.

There are several ways in which light pressure enters into physical problems. Boltzmann used it in deriving the second law of thermodynamics. It can be used in deriving the law of the adiabatic expansion of black body radiation $pv^{4/3} = \text{constant}$[1] and from this Wien's Law follows. It is used in deriving the change in wavelength in the Compton Effect—indeed, in every case in which the momentum of a photon is taken as hf/c.

Although we have indicated that the pressure of radiation on a particle of molecular size would be zero, and therefore we might expect that radiation would not exert any force in a vapor, still it is clear that if a mass of vapor absorbs a certain fraction of radiant energy passing through it, that same mass would absorb the same fraction of the momentum and would consequently experience a driving force.

Not only in regard to comets' tails but also concerning the expanding nebular layers enveloping a nova, the pressure of radiation enters. Innumerable papers have been written on these topics dur-

ing these fifty years. They are still going strong.

However, there is one result of the marvelous Maxwell theory that has been passed over. The statement that there is a pressure on an absorbing surface equal to the energy density in the radiant beam established the relation between mass and energy, $E = mc^2$. To make this conclusion convincing, let us consider a light beam one square centimeter in cross section. Let there be n particles or photons per cubic centimeter, each of mass m and energy E. The energy density is nE. The number of particles striking the surface per second is nc where c is the velocity of light. The momentum of each particle is mc. Hence the total momentum absorbed per second is nmc^2. But this equals nE. Hence $E = mc^2$. You may raise the objection that this applies only to the fictitious or quasi mass of a photon. But a photon has energy—it has mass. Moreover this energy and this mass are absorbed by the target. Its energy and mass have both been increased, but with the above relation holding. So for all mass and all energy the relation $E = mc^2$ holds.

Attempts have been made in recent years to verify the Maxwell-Bartoli formula for the pressure of electromagnetic waves. But here there are great difficulties. For example, K. Fritz[2] used energy of wavelengths from 112 to 295 cm directed against a resonant antenna of 1 mm diameter. What is the energy density in front of such an antenna? It receives energy from many directions. What is the vector sum of the forces?

Recently a similar experiment has been performed by Nello Carrara and P. Lombardini of Florence[3] using 3-cm waves. This was concerned with the pressure inside and just outside the mouth of a wave guide; and it was hoped that it would provide a method of determining the power propagated along the guide. An experiment completely verifying the Maxwell theory for electromagnetic waves still is lacking.

Before proceeding to discuss the other two scientific discoveries, I desire to pay a tribute to the memory of Ernest Fox Nichols. I think I cannot do this better than by quoting from an article I wrote, "Reminiscences of a Scientific Comradeship."[4] "Always there stands out the keen intuition of Dr. Nichols, his hatred of sham, his loyalty to friends. The four years spent with him were, for me, years of strenuous but congenial labor—years of a memorable scientific comradeship."

Identification of the Alpha Particle

In 1899 Rutherford divided the rays coming from radioactive materials into two classes, the penetrating and the easily absorbed. The nature of the penetrating rays was quickly solved. There were two kinds—those like x-rays and gamma rays and those of electron streams. But the easily absorbed rays, the alpha rays, what were they? Rutherford subjected them to the action of electric and magnetic fields. The results were submitted in his paper given at the Washington meeting. His conclusion was that they consist of particles which have the mass of a very small atom—either hydrogen molecules with a single charge or helium atoms doubly charged. He took the latter view. Now his experiments, which look so easy, were attended with the greatest difficulties, as may be seen from the fact that he was the only one in the world at that time, or for several years, to make those measurements. True, Henri Becquerel attempted similar measurements; but he came to the conclusion that the mass of the particle, whatever it was, constantly increased as it moved along its path. Hence no significance could be attached to it. I heard Becquerel at the international conference in Liége, Belgium, in 1905, make such a statement and claim that Rutherford's findings were in error. But one year later, four years after Rutherford made his measurements, Becquerel acknowledged before the French Academy that Rutherford was right and he was wrong. Thus the leading scientist of La Belle France bowed to the youth from primitive New Zealand.

It may be noted that Becquerel claimed that the apparent deviation of the alpha rays as found by Rutherford was due to the large deviation of the beta rays, which upon striking the walls of the enclosing container were reflected so as to give the appearance of a positive charge. Also Rutherford's conclusions regarding the nature of the emanations was challenged by M. Curie. But in both cases Rutherford proved that he was correct.

The new knowledge concerning alpha rays was fitted in at once to the extraordinary theory put forth at that time by Rutherford and Soddy,[5] the theory that radioactivity results in a succession of chemical changes in which new chemical atoms are formed by the ejection of an alpha or beta particle from the parent radioactive atom. They called it atomic disintegration, an unhappy phrase since it implied that the offspring was rather inferior to the parent. This point of view is frequently held by human beings. Fortunately it is not always true, else the human race would be in a more deplorable condition than it is at present. Rutherford and Soddy were perplexed as to the names that were to be given to the new atoms. But they boldly named the unknown offspring in the family trees

headed by uranium, thorium, and radium. In the last case, for example, they named five members, radium, radium emanation, radium excited activity I, ditto II, ditto III, and subsequent unknown particles.

Rutherford and Soddy[6] established one other fact of vast importance, a prophetic fact, namely, that the energy involved in a radioactive change must be at least 20,000 times, and may be several million times, as great as the energy of any chemical change. (Also they deduced "that the energy latent in the atom must be enormous compared with that in ordinary chemical change.") They foresaw the possibility of what we now call atomic energy and the atomic bomb. How many of the physicists who heard Rutherford's paper envisioned the vast changes which have come to the domain of physics—indeed, to the whole world—by disclosures made by Rutherford, and by Rutherford and Soddy, at that time?

Cosmic Rays

The third discovery announced at the meeting of fifty years ago was that of a new penetrating radiation in the atmosphere, the cosmic rays—or better, the celestial rays—of the present time. There were two papers regarding these, the first by E. Rutherford and H. L. Cooke, the second by J. C. McLennan and E. F. Burton. Although Rutherford and Cooke gave as their title "A Penetrating Radiation from the Earth's Surface," they proved conclusively that in the neighborhood of Montreal such radiation did not come from the earth's surface. By noting the rate of discharge of a well-insulated gold-leaf electroscope, it was seen that there was a very penetrating radiation present, first, in the physics laboratory of McGill University; then in the library, which was entirely free of radioactive substances; and finally, on the campus of the university. In the last case the electrometer was placed on the frozen earth at a distance from the laboratory. In all cases the rate of discharge decreased when metal screens or large screens of water were placed around the instrument. As much as five tons of pig lead, of 5 cm thickness, placed around the instrument when it was on the frozen ground resulted in a decrease of 30 per cent. When the screens were removed, the rate of discharge returned to its original value.*

* In *Time* magazine of January 19, 1953, there is an item released by the Atomic Energy Commission regarding the "hottest" radioactive source. It is 10 pounds of cobalt 60. Prepared in the Brookhaven Laboratory, it was sent to California in a 2-ton lead shield. But Rutherford and Cooke used 5 tons of lead in an attempt to shield their electroscope from the penetrating radiation which they had discovered.

Thus the authors proved that there was a very penetrating radiation in the atmosphere, that it did not come from radioactive substances or from the earth in the neighborhood, and that it came from all directions except from the earth.

Now Rutherford knew something about penetrating radiation. He was perhaps the foremost man in the world in researches on radioactive matter and the rays which such matter emits. He had worked with the penetrating radiation from thorium and radium, and he recognized this radiation in the atmosphere as being out of the ordinary. But his great interest at that time was in analyzing radioactive substances and in building up the radioactive series which illustrated the extraordinary theory that he and Soddy had just put forth. He did not follow up this first work on cosmic rays, but beyond question he was the first to call attention to this extraordinary phenomenon —one that for fifty years has been the subject of a vast number of experiments, that has held the attention of physicists all over the world, and that is the subject of articles set forth in millions of pages.

The paper by McLennan and Burton had as its title: "Some Experiments in the Electrical Conductivity of Atmospheric Air." Again the rate of discharge of a well-constructed electrometer was observed. The authors at first were of the opinion that the metallic wall of the electrometer was slightly radioactive, and they made several tests with electrometers made of different metals. Finally they immersed a large, heavy electrometer into a very large tank of water, and they found that the rate of discharge decreased by 37 per cent. For a smaller tank the decrease was 17.5 per cent. They then came to this clear conclusion: "From these results it is evident that the ordinary air of a room is traversed by an exceedingly penetrating radiation such as that which Rutherford has shown to be emitted by thorium and radium and the excited activity produced by thorium and radium."[7]

Work upon this penetrating radiation was carried on intensively at the University of Toronto. C. S. Wright[8] in 1907 arranged his apparatus so that it recorded continuously, day and night, the intensity of the radiation. Also, he made observations not only in the physics laboratory but also in several buildings of the university, at the top of towers open to the sky, on the ice of Toronto Bay, and on board a vessel moving back and forth across Lake Ontario. G. A. Cline[9] in 1908 repeated and extended these observations and came to the following conclusions: "It is possible that the penetrating radiation now present *at the surface of the*

earth has its origin in the sun or in other celestial bodies." You will notice that he did not say it came from the earth, but that it was being investigated at or near the surface of the earth. The Toronto name would be celestial rays, and I think that is a better name than cosmic. What do we know about the cosmos? The work of these Toronto investigators made it perfectly clear as early as 1909 that the earth was not the source of the radiation. The problem then was, how were observers to take their apparatus away from the earth?

By this time physicists in various countries were making measurements of this radiation. In Canada free ballooning was not an outdoor sport. In Switzerland and Austria it was. A. Gockel went up 4500 meters over Zurich. He found the same radiation, sometimes of more intensity, sometimes less. Observations now became more definite by the use of the Wulf electrometer, in which platinized quartz fibers replaced gold or aluminum leaves. It was used by V. F. Hess during several ascents. At first there was no clear indication of any increase in the ionization; but finally, in 1912, on the seventh ascent from Vienna, Hess found a very rapid increase for a change in height from 2000 to 5000 meters. In the following year Kolhörster went up to 9000 meters and also found a large increase in ionization. Then came Millikan, who in 1922 sent up his instruments ten miles. So it continued. And now we ask the question—who discovered cosmic rays? Was it those who read the papers before this Society fifty years ago, or was it this one or that one who added fact after fact to the original observations? Was it Gockel or Hess or Millikan who showed that the radiation was very strong at considerable heights above the earth's surface? Or was it Clay and Arthur Compton, who showed that this radiation was not entirely, nor even chiefly, of the nature of gamma rays, as had been thought and as Millikan strongly contended, but was of the nature of charged corpuscles, influenced by the earth's field? Or was it Carl Anderson, who discovered the positron, and very specially the meson, in the rays? Or was it Street and Stevenson of Harvard, who very definitely confirmed Anderson's discovery? Or was it C. F. Powell and his group at Bristol, who found only five years ago a whole family of mesons in these rays? All these may be regarded as discoverers, but all have been observing the same radiation. My opinion is that if any one man is to be credited with the discovery, that man was Rutherford; if any group, it was Rutherford and Cooke at McGill University and McLennan and Burton at the University of Toronto—and the papers announcing the discovery were read at the meeting of this Society fifty years ago.

What a fifty years it has been in physics! One hundred and ten years ago Alfred Tennyson wrote in Locksley Hall,

> "I dipt into the future, far as human eye could see,
> Saw the Vision of the world, and all the wonder that would be."

But he did not see—no poet, no physicist of even fifty years ago envisioned what we have actually seen—the incredible extension of our domain. The program of this meeting gives evidence of its vastness. What a joy it has been to have been working in this domain all these years. We are always at the dawn of a new day.

References

1. Hull, G. F. *Elementary Modern Physics.* New York: Macmillan, 423 (1949).
2. Fritz, K. *Ann. Physik,* **11,** 987 (1931).
3. Carrara, N., and Lombardini, P. *Nature,* **163,** 171 (1949).
4. Hull, G. F. *The American Physics Teacher,* **4,** 61 (1936).
5. Rutherford, E., and Soddy, F. *Phil. Mag.,* **4,** 370, 569 (1902).
6. *Ibid.,* **5,** 589, 590 (1903).
7. McLennan, J. C., and Burton, E. F. *Phys. Rev.,* **16,** 192 (1903).
8. Wright, C. S. *Trans. Roy. Soc. Can. III,* 85 (1908).
9. Cline, G. A. *Ibid.,* 111 (1909).

The Financial Threshold of Alaska*

ELMER E. RASMUSON

The author is a native Alaskan, born in Yakutat. He was educated at Harvard University, which granted him B.S. and M.A. degrees. He has been a member of the Anchorage City Council and City Planning Commission and a Director of the Chamber of Commerce. Formerly of the Anchorage accounting firm of Arthur Andersen and Co., he has been President of the Bank of Alaska since 1943.

WHEN I think of a threshold, I picture a traveler standing at a doorway with some hesitation, preparatory to entering a new environment. The incident is full of question. Will he stand there for some time, with anxiety mounting the while? Will he step backwards and not come in? If he does step in, will it be timidly or with a boldness that inspires confidence in all who watch? Alaska is in such position today. A threshold is an intermediate point, and it implies change. It is often a point of decision. But, to know whether the movement will be forward or backward, and at what period in time and at what rate, implies an appreciation of all the background leading up to the present.

To know where Alaska is going necessitates an understanding of why and where it is today. Let us start with geography. In location and climate Alaska is northern, and even arctic. The coastline is long, with mountains rising at the water's edge along most of the southern coast. This coastline is further sealed off from the interior by natural surface highways—the great rivers—which flow from the north and from the west.

Small wonder then that our economy has been built primarily in the coastal area. Only one city in Alaska with a population in excess of 1000 is not on the coast. A natural consequence is an extreme seasonality of all endeavor which has been a troublesome factor in practically everything in Alaska. In these factors and the size of the country —one-fifth that of the United States—lies the explanation of the diversity of business and production. It is thus easy to understand why we have had a philosophy of impermanence. This philosophy has also produced an insulated outlook in our population. Many residents have felt closer to the states they came from, vacation in, and expect to return to. The initial registration this year at the University of Alaska did not include one student from several of our southeastern cities. Small wonder that a fisherman, for example, is not inclined to invest savings in Alaskan mining or agriculture. This situation tends to aggravate the deficiency of capital in Alaska.

As a result, Alaska has a pioneer or frontier economy. The population has been predominantly male. There has been a strong emphasis on exploitation. This is equally true of the current construction industry and of those dependent on natural resources.

We have a concentration on production and a relative indifference to commerce and trade. This, of course, parallels the history of any similar developing country. The political and social significance is obvious.

In finance, we have a high gross return on capital, the result of scarcity and risk. Since capital opportunities in Alaska must compete with all the states and indeed the world, Alaska is relatively unattractive to either outside or home capital.

In marketing, we have even today a trading-post philosophy of high markup which is satisfied with low turnover. That means few real sales and clearance of goods, and emphasis on retail merchandising rather than wholesaling.

Some of the financial consequences may be briefly listed. Our demand for capital far outstrips our own capacity to supply. Although short-term bank money is available through participation of Alaskan and stateside banks, there is no real investment capital as in the older population centers. There are no insurance companies lending in Alaska, except on a government-guaranteed basis, and little stock brokerage.

* Based on an address presented at a general session of the Third Alaskan Science Conference, Mt. McKinley National Park, September 22–27, 1952, sponsored by the Alaska Division of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The high-cost economy of Alaska thins the dollar even more than in the States and circumscribes operations in the Territory. The same capital can leave Alaska and go to the States and with lower costs accomplish a successful unit. A notable example of an industry starved for want of capital is Alaskan agriculture.

The tourist industry is limited by high seasonableness in Alaska and lack of boat and hotel space in the summer—the best tourist time. Alaskan banks have their own special impact of seasonality. Bank deposits increase in the fall, when merchants and other producers pay off their loans. In the spring, when deposits are down, comes the demand for seasonal loans; these funds are drained off in payments to the States. Incidentally, this problem is intensified by concern over the financial consequences that would come with evacuation under wartime orders. Because of the absence of sufficient accounting and auditing services in Alaska, there is a tendency in Alaska banking to lend more on real estate security than on financial statements.

Another evidence of our intermediate development is the relative scarcity of corporations as a form of business organization. Alaska is still primarily a land of proprietors and partnerships, although this is changing.

Our short biennial Territorial legislative sessions have not been able to keep pace with Alaskan or outside development. In consequence, many statutes which business elsewhere has come to regard as indispensable are lacking in Alaska. Furthermore, the emphasis on government activity in Alaska during and since the war has produced a negative attitude of legislators toward business. This is not a hostile attitude, but rather an unconscious indifference, because the daily livelihood of such a substantial proportion of the Alaskan population does not at present depend upon private business. In the competition for capital this has an adverse effect on prospective investors.

What are the possibilities for the future development of Alaska? The first is that Alaska may stand still. This does not seem a reasonable likelihood because rarely does a country stagnate, though change is often temporarily masked by a consolidation of position. There is too much inherent instability resulting from the pushing forward of special forces. For example, the expansion in transportation and military defense alone in recent years in Alaska produces a pressure on other economic forces that cannot be denied. Therefore, the conclusion is that Alaska must move.

Will it move backward? For this possibility to become a reality, many private and public investments would have to be abandoned; such an event would be strongly resisted by the rigidity of modern economic forces. We pride ourselves on economic flexibility, but actually the present and growing tendency is the opposite.

Many people ask about the importance of military construction as a prop to Alaskan economy, and about what is going to happen to Alaska when this construction boom ends. Unfortunately we do not have a quantitative analysis, but surely its significance is greatly exaggerated. Only a small percentage of these gigantic appropriations ever reaches Alaskan merchants: half of the appropriations is spent to purchase materials outside the Territory; from the balance must be deducted the cost of employing labor in isolated places, the cost of construction of military camps, and the funds sent to the States by the transient construction crews. This does not mean that construction is not important. It does mean that military construction, as opposed to military operation and maintenance, is not covering an otherwise unbridgeable gap, and, dollar for dollar, it is not to be compared, say, with the value of farm produce grown in Alaska.

Incidentally, as an example of the extent of seasonableness and impermanence in the present Alaskan economy in a military construction area, here are some statistics from The National Bank of Alaska. Over 50 per cent of closed-out accounts were in existence less than 30 days, and over 80 per cent were in existence less than 90 days.

Actually it is much more difficult to go backward than forward. A retrogression in Alaska would mean a sharp reversal in non-economic motives of national defense and development philosophy for the Territory. The prophets of depression have much the weaker case, and they argue for a movement which is contrary to trend. However, there are more positive indications that Alaska's step across the threshold will be forward and onward.

Traditionally, economic demand for goods and services has been in terms of a price. Even if Alaskan costs are not reduced, an increase in total demand, such as an increase in Alaskan population, will stimulate economic development. This increased demand should result from military or other movements of population to the Territory for non-economic reasons, and from birth increases provided that they are not lost by emigration to the States. Foreign immigration would be a population stimulant if national policy were modified to permit it. A depression in the States would have the same effect.

This increase in demand can arise particularly with respect to non-competitive goods or services.

The University of Alaska campus at Fairbanks (U. S. Bureau of Reclamation).

Examples are the tourist trade attracted to non-exportable scenery and industrial demand for special products of the forests, mines, or sea. The field is tremendously expanded when the possibilities of new products such as oil and plastics are considered.

However, it is in the area of cost reduction that the possibilities of increased demand in Alaska are the most intriguing. The most assured development with far-reaching consequences is that of hydro-electric power. This is an inexhaustible economic force, and through it Alaska, having a tremendous cost advantage, will break the vicious bonds of its high-price import economy. This resource can attract an entirely new industry and involve an economic revolution; in fact, the Aluminum Company of America has already announced plans for its Taiya project. In addition to new industry, cheap power will stimulate many mining and forestry operations now marginal in the national market.

Improvement in transportation is another field of cost reduction, in both time and ton-mile costs. We have already seen it in the field of air transportation; it is coming on land with road extension and the hard-surfacing program, and railroad improvements. It is to be hoped that the problems of ocean transportation can be solved to permit expansion of movement by the sea.

Of special significance to Alaska is any development which produces a back-haul. This is doubly effective because the exported product not only yields production benefits but also tends to reduce transportation costs further by permitting round-trip costs to be assessed against two cargoes. One of the reasons why cessation of military construction will not provoke a recession is that automatic offsets arise from the labor and transportation thereby made available. The necessity for overtime will be reduced, and competitive labor demands, as in the field of agriculture, can be thereby satisfied. The stimulation of agriculture will in time tend to reduce living costs and bring Alaska more into line with the States. Additional storage facilities will extend the marketing season and farm incomes. The extension of local feed production has been effective in reducing livestock and milk costs.

With increase in population come improved efficiency, a larger and more trained labor force, and more skilled management. The population tends to become more satisfied and stabilized, and transients become permanent residents. This re-

duction in turnover is reflected immediately in lower operating and construction costs.

If, in addition, the improvements that can logically be expected to come through technological invention are considered, it seems reasonable to conclude that Alaska's costs and prices will improve in relation to those of the States.

Economic demands are usually thought of in terms of price. However, more and more demands are becoming non-monetary in origin and are not equated with supply through price. The importance of this phenomenon increases as the importance of government is expanded. The best example is national defense. A strategic location like Alaska's cannot be measured in terms of money. Is it not unreasonably narrow, then, to regard military spending in Alaska as a temporary construction problem? In the first place, obsolescence proceeds at such a fierce rate that a continuous construction program of some degree seems assured. In the second place, maintenance and garrison of the facilities are a necessity for the foreseeable future, and this provides much greater and longer-term economic stimulation than construction.

Another stimulating pressure is the national obligation of the United States to develop all parts of our country including our Territories. It might be called the conscience of our nation. Although it does not have a monetary origin, it is a real and dynamic force in our economy.

As international complications strain our national economy, production is rechanneled and price is limited as an effective demand factor. The most extreme minimization of price comes under rationing, for then allocations rather than price determine the demand. For the producer hard-to-get materials are stimulated, and for the consumer travel, tourism, and recreation are favored. Alaska has had substantial stimulation from these causes, and, although they may not be continuous in the future, they can be expected to materialize forcefully and periodically.

Economic development has many psychological barriers. Alaska's greatest obstacle is its lack of political equality. With statehood, Alaska would be one with the States in the minds of countless prospective settlers and investors. This would make population and capital shifts to Alaska more inviting. In addition, the expansion of various government services would increase the demand for specialized labor skills. And, in turn, expanding population from any source stimulates demands for additional goods and services, more public improvements, more varied living, and so forth.

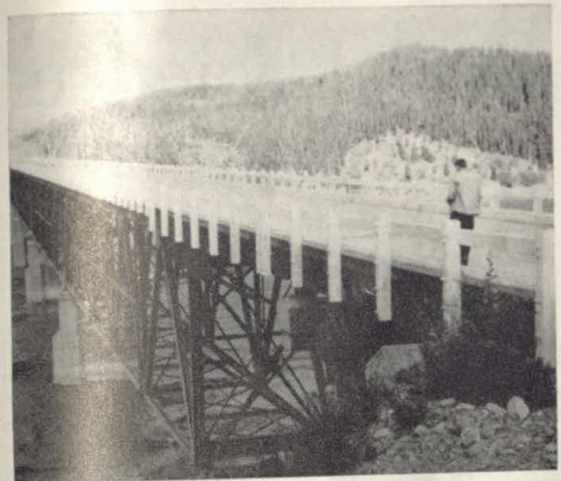
It would be most difficult to predict the time and the rate of this anticipated development. Undoubtedly a sharp focal point would be an event such as statehood or the aluminum development.

This discussion would not be complete without some anticipation of Alaska's financial position after the threshold has been crossed. A common presumption is that Alaska would be like the western states, except for differences in climate and geography. But Alaska will probably be different because of the national and world changes that will have taken place by the time Alaska comes into its own. It is exciting to realize that we can shape its destiny if we are conscious of our power, make scientific use of our collective knowledge, and are sincerely interested in the results. Our political future is what we make it. For example, we need not start out with a predetermined county organization. We can have a fresh approach to the suburban problem. With good transportation and available land, there will undoubtedly be a lack of concentration within the legal limits of cities. For example, at present, two-thirds of the population of the Anchorage area resides outside the incorporated city limits.

Social welfare will surely receive great emphasis in our future expansion. As a result a greater percentage of our income will be spent through public channels, and substantial tax rates will probably be required. Because of the size and diversity of Alaska, and the subdivision according to judicial divisions, there will probably be a tendency to channel funds through the territorial, or state, government, in order that the more needy areas can be helped through contribution by the more developed portions.

Our development will at first be based on natural resources. There will be continuous emphasis on cost reduction, because Alaska starts off with a high cost economy. This is one of the most important differences between conditions in Alaska and the usual situation encountered in developing a new area. Our difficulties will be comparable, say, to Venezuela, which is endeavoring to stimulate agriculture and manufacturing in an economy of high prices caused by almost a total reliance on oil exports.

High costs tend to attract the introduction of machinery to save labor. This in turn emphasizes the need for additional capital, which must come from outside Alaska; such capital results in nonresident ownership of our industry. The pace of our development will to a marked degree be limited by our ability to attract and hold capable management.



Robertson River Bridge on the Alaska Highway, a link in the road system opening up Alaska (U. S. Bureau of Reclamation).

If our Alaskan development is forthcoming and if it needs substantial amounts of capital, it is inevitable that government will play an important source role. In the field of transportation, the Federal government operates the Alaskan Railroad, and the Territorial government the Juneau-Haines ferry. In agriculture, the Alaskan Rural Rehabilitation Corporation handles the sale of most of our agricultural land and our federal agencies finance the improvements and production of crops.

Public power is developing the Eklutna project near Anchorage, and undoubtedly it will proceed with other hydroelectric developments. To stimulate housing construction in Alaska a special Congressional act was passed with both government guarantee and loans. The evidence indicates that under government guarantee the lending in the future will gradually be taken over by private banks.

Banks have always played a versatile role in Alaska and should continue to do so. Probably a secondary market will be needed for long-term investments such as deferred RFC participations until insurance money is available for conventional loans.

As Alaska grows, there will probably be one or more local insurance companies. With the transition to the corporate form of organization, a local stock exchange will eventually arise. As wealth accumulates and the population increases, both corporate and individual trusts will come into common use. Foreign transportation lines, with direct Oriental trade and transpolar routes, will operate to Alaska. Banking will then adjust to direct clearance of foreign items.

The possibilities are endless, and no one can foresee with complete accuracy. However, two conclusions are evident. Alaska will cross the threshold with a forward step—the route and pace are within our control. That is the fascination of Alaska. We are a young country entering into a mature society, and we can help write history instead of merely reading about it.

Where Are the Social Sciences in Alaska?*

MARGARET LANTIS

The author is a graduate of the University of Minnesota and received her Ph.D. in anthropology from the University of California. Her interest in culture studice in Alaska, and also has taught in Aemrican universities. Dr. Lantis has been shown by the variety of her work. She was a teacher for the U.S. Indian Service in Alaska; and also has taught in American universities. Dr. Lantis has been a social analyst for the War Relocation Authority, a social science analyst for the U.S. Department of Agriculture, and a member of the staff for the Study of Lifetime Social Adjustments, at Harvard. At present she is with the Arctic-Desert-Tropic Information Center, Maxwell Air Force Base, Alabama.

SCIENCE CAN BE a way of life, a basic attitude toward the world. Before one can understand the scientist's goals and problems, one must have some understanding of the scientist himself. What is his value-system? What are his functions in society? Since science is not characterized by the things it studies—by its content—but by the way it studies them, the student of social phenomena can be a scientist too, if he lives by the rules. So we can be excused for talking about ourselves for a moment, not as geophysicists or ichthyologists or sociologists but as generic scientists.

The scientist is, first, a person who lives for and in the future. The true scientist is a dreamer, with discipline. His objective is to state generalizations, principles, propositions, tendencies—whatever you want to call them—laws that are bases for prediction. The scientist always must stand the test of prediction. Thus, even though a paleontologist or archeologist is concerned with reconstruction of the past and does not expect to see trilobites or Neanderthal man on earth again, he uses history to formulate principles of anatomic change or cultural change. In other fields the use of prediction is even more obvious and more immediate.

But philosophers and religionists state principles, and everyone predicts. Any person functioning as a scientist is different, however, in: his use of exact methods, his willingness to reveal these methods to others, and his integrity and impersonality in stating his observations. The sin of the scientist is to conceal or to distort. Hence he is (or should be) always conscious that he is standing the test of the future. For this reason we say that he is not merely a dreamer, but a dreamer who has self-discipline. (The man who goes through exact routines and does no more is a technologist, not a scientist.) The scientist constantly seeks new things: new ideas, new methods. He can outdream the advertising and public relations men, but he can only envy—he dare not imitate—the advertising man in his claims. He disciplines himself in formulating exact plans for research. He disciplines himself to be a meticulous observer and recorder, to make careful reports and cautious claims, and to accept criticism when he really wants to call his critic an old fool.

The scientist differs from the layman in another way: he does not only try to see *what* will happen, he also tries to understand *why* it will happen. Hence we further characterize scientists as people with terrific curiosity, always asking "Why?" Members of this Conference know that, as the scientist learns more about the why, the more accurate he becomes in predicting. In Alaska prediction may be especially frustrating and difficult because of extinction of our subjects: owing to rapid changes on this frontier, peoples and animals disappear while we are awaiting for some of our predictions to be proved true or false. But the process is essentially the same in all science.

It seems to me that there are two kinds of work in science; and, to the extent that a person prefers one and uses one more than the other, he is either a watcher or a tinkerer (maybe we can add a third: the fixer); in other words, the natural historian or the experimenter. Both are necessary, and

* Based on an address presented at a general session, September 23, of the Third Alaskan Science Conference, Mt. McKinley National Park, September 22–27, 1952, sponsored by the Alaska Division of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

both are present throughout the history of any one science. However, in relative numbers, generally the watchers have preceded the tinkerers. They had to look and see what was there before they started working on it. Let us consider biology here in Alaska for an example. Edward William Nelson, whose first big field study was made in the Lower Yukon region 75 years ago (1877–81) and who was Chief of the Biological Survey from 1916 to 1927, contributed to science by his observations and collections in areas never before explored by zoologists. Nelson, Ray, Stoney, and the other men of their period in Alaska did not need many hypotheses and theories. Just to go out and collect was important and sufficient 75 or 50 years ago.

Then the experimental biologist stepped forward. In Alaska during the past fifty years the Biological Survey, the Reindeer Service, and other groups have made field experiments, necessarily uncontrolled in most cases, hence with more hope than prediction. They were very useful, though. Finally came the pathologist. But not until 1948 was the Arctic Health Research Center established. Its work leads us to the real objective of science from a humanistic standpoint: treatment and prevention.

Along another line of development we come to another new institution in Alaska, the Fishery Products Laboratory. We must not forget the Agricultural Experiment Station; but laboratory experimentation in highly specialized fields of zoology and physiology, for example as carried on at the Arctic Research Laboratory at Point Barrow, is recent in Alaska. In these new programs, as in the Coal Analysis Laboratory and the Geophysical Institute at the University of Alaska, the scientists are as usual looking ahead, in application as well as in formulation of theory. In any field we must watch the trends, we must see ahead, not trail after.

Although this is true whether we are studying structure or behavior, it is especially true when we study behavior. Are bogs advancing at the expense of forest, contrary to expectation? At what rate is the Magnetic Pole moving? Are the caribou changing their range? Are the Southeast Alaskans successfully combating tuberculosis while the Central Alaskan Indians are not? Ten years or fifty years from now, one may say, "Well, the caribou used to be here but they aren't anymore." That is, ten or fifty years hence one can say "what" happened but will have difficulty answering the all-important questions "why?" and "how?" It is better to *observe* the rate than to *reconstruct* the rate.

The modern scientist is just the opposite of the popular stereotype, that mentally remote character mounting butterflies and moths on pins and completely unaware that the caterpillars are destroying the orchard. Even if he is not an economic entomologist, the modern scientist, because he is so interested in processes, in the dynamics of life, must be and is a Johnny-on-the-Spot, right there watching the processes as they occur. Consider, for example, the glaciologist who today is probing around in the innards of the glacier trying to find out how long it takes snow to get from the topside to the bottomside and what happens to the crystals on the way down. (May I be forgiven for my phrasing, not being a glaciologist.) People no longer talk about glaciers as just ponderous masses. Laymen would be surprised to hear what glaciologists are doing with their thermistors and other gadgets, and often, of course, they would be dubious about the work if they did know. (This is true of the detailed work of any science.) Glaciologists, however, have it easy. They just talk to other glaciologists or perhaps some physicists and meteorologists. They do not have to ask permission of Old Man Taku (the glacier) to dig around under his skin, as a social scientist would have to do with his subjects.

At last we have mentioned social science. Going from the generic scientist to the specific, let us see what breed is the sociologist. Some people may consider him the Sad Sack among scientists, but let us be charitable and include him in this group of forward-looking men just described. And, let us be sympathetic. Sociology has suffered from both internal and external difficulties. Internally there was too much and too early *emphasis on social pathology*. The curriculum was full of courses labeled Criminology, Social Disorganization, and Problems of Social Welfare. Ecology seemed to become only a study of blighted areas of cities. Lectures on "The Family" dealt chiefly with family disintegration and divorce. Someone has said that among early sociologists in America there were too many ministers' sons who were just trying to talk about sin in scientific terms. All this robbers-and-cops stuff antagonized many laymen, especially civic leaders. The sociologist and several of his fellow social scientists always seemed to show up the worst in the community. Also, just by trying to be disciplined scientists, they often got in bad with the boosters.

Lewis Mumford, the student of city planning and architecture, has said, "It is better to face chaos courageously than to cherish the dream of returning to an outworn synthesis." But of course most people do cling to the old synthesis.

The psychologist has had the same difficulty whenever he got beyond a study of special abilities. In the study of the dynamics of the personality, there was more abnormal than normal psychology; and the layman felt uncomfortable and suspicious. Instead of being reminded how remarkably subtle and clever, yet consistent and strong, is the individual personality, he was made to feel that he was full of irreconcilable conflicts and about to go off his rocker. Fortunately, in sociology, social anthropology, and social psychology the early stage of discovering all the awful things that are wrong with man—his logic-tight compartments, sibling rivalry and Oedipus complex, racial prejudices, and culture lag—has been passed. In psychology now we are hearing about ego strength and ego ideals; in anthropology about the cultural values, that is, the commonly shared beliefs and attitudes that people live by; and in sociology about the processes of achieving consensus or agreement. These topics are not only positive, they are dynamic. The psychologist, for example, wants to know how a youth builds his personal model for conduct. Or, in fancier language, how does he integrate an ego ideal? And so on through a cheerful list of questions.

There are, however, still some difficulties for the development of sociology in Alaska or elsewhere. First, it is very hard to experiment in the field of human relations. We still are in the natural-history stage. At any rate, those of us who make field studies as well as doing armchair theorizing about people and politics are the natural historians of man, trudging up and down the hills of society. Yet we are no longer merely the historians of man, because we now have a much better understanding of scientific problems and of the formulation of hypotheses; and we have more sense in the use of special tools for field study, for example, opinion polls. Although I have not had contact with enough of the other disciplines to be sure of this, it does seem to me that in many fields there is a renascence of good old field observation, a refined natural history done with remarkable new tools. Consider the studies of the stratosphere, of communication among bees and other insects, of the social system of many mammals, for a few examples.

At the annual meeting of the American Psychological Association in Washington, D. C., in September, 1952, Dr. J. M. Hunt in his presidential address urged psychologists to get out of their laboratories and go to "the bank of society" to borrow greater experience and knowledge. He also urged greater collaboration with other professions.

Some of the social scientists also are seeing the value of a well-rounded natural history, not because they went too rapidly and exclusively into experimentation but because they tried prematurely to formulate rigid laws. Economists, for example, have found that economic man is at the same time social man and political man. As Joseph Spigelman pointed out in an article in *Harpers Magazine* ("Can Science Make Sense?" May 1951), the economic system is not autonomous. All sorts of people—businessmen, union leaders, and government administrators—are not letting either prices or wages move according to "economic laws." They want to plan them.

Now we are led to a second difficulty (or a supposed obstacle) in social science: the complexity of human behavior. Actually it is not so complex when compared, for example, with the chemistry of the human body. The biggest difficulty is simply the lack of data, the very small number of scientific observers for such a very big subject. In the United States, for the 600,000 patients in public mental hospitals and for the general population, there are about 7500 psychiatrists, few of whom have time for research on mental health. The Director of the National Mental Health Institute says, "Our minimum need is 15,000."

In my own chief professional society, the American Anthropological Association, there are about 1400 members (not including institutional subscribers), but more than half of these are only subscribers to the journal. Only 600 are Fellows of the Association, that is, professionally trained, professionally functioning anthropologists. In contrast, the current membership of the American Chemical Society, excluding student affiliates, is 67,500. Some of these probably are merely subscribers to *Chemical and Engineering News*; yet, if only one-half are trained active chemists, there are still 33,750. In 1950 a chemist at Merck's plant at Rahway, New Jersey, told me that 90 Ph.D.'s in chemistry were employed there, in addition to all the Masters and Bachelors of Science. As Governor Gruening said, private industry will develop research it is interested in.

On this question of the supposed complexity of man's social behavior, I commend to you George A. Lundberg's article, "Alleged Obstacles to Social Science," in THE SCIENTIFIC MONTHLY, May 1950. Still, it *is* hard at this stage to predict man's social behavior accurately. Nothing so convinces the layman of the value of a science as a few dramatically accurate predictions. Since people in these confusing times need dependable authorities, nothing is

so disconcerting to the layman as the scientist's habit of hedging his predictions. When the responsible social scientist hedges—as he feels he must at this early stage—and talks about variable factors and unknown parameters, the layman decides that he really doesn't know what he is talking about but doesn't want to admit it.

The alternative, which has occurred, is that the political scientist, sociologist, or other social scientist has tried to predict when he did not know enough. He is pressed to do this. Consider how hard it has been to teach patience to the public regarding new therapy of common diseases. The public is slowly learning the caginess of the medical research man and the physician regarding new cures. John Q. Public, however, is impatient for both medical and social wonder drugs. He may feel the kind of frustration an Australian feels when he gets a new boomerang and tries to throw the old one away.

It must be admitted that physical scientists often are as bad as laymen regarding acceptance of new therapeutic methods in the field of human behavior without demanding cautious research and trial. I happened to be traveling about the United States when L. Ron Hubbard's system of treatment for personality ailments was at its height. What did I find? All across the country, many aeronautical engineers, electronics engineers, theoretical physicists, and others in the physical sciences were enthusiastic readers of *Dianetics*. Hubbard had presented a neat system, a special terminology, and other trappings of a scientific theory and a treatment based on it. The system was—shall we say?—premature. We hope no one will expect the social scientist or the psychologist to do what he himself will not do in his own field.

Finally, one big difficulty for the sociologist is definitely not of his own making. It is the sacredness of society to the American. We have progressed so far in medical research that one can divide the children of Houston into two groups, giving half of them gamma-globulin and the other half a placebo; but who would suggest sending half the Houston children to school together regardless of race and segregating the other half, as a social experiment?

Of course, all peoples have similar fears of changing certain parts of their institutions, although the identity of the sacred segments varies. American culture is remarkable in that we have extended scientific methods to one aspect of life after another, much more broadly than most people. Now Americans are taking their stand on the structure

A baby fur seal at the now flourishing rookery on St. Paul Island (U. S. Bureau of Reclamation).

of their social institutions. However, just as public misunderstanding, even hostility, to open discussion of pregnancy and of urinogenital diseases has been overcome, so public consternation at analysis and open talk of social change is being overcome, albeit with bitter skirmishes.

Alaska is much more fortunate than other territorial dependent areas, more fortunate even than the United States in several respects that are interesting to us here. Just when Alaska first feels its need for social studies and first becomes aware of its lack, it happens to be *at the right stage in the development of science* so that it can receive and use the social sciences, and they can do the necessary job. As the president of the Alaska Division (1952), Dr. Laurence Irving, has said, "Location and timing of the events observed can magnify or reduce the value of observations in bringing about the progress of knowledge and thinking."

Where does Alaska fit in the trend of science just reviewed? From atomic physics to social anthropology, *scientists are now interested in processes, in dynamics, in behavior*. No area offers a better opportunity to study social dynamics of a city virtually from the beginning than Anchorage, with its population growing so fast that it is almost a demographic explosion. Social organizations are multiplying all over Alaska. The Territory needs, and should be especially attractive to, people working in basic social science, studying processes of the

formation of a new society. Nowhere do we have enough of basic science in the field of social relations.

There is a *new appreciation of the natural historian, with his interest in the whole habitat,* the ecological community, the "field" and its relationships, which he now studies with improved techniques. This is an aspect of the modern scientist's rejection of autonomous systems, which is happening in all sciences—from astronomy and mechanics to neurology and economics. Today we may be floundering a bit in our study of interrelationships in a total field, but we probably are nearing a new organization of knowledge.

Alaskan communities, although growing, are not yet too large or too suburban to be studied very profitably as functional entities. Whether the communities and clubs and customs are just starting or are dying (as some Alaskan villages are), the interrelationships throughout the Territory and between it and the States can be studied. For one thing, migration into and out of the Territory can be ascertained more exactly than in any single state. We have, then, a manageable field for study.

Every real scientist accepts the necessity of prediction. How Alaska needs scientific prediction! If it is not to become a neglected social and political jungle, it needs not only the field observer, not only the basic scientist phrasing concepts and hypotheses; it needs also the man who will apply the generalizations to specific cases. Once the schools and other institutions are built in the wrong places for a changing population, once an agency unequipped to perform a certain duty is given that duty, both employees and users of the institution always suffer when a change must be made because there was poor planning or no planning. Everyone suffers from the malfunctioning of social institutions, and some people suffer from the change that must be made. The social scientist may be unwilling to commit himself on a prediction or he may make a mistake when he does commit himself; nevertheless we need him so much that we can risk giving him a chance at the job.

It is not merely because Alaska is growing that we must train and encourage social scientists for it. One good reason is that Alaska has, so far as I can discover, no sociologists—at any rate, none free to do research—and very few other social scientists. The Alaska Division of the AAAS has to ask an anthropologist to talk about and speak for the sociologists. The University of Alaska, whose president is aware of the need and wants to do something about it, has not yet been able to offer courses in sociology except two given rather apologetically by an anthropologist. At a conference on Alaska's resources, held in the Department of the Interior just a few years ago, the original program included everything from clams to hydroelectric potential, except of course its people. (Fortunately, one man in the Department, seeing this omission, insisted on talking about native Alaskans as an important resource.) The founders of the Arctic Intstitute of North America, the only sizable and international foundation devoted to research on the American Arctic, originally proposed to sponsor work in the social sciences as well as physical and biological sciences, but one founder objected and the social sciences were deleted from its charter. Increasingly, however, the Institute has supported anthropological research that is very close to sociology.

The Geological Survey had more than 40 field parties working in Alaska during the summer of 1952. The Fish and Wildlife Service had about 150 people in the field. And there are related Territorial agencies. In contrast, there is no research agency (excepting the specific field of public health) comparable with the Geological Survey to study the most important animal of all, man himself. The Geological Survey is studying the processes of solifluction and the boundaries of permafrost. No one is studying the vigorously contending processes of competition and cooperation in these new communities. No one is studying the shifting boundaries between private development of a new area and governmental development. Because of the recently expanded work of geologists and engineers in the North, the engineers can build better on permafrost. In contrast, no one is learning how to handle the processes of competition and cooperation so that there will not be a frost heave in the community every summer when migratory workers—the miners or cannery workers or construction workers—come in.

I do not want to make the geologists and zoologists feel guilty. They have no reason to feel guilty, but they should feel lucky. A scientific discipline's usefulness depends not only on its ability to do the necessary job but also on its being given the opportunity to work. The geologists have been given, or have fought for and won, that opportunity. Now the social scientists need their chance.

Social research is not completely lacking in Alaska. With the generous help of Alaskans I have compiled a list of fourteen projects (1948–52). It is noticeable that economists and anthropologists are doing the most in social research here. Five of

the fourteen studies are wholly or largely economic. Five have been made by anthropologists. The remaining four are divided among the fields of public administration, sociology, and mental health research. Although Alaska has housing specialists, trained social workers, and a specialist in vocational rehabilitation, only one professional sociologist (so far as I could ascertain) has worked here —on a summer's field trip. Social scientists must take part of the blame for their absence from Alaska: they should see what an opportunity it offers! And part of the blame rests with Alaska.

Every frontier region has to reach a certain stage of social organization before it can support professional specialization. Each of the American frontiers in succession seems to have gone through similar stages. To illustrate where Alaska now stands in this development sequence, let us paraphrase the terminology of our friends, the American archeologists, especially those who study the cultures of the Southwest. Using styles of pottery to designate the stages within each culture period and area, they have given the periods names like Black-on-red I, II, and III or Red-on-buff I, II, III, and IV.

For Alaska let us take music instead of pottery. As a somewhat undisciplined dreamer with one eye shut, I am going to call the first period Kobuk Maiden I to XII. To cheechakos we must explain that "The Kobuk Maiden" is an Alaskan ballad, of which there must be at least twelve versions. In frontier anarchy, when cultures meet and races meet, human association is spontaneous. The community does not sponsor any national chess tournaments. People dance, sing, play cards, or pray with different participants every week and usually with any participants who want to join in. There are only a few very basic organizations, and they also apply to or are open to all: enough town government to try to keep law and order, material services such as general store or the garage and later the water district or the toll road, and some religious services. Men band together to form a posse or a search party for a downed plane, to bring in a wagon-train of supplies or a plane with penicillin. They talk about independence and self-reliance out there on the frontier, but they join together very freely. And they sing doleful ballads of their difficulties, also spontaneously and anonymously. Fortunately, to most people "The Kobuk Maiden" is anonymous.

Then comes the second period: John Philip Sousa I to ?. People then talk much more about their common interests, while they are busily setting up organizations for their particular interests. Parents' groups, the Grange or Farm Bureau, the singing society (possibly even called a Choral Group), the whist or bridge club, and especially the businessmen's organizations. Probably the first professional or quasi-professional group is the Historical Society. People, seeing the rapid changes on a frontier, become aware of history and of their individual place in the history of their particular community. So they enjoy pageants and memorial ceremonies; they join the town band and play John Philip Sousa.

Finally, there is the Bach and Boogie Period, which Alaska is just entering. The bawling balladers have now become the Madrigal Singers or perhaps a Chamber Music Group. There will be later a variant: the Chamber Music Society of Lower Basin Street. In this period there is less talk of common interests, although there are more and larger organizations. It is recognized seriously that everyone has special interests. There are flying clubs, and the Culinary Workers' Union, and neighborhood associations, and the Society of Dental Technicians. As a local example, this year (1952) the Alaska Nurses' Association was organized and held its first annual convention. Alaska needs all kinds of specialists and associations of specialists and is gradually getting them.

Alaska offers as wonderful opportunities to the sociologist or social psychologist as to the archeologist, who here finds ancient cultures beautifully kept on ice. Alaska contains nearly all the stages of modern American culture, not on ice but—better still—decidedly viable and excitable. Some of its people have not forgotten, nor yet got to the stage of "reviving," The Kobuk Maiden while other Alaskans are singing the great religious chorales. The Territory seems to have very little dead wood in its society and fewer dominating vested interests than it formerly had.

We have seen that the social sciences are at the right stage of development to make a constructive study of an area like this. Now we can add that Alaska is at the right stage of general cultural development to accept professional social study of itself. We must not deceive ourselves into believing that on the one hand there will be no public opposition to social science in Alaska or on the other hand that there will not be inadequate work by the social scientists. I am hopeful, however.

At Alaska's present stage of readiness, perhaps it only wants to know how big and great it is becoming. Perhaps it only wants to know how many robbers it has, in order to decide how many cops to ask for. However, as one federal administrator

pointed out to me, there have been too many studies of this kind without anyone ever having enough power to get those policemen that the Territory knows it needs.

I agree that such counting of heads is not enough. Instead of merely surveying the social pathology of the Territory or of surveying anything, we want to study dynamic processes, just as the geologist and botanist study the forming of soil polygons in the tundra. Although Alaska is not the only new society and new economy that might be studied and even though many processes at work here may have been observed elsewhere, it does offer a fine new opportunity that should not be missed. Probably volcanologists did not make any stupendous discoveries from Paracutin, but some of them managed to get to that cornfield where a little volcano was sprouting.

I probably have talked too much in generalizations. Although we do not have time here to outline specific research projects for Alaska, let us bring this discussion closer to earth at the close. What *should* we study here in Alaska? I can give one example most clearly by telling what I think is the socio-economic trend of Alaska's development, hence what we must know in order to accommodate that trend, to adjust to it.

Agriculture will increase in a few parts of Alaska, but here, as in Norway and Sweden, mines, manufactures, and fisheries will support a growing population and economy far better than agriculture. We cannot ignore Alaska's topography. A non-agricultural region can be economically useful in three ways: production of raw materials (minerals, oil, timber); processing and manufacturing; provision of services, including trade. Alaskan economy can be stabilized by varied manufacturing and processing, especially processing. People forget that the basis of Southeast Alaskan economy for 75 years has been a processing industry, seafood canning. For the Territory as a whole, the economy has been based almost exclusively on exploitation of natural resources: fur-bearing and oil-bearing animals, fish, minerals, and, to a small extent, timber. With the exception of fish, virtually all products were shipped out unprocessed.

Now that the Alaskan economy is getting its new start by means of a construction boom and will have many construction needs for a long time, the first requirements are for local processing plants to provide construction materials (cement, lumber, aluminum) as well as such plants as pulp mills to process materials for export; power to run the plants; local skilled labor; the service trades. Getting out *raw* materials with modern technology means few men and a lot of heavy equipment. Processing plants and especially the service facilities, on the other hand, require workers. A particular type of economy facilitates or even requires a particular type of society and perhaps of political organiation. We do not know nearly enough about the relations between economy, society, and politics; and Alaska is just the place to study them.

In the present condition of Alaska and in the economy that it seems to be getting, we must study three dynamic relationships: the people and their material resources; the people who are coming here and the new socio-economic system they are building; the people and their Territorial political system, a survival from another era. Together they would provide an example of ecology in its largest sense and an exceptional opportunity for the study of process: migration, diffusion of culture, selection, adaptation, inner adjustment, invention. *The equipment that new settlers and native residents bring to the new society are as important as the country that receives it,* especially the ideas that individuals live by: their unstated assumptions, their expectations, their struggles to get what they want, in a job or a place in the community or a government protection. We call these "cultural values." To learn about them, one does not simply distribute a questionnaire. This is a task for professionals, and a tough one even for them.

After one has dreamed in large concepts for a while, the old self-discipline starts functioning. So we study this village and that union. carefully and in context. Then we compare the value systems of different groups. Then what happens? We find that we now understand some of the bases of people's attitudes toward each other. We see much more clearly the common assumptions, the common ideals at work in a community, and the subtle differences. We see how different people can get self-respect and a feeling of well-being in very different ways. To struggle for self-esteem is to be human. To struggle in a particular way is to belong to a particular culture and society.

To make life more satisfactory for Alaskans, one must study Alaska. And in understanding the processes of behavior in any group, one understands more about mankind and contributes to basic science.

Circumetrics

N. T. GRIDGEMAN

The author is a graduate of the University of London. Although his interests originally lay in the field of chemistry, he turned to biochemistry and has specialized in the fat-soluble vitamins and biological assay. He is now a statistician in the Division of Applied Biology, National Research Council of Canada, Ottawa.

Also he made a molten sea of ten cubits from brim to brim, round in compass, and five cubits the height thereof; and a line of thirty cubits did compass it about.——II Chron. iv: 2

VENTURE a hop, a skip, and a jump into the enchanted labyrinth of mathematics, and you encounter, not a monster, but a protean character of numerous and subtle disguises. It may appear the integral of $(1+x^2)^{-1}dx$, may turn up as the square of the gamma function of one-half, or be revealed as the number of *i*'s in the natural logarithm of minus one. And even if you decline to have truck with such high-falutin appearances, you can hardly withhold friendly recognition of its best known, although far from most characteristic, garb, one that has endowed it with a practical interest having a documented history of four millenia, and a record beyond compare of man-hours of devotion by the geniuses, the norms and the cranks of half-a-dozen civilizations round the globe. For our Proteus is also the measure of the circumference of a circle in terms of its diameter, known in modern times as π (symbolizing perimeter).

In the beginning[1-6] the problem of estimating π was often cast in the allied form of squaring the circle, the Rhind Papyrus of about 1700 B.C. providing us with a perfect example in A'h Mosé's injunction to "cut one-ninth off the diameter and construct a square on it; its area will be equal to that of the circle." In other words π is there declared to be $(4/3)^4$ or 3.1605, a most respectable value—one that is way ahead, not only of Solomon's unwisdomly three, but, more impressively, of the approximations used by competent Roman engineers some sixty generations later. Meanwhile, the Greeks, geometers *pur sang*, had pondered the question; Archimedes, working with polygons up to 96-sided, laid down upper and lower limits of 22/7 and 223/71. Ptolemy's value was 3° 8′ 30″, which is 3.1417. This represents an error of roughly 25 parts per million, equivalent to about two-thirds of a mile in the circumference of the earth, and it prompts the question of why, if a tolerance good enough for a twentieth century precision machinist was achieved in second century Alexandria—why has its ultra-refinement been so zealously pursued through the ages that followed? An adequate answer could not be attempted here; it is too complex; but it may be noted that the question is bound up with the scholastic interregnum of the Dark Ages, with the weaknesses of communications, with the belief that π was a sort of philosopher's stone of mathematics, and even with the spirit that sends men to the toil of Mount Everest. The interregnum meant a virtual cessation of π-work in the West for roughly 1000 years; and the communication weaknesses linked up with the formidable linguistic differences between cultures that obscured from us, until comparatively recently, the pi-istic contributions of the Far East. Additionally, the all too common Asiatic inclination to fuse science and Holy Writ, and the twisting of expression to meet the demands of versification—especially in India—combined to fuzz the picture. Today however there is a bulky literature on the topic from which we cannot fairly omit to mention one or two highlights. For example, in A.D. 479, Tau Tung Chih put forward the fraction 355/113, which is correct to six decimal places (and which was exported to Japan, and was to turn up in sixteenth century Europe in the writings of Otho and Metius). A hundred years later Arya Bhata the Elder's work with polygons led to the recipe "Add 4 to 100, multiply by 8, add 62,000, and the result is the circumference of a circle of diameter 20,000," which implies a π of 3.1416. In both India and China, as well as in certain European centers, there have been advocates of $\pi = \sqrt{10}$, and it is perhaps surprising that the obvious appeal of this value has not had more success—especially among circle squarers.

Pervading much early speculation and measurement is the belief that that π is a rational quantity, i.e., that it can be exactly rendered as a vulgar fraction. The Greeks were fully aware of the concept

of irrational quantities, *teste* Pythagoras's celebrated proof of the irrationality of $\sqrt{2}$, but it was not until 1761 that π was put into that category by Lambert (and thirty-three years later Legendre made the important extension to π^2). And even those who felt in their bones that a simple fraction was unrealizable could not be blamed for putting their money on some not too cumbrous expression that would be π. Such as, for instance, Kochansky's $\sqrt{4+(3-\sqrt{1/3})^2}$ yielding 3.14153, and Specht's $1/10^7+\sqrt{(146)}\times 13/50$ in 1808, which is correct to 14 decimals! In 1882 Lindemann settled once and for all the kind of number that π is by a brilliant proof of its being transcendental, thus giving the congé to all attempts to square the circle geometrically. Almost needless to say, no circle squarer worth his mettle was deterred by the Great Event of 1882 (any more than he had been by the significant and forward-looking resolution of the French Academy of Science, a hundred years earlier, not to accept any more papers on the subject). Indeed, as we shall see in a moment, at least one later champion carried his banner with the strange device to august and unlikely places.

But to go back: an important transition in pi-valuation took place during the Renaissance, namely, the replacement of polygonning by the setting up of infinite series that could be calculated to whatever degree of accuracy a man cared to give his time. Probably the first formulation was Vieta's

$$\frac{2}{\pi}=\frac{\sqrt{2}}{2}\,\frac{\sqrt{(2+\sqrt{2})}}{2}\,\frac{\sqrt{[2+\sqrt{(2+\sqrt{2})}]}}{2}\,\cdots$$

in 1597. Nevertheless, polygon devotees throve for long enough afterwards. Ludolph van Ceulen, for example, began, about the time of Vieta's discovery, to lay aside all other activities to make piiferous polygonning a full-time occupation. By the turn of the century he had established 20 decimal places; when he died in 1610 the number was 35, which value of π was inscribed on his tombstone at Leyden; and apparently π is still sometimes called the Ludolphian Number by German-speaking peoples. In 1630 Grienberger published the 39 places of π he had sweated out using a simpler process than van Ceulen's. His was the last effort of note, although constructional polygonning as an indoor sport continued, and late in the nineteenth century a dedicated German devoted ten years to inscription of the fifth and last prime-sided polygon of Gauss's series; the resultant 65,537-sided figure lies today in the archives of Göttingen, a wondrous monument to human diligence. The seventeenth century witnessed the development of convergent infinite series and fractions: Gregory and Leibniz evolved the series

$$\pi/4=1/(2+3^2/(2+5^2/(2+7^2/(2+9^2\ \ldots$$

that was later to become the basis of modern rapidly converging series, although it is itself arithmetically exasperating when x is put equal to unity for the equality of $\pi/4$. Then the Englishman Wallis turned up with the remarkable formula that may be stated: $\pi/2$ is the product of the infinitude of terms $n^2/(n^2-1)$, where the n's are the even integers. Incidentally, en route to this expression Wallis concentrated on the area of the quadrant and found himself building up a series that cried out for the method of interpolation—for which he had no tools. So he appealed to Newton whose solution of the problem led to his evolving the Binomial Theorem. This is one of several examples of the insemination of other departments of mathematics by pi-ists. At about the same time Wallis's contemporary and correspondent, Lord Brouncker, worried out what was then a new kind of expression, namely,

$$\pi/4=1/(2+3^2/(2+5^2/(2+7^2/(2+9^2\ \ldots$$

and thereby initiated the study of continued fractions.

In the year before the century ended, Abraham Sharp, working under the direction of Halley and using the Gregory-Leibniz series, evaluated π to 72 places, only one of which was subsequently found to be incorrect. In 1706 a derivative of the same series was used by Machin to compute π to 100 places, and his formula,

$$\pi/4=\text{arccot } 1=4 \text{ arccot } 5-\text{arccot } 239$$

was to become famous—and has never really been bettered. It is, as a matter of fact, one of innumerable "arccot splits" that can be used to compute π, and over thirty of them are to be found in the literature. Among notable ones is Strassnitzky's arccot 2 + arccot 5 + arccot 8 with which Zacharias Dase, a sort of animated Eniac in the employ of Gauss, calculated π to 200 places in 1844. This was early in an era that can almost be described as one of furious competition among European pi-makers. Three years earlier Rutherford had computed π to 208 places (of which, however, the final 56 were in error) with an arccot-split given by Euler; and he eventually built up 440 correct places (1853). Meanwhile, in 1847, Clausen, reverting to Machin's formula, had published a 205D value, correct to 200D. Then Richter published 330D, of which 300 were correct, in 1853,

and in the following two years he pushed on to the 500th place. In this he was just behind the Englishman William Shanks, who hit the jackpot with a 530D value via the Machin formula. But not content with so small an edge over his rivals, Shanks pressed on to reach apogee in 1873 with the 707th decimal. This singular triumph was to remain peerless and unchallenged until post World War II; no doubt it was felt that pi-valuation, like Rodgers' Kansas City, had gone about as fur as it could go, and in a book revised as recently as 1945 can be found the statement that anyone wishing to reach the 1000th place must be prepared to devote ten years to the job. The writer must momentarily have forgotten that he was living in the electronic age. Incidentally, although the several computors' values up to 500 or so decimals provided cross checks, the higher Shanksian figures could not be anchored to any such source of confidence. The 707D has been constantly quoted over the years despite the early discovery of the suspicious circumstance that the distribution of the digits was not random, and that there was a deficiency of 7's.

We now move to the ancient English city of Chester where, quite recently, pi-ist Ferguson, using the formula

$$\text{arccot } 1 = 3 \text{ arccot } 4 + \text{arccot } 20 + \text{arccot } 1985$$

attributed to a colleague, although in fact to be found in a Victorian textbook, began to build up π anew, and in 1946 was able to announce that Shanks's 528th decimal, and hence every subsequent one, was wrong.[7] Meanwhile Smith and Wrench, of Georgia and Washington, D. C., working with Machin's old formula, were doing the same chore; they confirmed the Shanks breakdown point, but did not wholly agree with Ferguson's amendments.[8] There followed a period of Transatlantic error-swapping that prefaced a Ferguson-Wrench collaboration that really got the bugs out of the high decimals and enabled them to publish a cross-checked value to 808D in 1948.[9, 10]

And now a formidable group begins to paw the pi-ous ground namely, the ENIAC workers.[11, 12] Three of them, Reitwiesner, Neumann, and Metropolis, began by weighing the relative convenience to eniackery of three arccot-split formulas, including Machin's, the one Ferguson had used, and another. The first-named, they decided, was best suited to the *modus operandi* of the computing machinery. Having carefully programmed the work, they chose the long week-end that included Labor Day, 1949, to carry out the job without interfering with ENIAC's routine work. Four operators worked eight-hour shifts, night and day, putting in a total of seventy man-hours, and emerged, pale-eyed but happy, with π to an elaborately checked 2035D. Subsequently, the digits (which, incidentally, confirmed the Ferguson-Wrench sequence) were thoroughly tested for randomicity and came through with flying colors. Only the irredeemably soulless will not take off his metaphorical hat to the heroes of that memorable week-end.

In pursuit of the main flight of pi-ists we have necessarily had to leave behind an interesting side flight and the lunatic fringe; to these we must now return. The side flight is probabilistic π, quite a subject in itself: it is concerned with the making of trials of various possible conjunctions such as the random selection of pairs of prime numbers that are prime to each other, or the random contact of regular objects, whose probabilities are expressible as formulas that contain the number π. Thus Chartres, in 1904, made a random selection of 250 pairs of primes and found that 154 of them were prime to each other; and as it is known that the probability of the conjunction is $6/\pi^2$, his trial amounted to an estimate of π of 3.12. A trick that has been attracting spasmodic attention ever since its first presentation by Buffon (yes, the naturalist!) rests on this proposition: In repeated throws of a stick of length d (less than unity) onto a grill of parallel lines unit distance apart, the fractional frequency of hits will tend to $2d/\pi$. Several results of π-estimations by this means are on record, the most spectacular being by Lazzerini in 1901 who made 3408 throws to produce a value of 3.1415929, an apparent error of one part in ten millions! Unfortunately it is not difficult to show that this and some other impressive results are too good to be true; they are not "estimates" at all, and must have depended heavily on the experimenters' preconceptions. Mechanized versions of Buffon's device have been used in science exhibitions to interest visitors in helping build up an estimate of π by operating it themselves.

A comparable proposition regarding the dropping of a sphere onto a plate in which circular holes (of larger diameter than the sphere) have been cut in a regular pattern, is as follows: The frequency of clear throughfalls tends to $2\pi/\sqrt{3}\, k^2$, where k is a function of the dimensions. The notable feature of this device is its having been tested with extraordinary thoroughness by Clarke in the early 1930's. He recorded no fewer than 250,000 falls of a ball-bearing onto a machined steel plate and arranged for the contacts to be electrical and to

produce a headphone click.[13, 14] His final best estimate of π was 3.143.

What I have called the lunatic fringe is the habitat of the circle-squarers, mostly those perfervid souls who, in effect, feel divinely convinced that π must be a simple number on the ground that God would hardly permit so important a value to be incommensurable. They come from all classes in every country. Even Thomas Hobbes, whose sanity was positively awesome, and who would scorn to invoke His geometry, hotly insisted, in his *De Problematis Physicus,* that π must be 16/5, and he wrote the most scarifying tracts against Wallis and his more orthodox views. Joseph Scaliger, the great Latinist, fell victim, and many were influenced by his authority. Your true pi-man, by the way, is rarely a Simple Simon; as like as not he will also be a pyramidologist, or a perpetual motioner, or a carrots-and-sex regimer, or a flat-earther (although the ranks of the last-named have thinned pitifully in recent years) into the bargain. We may instance Henry Sullaman who found that the Number of the Beast, 666, was the key to the problem. Then there was de Causans, a French Guardsman who, according to de Morgan, "cut a circular piece of turf, squared it, and deduced original sin and the Trinity." It is to de Morgan's delightful book, *A Budget of Paradoxes,* that we owe much entertaining information about that prince of circle-squarers, James Smith of Liverpool, who, in the 1850's and 1860's, devoted an incredible amount of time, energy, and paper to his contention that $\pi = 3\frac{1}{8}$. He would send de Morgan enormous letters, sometimes several in a week, to try to convert the errant mathematician; and he generally exhibited all the symptoms of what de Morgan called *morbus cyclometricus*. In connexion with the seemingly endless dance he was led by the contemporary circle-squarers, de Morgan once declared that their patron saint should be St. Vitus.

It is sad that de Morgan did not live to appreciate the wondrous things that were later to happen in the Middle West.[15] In 1889, Dr. Goodwin, a medical man of Solitude, Indiana, copyrighted the following statement: "A circular area is equal to the square on a line equal to the quadrant of the circumference; and the area of a square is equal to the area of the circle whose circumference is equal to the perimeter of the square." Later he published an article *in an orthodox mathematical journal* on the "The Quadrature of the Circle," the opening paragraph being the copyrighted statement given above and that thereafter drivelled on to "prove" that $\pi = 16/5$ by a method that involves, by implication, the identity $7^2 = 50$. The article was published "at the request of the author" and is significantly never referred to in the lively discussion columns of subsequent issues. Dr. Goodwin made good use of that publication; it enabled him to claim that his quadrature was "accepted as a contribution to scientific thought" by the journal, a claim that was prominent in the taking of his next step. On January 18, 1897, Goodwin's county representative introduced House Bill No. 246 into the Indiana State Legislature; it began:

> A bill for an act introducing a new mathematical truth and offered as a contribution to education to be used only by the State of Indiana free of cost by paying any royalties whatever on the same, provided it is accepted and adopted by the official action of the legislature of 1897.
>
> Section 1. Be it enacted by the General Assembly of the State of Indiana: It has been found that a circular area is to the square on a line equal to the quadrant of the circumference, as the area of an equilateral rectangle is to the square on one side. Etc., etc., etc.

The worthy Representatives, finding that they would be able to collect royalties from the willing scholastic converts beyond their borders, routed the bill through the Committee on Canals (*sic*) then through the Education Committee, both of whom approved it, and on February 2 the House voted its acceptance by 67 to 0. It then went to the Senate who thoughtfully referred it to the Committee on Temperance, which, evidently convinced that it did not endanger the sobriety of Indiana's residents, promptly passed it. By this time, however, the Indianapolis press had got hold of the story, and it began to spread across country. Just in time the Senate woke up to the enormity of the gaffe it was about to commit, and on February 11, House Bill No. 246 was postponed indefinitely. So fell the curtain on what is surely one of the most fantastic scenes in the history of both pi-istics and legislature.

A myth of peculiar virility in the busy world of circle squarers is The Reward, a princely sum alleged to have been set aside, usually by a government, and mostly that of France, for the long-sought solver of the "problem" and, *ipso facto,* benefactor of mankind. The last century saw the high-water mark of would-be applicants; now they are pretty rare. But not extinct; only a few years ago access was gained—indeed, almost forced—to the study of a well-known Canadian scientist by a tense little man who held him with his glittering eye, announced with suppressed passions that he had just squared the circle, and please would the doctor tell him how to set about collecting The Reward? The scientist gently assured his visitor

that he would have to be content with the laurels of success finely won, and, -er, perhaps he would demonstrate his achievement? The little man rose in silence, cast over the desk a look in which was mingled impatience, scorn, and a cagey I-know-a-con-man-when-I-see-one, and swept out. From his window the scientist watched with emotion the last proud standard bearer of St. Vitus hurry round the block into a street where, come to think of it, the French Consulate happened to be. Did I, foolishly, say *last*? No, no; doubtless, someone, somewhere, at this very moment. . . .

References

1. DE MORGAN, A. *A Budget of Paradoxes.* London: Longman Green (1872).
2. CAJORI, F. *A History of Mathematics.* New York: Macmillan (1926).
3. ———. *A History of Mathematical Notation.* Chicago: Open Court Publishing Co. (1928).
4. BALL, W. W. R., and COXETER, H. S. M. *Mathematical Recreations & Essays.* London: Macmillan (1892–1947).
5. KASNER, E., and NEWMAN, J. *Mathematics and The Imagination.* New York: Simon & Schuster (1940–45).
6. MCKAY, H. *The World of Numbers.* New York: Macmillan (1946).
7. FERGUSON, D. F. *Nature,* **157,** 342 (1946).
8. ARCHIBALD, R. C. *Mathematical Tables and Aids to Calculation,* **2,** 143 (1946–47).
9. FERGUSON, D. F., SMITH, L. B., and WRENCH, J. W., JR. *Ibid.,* 245.
10. FERGUSON, D. F., and WRENCH, J. W., JR., *Ibid.,* **3,** 18 (1948–49).
11. REITWIESNER, G. W. *Ibid.,* **4,** 11 (1950).
12. METROPOLIS, N. C., REITWIESNER, G. W. and VON NEUMANN, J. *Ibid.,* 109.
13. CLARKE, A. L. *Can. J. Research,* **9,** 402 (1933).
14. *Ibid.,* **11,** 658 (1934).
15. EDINGTON, W. E. *Proc. Indiana Acad. Sci.,* **45,** 206 (1935).

RUBY

Now as the sun incarnadines the West
This stone reflects it in a sea of flame;
The children of July proclaim it best,
And ruby, or red sapphire, is its name.
Here is a gem to fan the heart's desire;
In times gone by an Emperor of Cathay
Illumed a golden chamber with its fire;
It lit a god's abode, so legends say.

Fabulous stone, to match the "pigeon's blood,"
Yet clear, transparent, held against the light,
Red, red, and red again, and still more red,
Its brilliance penetrates the darkest night;
Here are the carmine rose, the velvet bud,
And poppies growing, cardinals in flight.

MAE WINKLER GOODMAN

Cleveland, Ohio

Mapping Water-Saturated Sediments by Sonic Methods*

W. O. SMITH and HERBERT B. NICHOLS†

William Smith, geophysicist in the Ground Water Branch of the U. S. Geological Survey, received his B.A. from the University of Pittsburgh and did graduate work at the University of California, Berkeley, and at the California Institute of Technology. He is the author of approximately fifty technical publications. His recent activities have included over-all direction of siltation studies at Lake Mead. Herbert Nichols, Information Officer for the U. S. Geological Survey, Washington, has a background of eighteen years as Science and Engineering Editor for The Christian Science Monitor. *Since taking his bachelor's degree in Systematic Botany at Harvard in 1932, he has made a career of writing about technical progress so that laymen as well as scientists in other fields may be informed.*

COMPLETION OF OCEANOGRAPHIC STUDIES in the Bay of Fundy for the Corps of Engineers, U. S. Army, makes it appropriate to look back over the progressive stages whereby a new technique was developed for geophysical investigations. This new method is based upon the speed with which sound waves of very low frequency penetrate water-saturated unconsolidated sediments.

The basic instrumentation is operated on the sonar (echo) principle, using a sonic signal given off by what is known as a "transducer." This is comparable to a radio station transmitter, of greater delicacy than a fine, jeweled watch and many times more expensive. In the working position the transducer is slung out over the side of the vessel from a davit. Essentially it consists of a metal and rubber container with carefully polished crystals, some of which change outgoing electrical impulses into sound waves as they head bottomward. Other crystals on the receiving end retranslate the returning echoes into electrical impulses which can be amplified and made to write their own record of underwater travels.

To date, such sonic methods have been successfully used to provide data on the approximate useful life of Lake Mead as a reservoir for water supply and power production; to supply the City of Chicago with a map of bedrock contours offshore under Lake Michigan along the route of its proposed $85,000,000 water tunnel; and, most recently, to outline for the Army Engineers the bottom conditions likely to influence location of engineering structures at the mouths of Passamaquoddy and Cobscook bays, should it ever be decided to develop tidal power in this area. (See paper by W. O. Smith in *Am. Inst. Mining Engrs.*, in press, 1953). There is evidence, too, that this same technique can be applied to measuring the capacity of large underground aquifers, particularly where the water table is not far beneath the surface of the ground.

For all this work the penetration of sound through sediments has been the basic yardstick. In the past there has been considerable uncertainty regarding the conditions under which sound will penetrate sediments, and doubt had been expressed that it would do so at all. These doubts have been dispelled through basic studies by the Geological Survey during the past several years, studies accomplished in close cooperation with the Navy Department.

First it is essential to recognize that the nature of sediments plays a dominant and often complicated part in their acoustic response. Sediments may consist of gravel, sand, silt, and clay in various proportions. Organic matter also is often present. Clays are, in general, plastic and present many complications. The importance of differentiation in sediments becomes apparent as investigations progress; and the physical conditions, especially as they relate to water content, are likewise important.

The Lake Mead Problem

Thus far all the Survey investigations have been concerned with saturated sediments overlaid by

* Publication authorized by the Director, U. S. Geological Survey.

† The authors wish to acknowledge the assistance of G. B. Cummings, formerly of the Bureau of Ships, Navy Department.

water, the depths ranging from a few feet to several hundred. The first observations on sound penetration in sediments were made at Lake Mead during 1948–49. The Bureau of Reclamation had encountered certain major problems in the operation of Lake Mead, the storage reservoir behind Hoover Dam, and had requested Survey assistance to determine the precise capacity of the reservoir and the rate at which sediment is accumulating and to give a good estimate as to the probable life of the reservoir.

Inasmuch as fairly accurate photo maps existed showing the terrain prior to construction of Hoover Dam, it was surmised that the sonic method could be used extensively in obtaining the needed data. The Secretary of the Interior requested assistance from the Navy Department. At first it was thought that Navy sonar equipment and personnel to operate it would be sufficient. But the problems introduced and the possibilities that developed were so far reaching that before the work was completed assistance rendered to the Survey included that from the Bureau of Ships, the Navy Electronics Laboratory, and the Bureau of Ordnance. The Scripps Institute of Oceanography also gave considerable aid.

Two years were required to plan the operations, assemble equipment, gather comprehensive data, and accomplish the necessary interpretations. Since the mapping of lake sediments required an accurate reference system to which all soundings could be referred, a third-order control net was established by geodetic engineers of the Geological Survey. The triangulation net as established consisted of a series of accurately located reference marks around the lakeshore. There were 307 such shoreline marks and 27 additional triangulation stations which tied the various surveys together and connected with base points of the U. S. Coast and Geodetic Survey. By taking sights to the shore stations, hydrographers out on the lake could locate themselves accurately when making soundings, taking samples of silt, or making other observations. Lake Mead is roughly a large Y in shape, with its base at Hoover Dam in Black Canyon. The north fork is the submerged river channel of the Virgin River; the south is the submerged Colorado.

Primarily the problem was to find out how much mud, silt, and sand had been dumped into the lake since the waters backed up to spillway level. Water and sediment accumulating in the lake come from a region extending far beyond the limits of the area studied. This extended area includes some 168,000 square miles, or roughly 5 per cent of the continental United States. The original plans called for the delineation of the top of the sediment by sonic soundings using standard Navy depth-finding equipment. Such instruments project sound waves downward from the surface and automatically translate into fathoms of depth the length of time it takes such waves to strike bottom and be reflected back to the instrument. The bottom of the sediment was to be determined from the topographic survey made by the Fairchild Aerial Survey just before the lake was filled. The difference would be the sediment thickness.

It was soon discovered that under certain conditions the existing sediments were easily penetrated by sound. A frequency of the order of 50 kilocycles did not penetrate except under certain conditions, but a lower frequency of 15 kilocycles did go through regularly when the sediments were of medium consistency. This generally meant fine silt and clay having a water content of 40 to 50 per cent.

An examination of a typical sonic profile taken 20 miles above Hoover Dam revealed some 80 feet of distinct sediment deposits produced by density flows, and the rock bottom of the old river channel was easily distinguished. Coring showed that these sediments were not very compact. Inasmuch as a complete topographic survey of the entire Lake Mead area had been made before the lake filled (therefore at a time when it contained no sediments), an excellent opportunity was afforded to compare the original bottom as disclosed by sound penetration with that actually known to exist.

With the exception of a small amount of sediment contributed at four places by slumping of the reservoir walls and by the precipitation of silica and calcium carbonate rock-forming materials carried in solution in the lake water, all the sediments of two great deltas, extending downward from the original head of the reservoir to Hoover Dam along the former river channels, had been supplied by the Colorado and the Virgin rivers systems.

Slightly more than 2 billion tons of sediment was found to have accumulated in Lake Mead. This material now occupies about 5 per cent of the reservoir storage capacity below the spillway level. From figures on the rate of sedimentation, the rate of sediment compaction, and the capacity of the reservoir for storing sediment above the lake level, the now generally accepted estimate of the time required for the reservoir to become filled was computed, assuming that the rate of sedimentation continues as at present. Plotting the mean elevation of the sand deposits and the finer silts and clay as two separate curves projected against time, it was found that they intersect at a point

445 years from the date Hoover Dam was completed, or at about A. D. 2380.

The Chicago Problem

At Chicago the situation was somewhat different, but the main problem again was to measure sediment thickness, determining the depth to bedrock along a 5-mile segment of the lakefront. It is planned to construct a new $85,000,000 Central District filtration plant and distribution tunnel north of Navy Pier to provide additional potable water from Lake Michigan. The present South Side Pumping Purification Plant is already the largest in the world, but the new one will be three times larger. It was important to know beforehand the configuration of bedrock along the proposed route, because for efficient construction and operation the tunnel must be cut about 50 feet below the top of the limestone bedrock.

A preliminary study of existing cores from the area showed that the Lake Mead equipment would not be suitable. It had neither the power nor a sufficiently low frequency to penetrate existing sediments. However, the Navy Department had just developed depth-finding equipment of much greater power, operating at a frequency of 11 kilocycles. With this equipment the Survey researchers felt that they had a fair chance of penetrating the Lake Michigan sediments to reach bedrock.

Sound profiles were obtained over an area about 3 miles wide extending from Navy Pier to Montrose Harbor. Sound velocities were determined from sound traces taken over existing cores. Known cores were thus relied upon for control, core depths being correlated with the corresponding time of traverse required for sound to travel the length of a given core. As was expected, geophysical interpretation of the sound traces also required geologic control. This was obtained from the numerous borings in the Chicago area, and it was not difficult to select the depths to bedrock from the sound traces. The bedrock on shore was contoured by means of the records of the borings. Hence, beginning with those sound records closest to shore, the elevations of bedrock were interpolated for the entire offshore area. Subsequent test drilling confirmed that the contours as drawn were applicable to the whole area.

In the course of the Chicago work, two hidden valleys were located in the bedrock, both buried beneath an overburden of mud and silt. One is 130 feet deep and 2 miles long, more or less paralleling the shore and making an abrupt turn to the southeast to join a larger valley, once part of an

The *David C. MacNichol*, a coastal freighter, pressed into service as a research vessel. Scientists of the U. S. Geological Survey used it to test the value of Navy depth-finding equipment for deep-water investigations of mud and bedrock in areas of prospective marine and submarine engineering operations. (Courtesy U. S. Geological Survey.)

ancient southeast-trending drainage system. The other hidden valley is 6 miles to the north and is about 80 feet in depth. It was traced for 4 miles out into the lake. In addition, several other deep depressions were located along the proposed tunnel route, which means that the tunnelers will need to work at a considerable depth in order to prevent "holing out" into soft mud. The bedrock configuration, characterized by modified karst topography formed by the action of water upon limestone, bears no relation whatever to present surface features like Belmont Yacht Harbor and the lake shore. These last features resulted largely from the action of Pleistocene glaciation.

As in the previous Lake Mead work, a plan of sonic-sounding courses was laid out, and careful sextant work was required to furnish direction control for each course. The equipment used was mounted on a 60-foot tug belonging to the city of Chicago. Applying data obtained from the sonic soundings, G. G. Parker, geologist of the Survey's Ground Water Branch, completed the final map of the bedrock topography.

In the course of the Chicago survey, an experiment was performed which indicated that sonic devices may be useful in ground-water investigations. While attempting to eliminate the difficulties that occur with multiple echoes arising from sound reflections between the water surface and water bottom, the transducer was lowered and placed directly on the bottom at a number of places.

Sextant readers and horizontal control operator at work on the upper deck of the pilothouse determining position of the vessel relative to shore points. Great care had to be exercised in the Bay of Fundy studies to maintain a straight course in spite of strong tidal currents and ocean whirlpools. (Courtesy U. S. Geological Survey.)

Wrapping the transducer in heavy sponge rubber, with only the sound face exposed, eliminated some slight trouble with residual multiple echoes originating from the back of the transducer. All echoes at the receiver then must have come from the sediment.

As a result of this experiment it was concluded that, in those places where the water table is at or within a few feet of the land surface (so that only shallow holes, at most, would have to be dug for the transducer), sonic devices may be used to obtain information on water-bearing sediments below. The main requirement would be to place the transducer a foot or so below the water table when measurements are made. It is conceivable that a considerable amount of ordinary test drilling could be dispensed with wherever sediments are saturated. Greater speed should be possible also, though some drilling would be required to give necessary geologic control.

The Bay of Fundy Problem

Before an opportunity was presented to develop further this approach to ground-water studies, discussions with the Chief of Engineers of the U. S. Army brought forth the possibility that vital data on suspected unconsolidated underwater deposits at potential dam sites in the Bay of Fundy might be gained for the Government at low cost. Funds in the amount of $3,900,000 had been estimated as necessary to reexamine the project for using the wide tidal fluctuations in Cobscook and Passamaquoddy bays as a potential source of electric power.

It was considered highly probable that the use of sonic methods might determine the foundation conditions at proposed structure sites and obviate the necessity for continuing the expensive program of detailed test drilling and sample collecting and testing that had been undertaken in 1935. At the same time, such work would provide the Geological Survey added opportunity to develop the techniques for using sonic devices in sediment studies, particularly in an area where its value for uncovering unknown geologic data on the continental shelf was concerned.

It was known from sediment cores obtained in 1935 that the overburden might be of the order of 100 to 150 feet thick, and that it consisted of glacial till having a water content of 20 to 40 per cent. The till was known to be principally silt, fine sand, and clay, with some gravel as well as marine clay.

To accomplish this new mission the Survey again had to assemble equipment of greater power and considerably lower frequency than that which had been needed at either Lake Mead or Chicago. New equipment developed by the Survey and the Navy's Bureau of Ships was used. A preliminary field study of the geology was made by G. G. Parker, later relieved by Dr. J. E. Upson and S. J. Spiegel, cooperating with E. W. Perkins of the Corps of Engineers. The sounding survey was made during July and August 1951.

The Survey "fleet" consisted of the coastal freighter *David C. MacNichol* and two small power craft which were used as work boats. A separate power plant and a distribution system were installed temporarily to supply power for the electronic equipment. Structural modifications, also of a temporary nature, were made to give adequate storage and work space for the scientific personnel. A small room was constructed in the hold with a bench along one side on which the Edo sonar unit and test equipment were placed. Above the pilothouse a crude, temporary "flying bridge" was added for use by the sextant crew. Two instruments were constantly in use during the operations. One was the new low-frequency high-power unit referred to above. The other was a standard small-ship sonar unit. It had less penetration and was for use close to shore and in shoal areas, mostly as an aid to navigation.

Plots were established for 6 to 10 parallel bot-

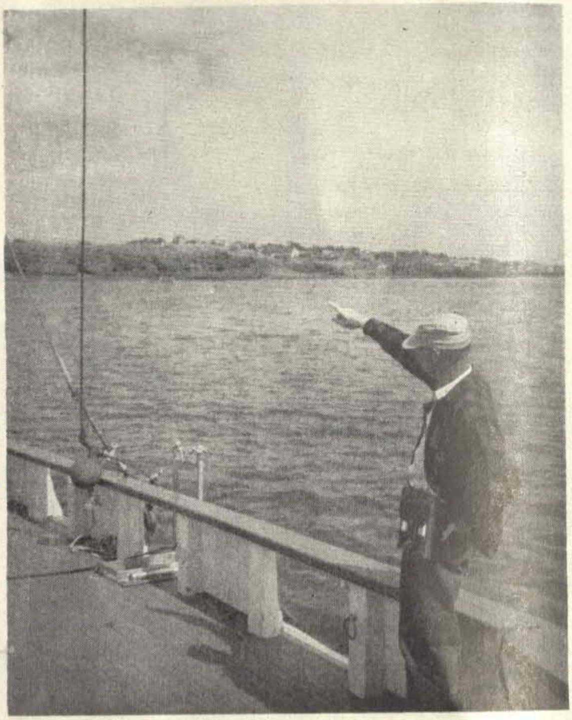
tom profiles to be obtained by following parallel courses in each of eight areas in the vicinity of Cobscook and Passamaquoddy bays: across Letite Passage in the north, the Pope Island to Deer Island area, Western Passage, Friar Roads, Treat Island, Estes Head to Lubec, Shackford Head, and Spectacle Island. The range lines were spaced 500 to 1000 feet, and at least once in each area the ship crossed these parallel courses at right angles to give an additional check on the profiles. Altogether 44 such profiles were made, showing depth of water, depth of overburden, and depth to bedrock.

Two assistants on the flying bridge took sextant readings to shore points so that the ship's course could be accurately plotted and correlated with each depth profile. As each sextant observation was made, the angles obtained were read into the public-address system and received by the plotter in the pilothouse where the ship's position was plotted. In this way there were continuous checks on deviations from the desired range line. When these deviations were serious, a change in course was ordered. Position fixes were obtained throughout each range line and as close to the limiting shores as the vessel could approach without running aground.

Because of the large tidal variation involved (some 18 to 27 feet between high and low tide levels, constituting the differential upon which the Quoddy project would depend for its power), it was necessary to establish vertical control as well. Therefore the ship maintained radio contact with shore points where every 10 minutes the height of the tide was read and added to the record. Leadline soundings also were made periodically to check the instrumental water-depth profiles. A bathythermograph was used to record water temperatures so that it was possible to obtain the correlated temperature and depth data needed for computing the velocity of sound through sea water.

Compiling the data, interpreting them, and completing the required maps of bedrock topography took several months and called for unusual skill. Proper interpretation of the profiles amounts to converting sonic tracings into geologic data—an entirely new avenue of approach to geophysical research. Pinnacle rocks projecting above the mud stand out like radio towers on a terrestrial landscape. Their roots in bedrock areas are as apparent to the scientist as bone structure in an x-ray film.

Bedrock in the Quoddy Bay region is of the Silurian and Devonian periods. Farther inland are fossils indicating that rocks were formed in the area at least during Mississippian and Pennsylvanian times also. However, glacial action and other erosional factors have removed all traces that would indicate the history of the Eastport area itself between the Devonian and the late Pleistocene.



Joe Upson, U. S. Geological Survey geologist, points to a bedrock outcrop of Silurian Age on shore near Eastport, the easternmost city in the United States. (Courtesy U. S. Geological Survey.)

It is the geology associated with the movements of ice during Wisconsin and post-Wisconsin times that is of most concern at Quoddy. Thick deposits of clay, sand, and gravel have been strewn about by glacial action. Sizable outwash plains cut across to the southeast. Proximity to the ocean and the tremendous weight of the intruding ice mass, forcing the coastline beneath the sea, gave the whole area a recent marine history as well. Deep canyons, some still beneath the ocean, were scoured by the action of glacial runoff. Two old river valleys extend out onto the continental shelf from present-day streams. The streams that cut them must have been much larger then than they are now. Offshore ocean currents have been at work on the loose materials available and have contributed additional deposits which show up as marine blue clay overlying the glacial till.

Some understanding of the geologic history of these surficial and submarine deposits is most necessary to engineers both before and during any construction activities. A pleasant surprise, for ex-

ample, was the discovery that the northernmost two-thirds of the coastal area where dams would be needed contains almost no clay sediment. The study showed that most of the sediments are in previously tested areas where drilling was done during 1935–36, just prior to the decision to abandon the project. The cores did serve a useful purpose, however, for they provided the needed control on which to base geophysical interpretations of the current survey. Good agreement was obtained for the bedrock elevations determined from those cores, and corresponding elevations were computed from sound velocities in the neighborhood of 6200 feet per second.

The work at Lake Mead, Chicago, and Passamaquoddy allows the conclusion that frequency is critical in so far as penetration of sediments by sound is concerned. Frequencies of 50 kilocycles and higher do not penetrate underwater sediments satisfactorily. Frequencies below 15 kilocycles do penetrate. Frequencies and power levels now obtainable appear to be adequate for locating bedrock beneath underwater sediments ranging in thickness from a feather edge to several hundred feet. The principle appears adaptable for groundwater investigations wherever the water table is close to the land surface. However, in this direction further investigation is needed.

DAY AND BEACH

On what once held an inland sea,
This shore has bits of stone.
A comber of eternity
Rolls in the wind alone.

And I, a lover of the light
And swimmer of the sun,
Now push the pale drifts left and right
That ages have begun.

DANIEL SMYTHE

Delanson, New York

SCIENCE ON THE MARCH

OUR HERITAGE OF GOOD FRUITS

PLANT breeders have given to the farmer and home gardener a number of varieties of fruits, flowers, and vegetables which are radically different from original types. In many instances dwarf varieties are available to the amateur with limited space for planting, whereas the commercial grower, who is primarily interested in larger yields, has recourse to types which produce bigger and better crops as the result of hybrid vigor. Double blossoms have largely replaced single flowers in border planting, and one may have blue roses and pink African violets if one wishes. Small fruits, like the strawberry, raspberry, and others have been bred to suit any clime in the United States so that successful production need no longer be restricted to a limited region. Another area in which the plant breeder has been successful is in the production of strains of plants which are resistant to specific diseases.

The object of this paper is not to discuss the excellent work of plant breeders, but rather to relate a few stories regarding varieties of fruits which have been given to us by nature, in many instances antedating the plant hybridizer. Sometimes the new type has arisen from a seed which was planted by a grower who knew nothing of the seed's ancestry. He merely planted a seed and "took a chance." In other instances, the new fruit has been discovered on a tree which was believed to be a seedling, perhaps found on a farm or growing "wild." The latter is sometimes referred to as a "wilding." In all cases the new variety appeared by chance, without its discoverer having had any part in pollination or hybridization or in any other manner planning its parentage.

Then there is the "bud sport." Sometimes flower buds on a single branch of a tree will bear fruits which are quite different from those which are produced on other parts of a tree. The new fruits may be seedless, they may have a different internal or external color, they may mature earlier or later, or they may be larger or smaller. These are bud sports. It is a characteristic of the whole branch which bears these fruits, because leaf-buds, rather than flower-buds, may be removed and inserted into the bark of another tree (of the same species), and the "bud wood" will grow and produce fruits identical with those of the original bud sports.

The Concord Grape. Let us begin with the story of the Concord grape. Most of us are no doubt familiar with this attractive blue variety of grape that makes its appearance on the markets in the early fall. It is the most widely grown grape on this continent and with its offspring, both pure-bred and cross-bred, represents 75 per cent or more of the grapes of eastern America. Here is how it started.

The early colonists attempted to introduce European grapes into this country from the very beginning of colonization. Grapes which they brought with them were the Vinifera or wine-grape type, such as is grown extensively in California at the present time. Attempts at wine production, or more properly the production of wine grapes, were made not only to please the palates of the colonists but also to create an industry for the colonies. So for 200 years experiments to grow wine grapes were made in all parts of the eastern United States, and for 200 years the experiments failed. It is now known that European grapes failed to grow in America because of attacks by the Phylloxera insect, mildew rot, and other native parasites to which the native American grapes are comparatively immune.

It seems that for a long time it never occurred to the early colonists to attempt to cultivate the wild native grapes. It is true that wild grapes were small-fruited for the most part and full of seeds, but the Indians utilized them. Eventually, however, the colonists began cultivating native species of grapes. John Larson, a Scotch engineer, spent eight years, beginning in 1700, in exploring and surveying North Carolina. He reported six kinds of wild grape, three of which had been moved to gardens.

Two important events occurred in 1852 which tended to revolutionize grape growing in America. The first of these was the production of hybrids between American and European grapes. The second event was the introduction of the Concord grape. It is this second event with which we are concerned. In the fall of 1852 E. W. Bull, of Concord, Massachusetts, exhibited a seedling grape at the meeting of the State Horticultural Society. The seedling was named the "Concord." Bull had planted the seed of a wild grape in his garden, and it bore fruit in 1849. The wild grape, from which the seed had come, had been transplanted to the garden from beside a field fence.

Other than this, the actual ancestry of the

Concord is not known. The botanical characters, according to Hedrick, indicate that it is a purebred Labrusca (Fox grape) although some are of the opinion that it is possibly a Labrusca-Vinifera hybrid. A Catawba grape vine was growing in the garden at the time that the Concord was discovered, and it is possible that the Catawba vine may have fertilized the seed from which the Concord grape was produced.

The new grape was introduced by Hovey and Company of Boston in the spring of 1854. From the time of its introduction its growth was phenomenal. Within a year its culture had spread halfway across the continent. In 1865 the Concord grape was awarded the Greeley prize by the American Institute. Horace Greeley, the donor of this prize, considered the Concord the best grape for general cultivation and called it the "grape for the millions." When one realizes that there were no other "juice" grapes at that time, it is not surprising that the Concord made such a hit. Even today this grape is one of the most popular varieties. There are better grapes in some respects, but they cannot be produced as cheaply as the Concord and must take second place from the standpoint of commercial production.

Two other table grapes appear on the market at about the same time that the Concord is offered for sale. These are the large, green or white, Niagara and the small, pink-fruited Delaware. Niagara is a known cross between Concord and Cassady, produced by C. L. Hoag and B. W. Clark of Niagara County, New York. The Delaware, however, belongs in our story because its origin is unknown. It was brought to the notice of the Ohio Pomological Society in 1851. It was traced to the garden of Paul H. Provost, a Swiss of Frenchtown, New Jersey. One story says it came from a brother residing in Italy; another that it was brought to Provost's place by a German. It could have grown up in the garden as a seedling.

Other Small Fruits. The origins of some of the other small fruits may not be as dramatic as that of the Concord grape, but many of them were chance seedlings or wildings or bud sports, and they serve to substantiate our claim that nature provided generously until man took over. For example, a very popular red raspberry, the Cuthbert, originated as a chance seedling in 1865. This is a large-fruited variety with a delectable flavor and aroma. Golden Queen is a bud sport of Cuthbert. It has been referred to as the only yellow raspberry which is worth planting. Golden Queen is more

Grapefruit on trees growing on the Yuma Mesa, Arizona (U. S. Bureau of Reclamation).

cream colored than yellow, and it retains all the flavor of the Cuthbert.

Most of the old standard varieties of black raspberries, such as the Cumberland, are supposed to have originated as seedlings. The same is true of one of the most popular purple varieties—the Royal Purple. Likewise, many of the older varieties of commercial blackberries originated by chance. Eldorado, Blowers, Agewam, Early Harvest, and Mersereau were either seedlings or wildings.

Perhaps more new varieties of strawberries than any other fruits have been introduced in recent years. In the past, however, we were dependent upon nature's productions for market varieties, many of which are still being grown commercially. A typical example is the Missionary strawberry, which originated as a chance seedling in 1916 in Virginia. It practically "grew up" with the winter strawberry industry in Florida. For about twenty-five years strawberries were shipped from Florida to the northern states during the winter months, and for many of those years it was the Missionary variety that was shipped exclusively. Strawberries were among the first luxury winter fruits in this country.

Peaches. If one seeks a dramatic origin of a variety of fruit, one need not look beyond the peach industry. The year is 1870. The scene is Marshallville, Georgia. In this region peach trees are always at their best, as they grow with their roots deep in red clay and their tops reaching up toward clear blue skies. In the year just mentioned Samuel H. Rumph, a prominent peach grower, had a "Chinese Cling" peach tree on his farm. Fruits of this variety are best suited for canning or preserving. Two seeds from the fruits of this Chinese Cling

Bing cherries from the state of Washington (U. S. Bureau of Reclamation).

tree made history. Mr. Rumph planted one seed and gave the other to his brother, L. A. Rumph. The first-mentioned seed produced the well-known Elberta peach. This is now the leading commercial variety, at least in the eastern United States. A large fruit with yellow flesh and usually red around the pit, the Elberta has graced many a fruit bowl. Since it is a late variety, it does not appear on the market until late summer, and it is marketed until frost.

What ever became of the other seed of the Chinese Cling tree? Mr. L. A. Rumph, who also lived in Marshallville, planted his seed too. Strangely this produced the "Belle of Georgia." In the opinion of many, no better peach was ever discovered. The flesh is white and not quite so firm as that of the Elberta; flavor is indescribable and Hedrick has spoken of this variety as "truly voluptuous in in form and color." Because of its tender flesh, Belle of Georgia will not stand the rigors of shipment to distant markets and is not able to compete with firmer varieties.

How did both a yellow and a white-fleshed variety originate from the same tree? Mr. Rumph stated that the Chinese Cling tree stood near Early and Late Crawford, Oldmixon free-stone and cling trees. Ample opportunity for cross-breeding existed so that each of the two seeds could have had different parents.

Other commercial varieties of peaches which originated as chance seedlings are J. H. Hale, Hale Early, Champion, Hiley, and, in all probability, Early and Late Crawford.

Cherries. Many of our leading cherry varieties in this country have a rather mysterious past. Early Richmond, a popular sour cherry, is the old Kentish of England and may have been introduced into England by the Romans. A standard late sour cherry in America is the English Morello. Its origin is unknown, but it is believed to have originated either in Holland or Germany, from whence it was introduced to England and later to France. By far the most popular sour cherry in this country is the Montmorency. It originated in the Montmorency Valley in France several centuries ago.

Of the sweet cherries, Napoleon is a leading firm-fleshed variety. It was grown by the Germans, French, Dutch, and English early in the eighteenth century, but its history prior to this is obscure. Another sweet cherry, Lambert, flourishes best in the northwestern part of the United States. This cherry originated as a seedling under a Napoleon tree about 1848.

Apples. Smock and Neubert, in their book entitled "Apples and Apple Products," state that there are well over 1800 varieties. Not many of these, however, enter into commerce today and, when considering only the most popular commercial varieties in the United States, the list can be reduced to 24. And what is significant is that only one of these 24 has been produced by plant breeders. It is the Cortland, which is the outcome of a cross between Ben Davis and McIntosh. All the remaining varieties have resulted either from chance seedlings or from bud sports, or the origin is unknown. Let us consider a few of them.

Rhode Island Greening and Winesap have been traced back to Colonial days, but the exact origins of these apples are unknown. Arkansas, Baldwin, McIntosh, Northern Spy, Stayman, Wagener, and Yellow Newtown originated as seedlings during the period between 1740 and 1892. Golden Delicious is another seedling, but its advent was more recent, since it was introduced in 1916. The following varieties were "found" and in all probability they were chance seedlings: Delicious, Esopus of Spitzenberg, Grimes Golden, Jonathan, Rambo, Smokehouse, York Imperial.

Pears. Most of our popular pear varieties have come to us from Europe. Bartlett, which leads all other varieties in America in number of trees, came to us from England and is there known as Williams. A schoolmaster from Berkshire discovered this pear as a wilding and brought it to America in 1797 or 1799. Bartlett is a large, light-yellow pear and is the first to appear on our markets during the summer.

Continental origin of several other pears is evident from either their present or original names. Anjou (originally Beurre d'Anjou) is an old French

pear with origin unknown. It is an excellent fall variety with a slightly astringent "tang." Comice (Doyenne du Comice) originated as a seedling. Beurre Bosc (now called Bosc) and Winter Nellis were both raised from seed in Belgium.

Our first typically American pear was the Seckel. Most of us remember this small, sweet, red-cheeked pear from our school days. Six or eight of these small fruits could be carried in our pockets at one time. The story of the origin of the Seckel pear is as unique as that of any other thus far unfolded. Toward the end of the eighteenth century there lived in Philadelphia a well-known sportsman and cattle dealer. He was known by the name of "Dutch Jacob." Each autumn Dutch Jacob would bring exceedingly delicious pears back from his hunting trips and would would distribute them to his neighbors. The exact location of the pear tree was known only to the huntsman. Soon a tract of land south of Philadelphia was sold in parcels and Dutch Jacob was one of the purchasers. As a result of his purchase he became the owner of the land where his favorite pear was growing. This was near the Delaware River. The land subsequently became the property of a Mr. Seckel, who gave the pear its present name. What about the origin of the pear? Your guess is as good as mine.

The Washington Navel Orange. In 1896 the Reverend F. J. C. Schneider, the first Presbyterian missionary to Bahia, Brazil, wrote to the U. S. Commissioner of Agriculture, telling him about an orange, grown locally, which might have commercial possibilities in the United States. This was the Navel orange. It probably originated as a bud sport. At any rate the orange appears to have been propagated between 1810 and 1820 by a Portuguese who lived at Cabulla, a suburb of Bahia, Brazil. It was here that the Reverend Schneider first saw the Navel orange.

After an unsuccessful attempt to ship bud-wood to the United States, a shipment of twelve budded trees was forwarded to the U. S. Department of Agriculture in Washington, D. C. They arrived in good condition in 1870. Other trees were propagated from these, and two of them were sent to Mrs. Luther C. Tibbetts, in Riverside, California. Mrs. Tibbetts grew the trees in her backyard. It is from these two trees that the Navel orange in-

Irrigation of a grape vineyard south of Fresno, California, from the Friant-Kern Canal.

dustry of California, and mainly of the whole world, has developed.

This Navel orange has had a number of "first" names. After its introduction into the United States it was called the "Bahia Navel." Later it acquired the name of the "Washington Navel," because of its having been propagated first in Washington, D. C. Next it was the "Riverside." Now it is once more called by horticulturists the Washington Navel, although tradespeople usually refer to it as the California Navel orange. This is not surprising since California supplies most of the navel oranges which we see in our markets. Florida-grown Navel oranges are quite popular with the frozen- or cold-section trade. Fruit sections are peeled for salads and shipped in gallon containers to the larger markets.

Seedless Grapefruit. It is strange how we show a preference for seedless varieties of fruits. The fruit merchant, realizing this, usually advertises his "seedless grapefruit" with large placards; but if he is selling the Duncan "seedy" type, he is more than likely to avoid any reference to internal quality. The housewife, too, prefers to serve seedless grapefruit. What if the seedy types of grapefruit do possess better quality—more sugar, more vitamins, and more minerals—than the seedless type? It seems that most of us still do not like to cut out seeds before serving and therefore, because of its seedlessness, the Marsh grapefruit eclipses all other varieties.

In 1862 William Hancock, of Socrum, near Lakeland, Florida, purchased a farm from one Mrs. Rushing, who is credited with having set out three seedling grapefruit trees. One of these trees bore seedless fruits and thus became the parent of the Marsh Seedless. Bud-wood was purchased by E. H. Tison, a nurseryman, who subsequently sold his nursery to C. M. Marsh, another nurseryman. The latter then marketed it as the Marsh seedless grapefruit. It is believed by some that the Marsh grapefruit originated from a "root-sprout" of an old tree which normally produced seedy fruits. Others still adhere to the theory that this variety originated as a seedling. Whatever the case, the Marsh grapefruit is now grown extensively in Florida, Texas, California, Arizona, South Africa, Israel, Australia, and South America.

Pink Grapefruit. At Manavista, on the Gulf Coast of Florida, may be found the Atwood Groves. If nothing unusual had ever happened to these groves they would still be famous because of the fact that the rows of grapefruit trees are a mile long. But something more important did happen in this grove.

In the citrus season of 1906–07 R. B. Foster was foreman of the Atwood Grove. Mr. Foster was sampling grapefruit and, when he cut open a certain one, he found to his surprise that it was pink. Other fruits on this one branch of the tree were also pink. *One branch on one tree in a grove of thousands of grapefruit trees bore pink grapefruit.* It is not surprising that Mr. Foster marked this particular branch with a large "P" (to indicate pink) in order that he might not miss it when returning. It should be mentioned that the original tree on which the pink-fleshed fruit was discovered was the Walters variety, a pale-fleshed, seedy fruit. This newly discovered pink grapefruit was therefore seedy too.

In 1913, in the Thompson Groves at Oneco, Florida (not too far from the Atwood Groves), a bud sport was found in a Marsh seedless tree. The fruit in this case was pink and seedless. Thus originated the Thompson Seedless grapefruit. This variety is pink fleshed and seedless.

The trees of Thompson (or Pink Marsh) grapefruit were sold to Albert E. Henninger of McAllen, Texas, in1926. In 1929 a new red-fleshed variety originated as a bud sport on one of these Thompson trees in Texas. Henninger called this new sport the "Ruby" and obtained a patent on the fruit. This was the first citrus fruit to be patented. The Ruby grapefruit is grown extensively in Texas, and it is mistakenly believed by many that all pink-fleshed grapefruit originated in that state.

The Temple Orange. No connoisseur of citrus fruits speaks of quality in oranges without mentioning the Temple variety. More and more this particular orange is becoming a familiar sight on the markets—deep orange in color, oblate to spherical, the peel slightly pebbled. To the layman it is often a "kid-glove" orange, for there is much in it that resembles the tangerine. The aroma and flavor of the orange-colored flesh can be described only as superlatively delicious—for want of a more descriptive term.

According to Webber and Batchelor the original Temple orange now stands in the old homestead of William Chase Temple in Winter Park, Florida. But according to T. Ralph Robinson, this tree in Winter Park was budded from the *true* original tree which is in Oviedo, about twenty miles away. The bud-wood for the tree in Oviedo was imported from Jamaica. And this is as far as we can go in depicting the origin of the Temple.

The characteristics of the Temple orange certainly suggest that it is a hybrid because it is part like a Mandarin, such as the Tangerine, and part like the common sweet orange. However, since it

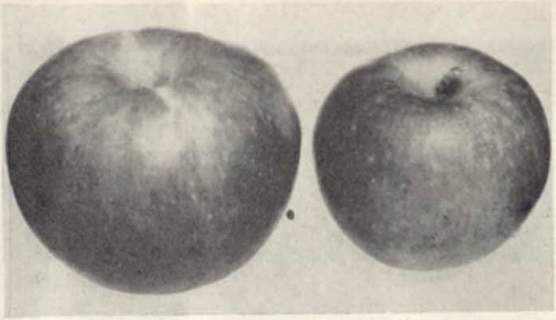
is susceptible to scab (and the sweet orange is not), it may have resulted from a cross between the Tangerine and the sour or Seville orange. But no Tangerine ever had enough sugar to overcome the acid normally found in a sour orange, so what tended to sweeten this offspring of the sweet and sour fruits one will never know.

It may be interesting to the readers to learn that there is a limb on the original Temple tree which regularly produces "navel" fruits, that is, Navel Temples. This may account for the fact that many of the Temple oranges in commerce show a protruding Navel, much like California Navel oranges.

Many other common varieties of citrus fruits appear to have originated by chance. Among the oranges, Boone's Early, Parson Brown, Conner's Seedless, and Pineapple originated as seedlings, and Hamlin and Enterprise are believed to belong in this category. The most popular Tangerine, the Dancy, began as a seedling. The same is true of the Eureka lemon, which is the most extensively grown variety of lemon in California.

It will be observed that a number of the varieties of fruits which we have listed are still in use. This is particularly true of the tree fruits (apples, peaches, pears, and citrus). One reason for this is that a much longer period is required to bring a fruit tree into bearing than is required for small fruits and vegetables. A plant breeder may spend years merely culling out undesirable crosses in his program. The hybridization of citrus fruits is fraught with still another difficulty. The seed of an orange or grapefruit may produce more than one seedling. Both the ovule and the nucellus may give rise to a new plant upon germination. The shoot springing from the fertilized ovule is a true hybrid, but that which originates in the nucellus is merely a vegetative sprout possessing only the characteristics of the female parent.

It is not the purpose of this paper to belittle the excellent work of plant breeders in this country who have introduced so many new and desirable types of plants. Our object has been to show how nature's guiding hand supplied us with many of our fruits until plant scientists were able to take over. Research workers in state and federal agricultural experiment stations and private institutions are performing an excellent service and many new types of fruit are now in commercial production. According to Darrow, 312 varieties of small fruits were introduced in this country between 1920 and 1950 and 98 of these are of some importance. By 1950 new varieties which were originated by government agencies alone accounted for about 55 per cent of the entire strawberry crop, 95 per cent of the red raspberries, 50 per cent of the purple raspberries, 30 per cent of the black raspberries, 5 per cent of the blackberries, 95 per cent of the blueberries, and 2 per cent of the grape crop. Some of the new varieties of peaches are now well established in trade channels and are rapidly becoming favorites among consumers. Other examples might be cited here if space permitted.



Doubling the chromosome number is one way to increase the size of fruit. On the right is an ordinary diploid Winesap apple. On the left is a giant apple chimera composed mostly of tetraploid tissue inside and diploid tissue on the outside; it was produced by Dr. Haig Dermen of the U.S.D.A. Plant Industry Station at Beltsville, Maryland. From it a completely tetraploid plant, usable in apple breeding, may be produced. (U. S. Bureau of Plant Industry.)

It should be borne in mind that our present mode of life has placed much greater demands upon the plant breeder than existed fifty years ago. Fruits of good desert quality and attractive appearance may be satisfactory for home consumption, but they may not tolerate shipment to distant markets or they may not be suitable for canning, dehydrating, or freezing. In fact it is now realized that no one variety can be expected to meet all these requirements and that we must look for a type for each specific purpose.

It is surprising therefore, that so many of the older varieties of fruits have stood the test of time so well. They represent a challenge to modern plant breeders and offer mute evidence of nature's handiwork.

Erston V. Miller

Department of Biological Sciences
University of Pittsburgh
Pittsburgh, Pennsylvania

References

Bailey, L. H. *The Standard Cyclopedia of Horticulture.* New York: Macmillan (1935).

Beach, S. A. Apples of New York. 2 Vol. N. Y. Agr. Expt. Sta. Rept. (1903).

Darrow, G. M. Breeding of Small Fruits in the United States. *Sci. Monthly,* 75, (5), 288 (1952).

Hedrick, U. P. *Cyclopedia of Hardy Fruits*. New York: Macmillan (1922).
Hedrick, U. P. The Grapes of New York. N. Y. State Dept. of Agr. 15th Ann. Rept. (1908).
Hedrick, U. P. The Cherries of New York. Dept. of N. Y. Agr. Expt. Sta. Rept. (1914).
Hedrick, U. P. The Pears of New York. N. Y. State Dept. of Agr. Ann. Rept. (1921).
Hedrick, U. P. The Small Fruits of New York. N. Y. State Agr. Expt. Rept. for 1925.
Robinson, T. R. The Bud-Sport Origin of a New Pink-Fleshed Grapefruit in Florida. *J. Heredity*, 12, (5), 195 (1921).
Robinson, T. R. The Origin of the Temple Orange. Proceedings of the Florida State Horticultural Society, 1945. pp. 3–5. (1946).
Smock, R. M., and Neubert, A. M. *Apples and Apple Products*. New York: Interscience (1950).
Webber, H. J., and Batchelor, L. D. The Citrus Industry. Vol. 1. History, Botany and Breeding. Berkeley and Los Angeles, Calif.: University of California Press (1943).

THE IMPACT OF CHEMISTRY ON THE WORLD OF SCIENCE*

MANY things may be learned from the meetings of scientific organizations where men bring the fruits of their labors and hold them up for all to see. From these meetings one thought recurs: research has come a long way in a relatively few years. Yet the active researcher who knows what riddles are still unsolved, who sees how much more needs to be done, and who senses that what will be done will dwarf what has been accomplished is impatient with anyone who pauses to look back. Although the accomplishments are impressive, the eager researchers are right—there is still a long way to go.

An association like the American Association for the Advancement of Science numbers among its membership men of all sciences. A good proportion are university men and staff members of research foundations. Others are the applied scientists, the individual and specialized vertebrae of the scientific backbone of industry. They first concern themselves with problems of pure science. They are the miners, the diggers who unearth nuggets of bright new truths from the mountain of phenomena that surrounds our existence. They are the searchers for the needle of causality in the haystacks of empirical knowledge. They are the architects of theories and the clever builders of natural laws. The scientists of industry, the applied scientists, use their time and their wit putting science to work. They are the goldsmiths who fashion the nuggets of pure science into useful or beautiful artifacts—the processes, the services, and the products—that insulate and move mankind away from the state of uncivilized nature—in which Hegel said life was nasty, brutish, and short.

* Based on an address given before Section C, at the Annual Meeting of the American Association for the Advancement of Science, St. Louis, Missouri, December 29, 1952.

It would seem that an association such as the AAAS might be a living nucleus for germinating a plan of integration among all sciences represented in the association. Out of such an integration, we might indeed see a most impressive and productive alliance cooperating to what certainly is a worthy end—a coordinated assault on the problems that attend putting science to work for mankind.

In the last twenty-five years the need for scientists in industry has mushroomed at a fantastic rate. This was inevitable for several reasons. Industry has expanded; the products of industry have become more complex; and, under the system of competition that prevails, management has seen the value of research and how well the logic of the scientific method and the data of the laboratory can serve the problems of manufacturing. Coupled with the interruption of training because of World War II and the indiscriminate attrition of scientific people by the needs of a garrison state, we are suffering an acute shortage of scientifically trained people. This shortage threatens to become disturbingly acute before it has any tangible hope of relief.

From time to time over the past few years, I have wondered whether members of the fraternity of pure scientists might not feel a touch of resentment because industrial employment makes such inroads upon the limited supply of scientific manpower. Each feels his own task and his own dedication to be supremely important. The depth of devotion of these earnest researchers for fundamental knowledge can be measured by their tireless persistency, by the years they spend accumulating their data, testing and retesting their facts, and refining their theories. This singleness of purpose marks the true research mind. The goals of the researchers so fill their lives that virtually every

other activity shrinks in proportion. Can anyone blame them if they have only shallow curiosity about unanswered questions outside their immediate inquiry and short patience with problems unrelated to their specialty? Hubble, the eminent astronomer, expressed this perfectly when he found to his chagrin that he was unable to answer a question about a planet in our solar system. He said, "I have commuted to the spiral nebulae so long that I have forgotten the suburban stations."

Yet applied industrial research need make no apology for its contribution to the growth of scientific knowledge. The sole difference, in fact, between the two fields of endeavor is the aim of the research worker. Both fields use identical methods of inquiry. As Weidlein and Hamor have remarked, "An investigator in pure science purposes only to advance knowledge, whereas an investigator in applied science desires primarily to contribute to industrial progress." The growing body of scientific knowledge that is swelling our libraries and trade journals is gaining nourishment from both the work of pure scientists and industrial scientists. Industrial research programs have expanded so much it may be said that the healthy, twentieth-century body of science is the result of a vigorous symbiosis between pure and applied research.

There is no question that within the various scientific disciplines fundamental knowledge derived from pure research is the parent of the accomplishments of the applied scientists. The evolution of each of the sciences follows a similar pattern as it develops. First, definition of the field and its limits; next, codification of data; then, the refinement of measurement; and finally, application of findings. In astronomy it can be traced as the emergence of the science of astronomy from the melange of astrology, the development of more precise measurement, on to its application to navigation, to accurate time measurement, and ultimately perhaps to prediction of weather changes and climatic cycles.

In chemistry, the pattern is much the same. Its earliest beginnings were rooted in what today we wonder at as the weird art of alchemy. Chemistry, in its primitive beginnings, was the empirical manipulation of completely misunderstood materials of nature. Then fundamental research discovered the basic elements, and codification proceeded. Fundamental research discovered the law of combining weights, Boyle's law and Charles' law. With this basic information, chemistry began its growth. Vast development of applied chemistry, however, could not begin until it teamed with engineering—so that its products could be made for mass use and made dependably uniform. When this level was reached, the science of applied chemistry was in position to take the findings of pure science and translate the knowledge into products or processes for use and to refine these end products for maximum effectiveness and to broaden their application to the widest possible usefulness.

Would any pure scientists contradict the statement that applied chemistry has tremendously benefited, not only the human race, but even the other sciences as well?

The science of chemistry started out as the groping, empirical manipulation of almost completely misunderstood materials found in nature. It proceeded to the refinement and duplication of materials found in nature. Over the past few decades, it has started the creation of a variety of improvements on nature, and herein lies the most profound promise, the most dignified and humanitarian significance of applied chemistry.

The contribution of chemistry to the advancement of science may be likened to a path through a labyrinth which has corridors that wind through all the specialties and through all the industrial arts. Science is knowledge; therefore, each chemical product, process, or application technique that adds to man's total body of knowledge is a step forward. Chemistry's contribution to other sciences is also a vast region. To paraphrase John Donne: No science is an island unto itself but all are part of the main. None of the physical sciences can disclaim its debt to mathematics, nor to one another. Advances in physics, chemistry, biology, and medicine codevelop, move forward on stepping stones which frequently are created by the findings of another science.

It is most astonishing to trace the interweaving of chemicals and chemical techniques among particular branches of science and throughout the industrial arts. Industrial chemists were called upon during World War II to help operate the chemical phase of the atomic energy project at Oak Ridge, Tennessee. Certainly, the engineering and operation of the first plant to manufacture fissionable materials was in itself a significant contribution to science by industrial chemists; but, the peculiar nature of the chemical products that have been developed gives us a radioactive tracer to determine how this particular piece of chemical engineering has contributed to the advance in other scientific fields. These contributions have been wholly peaceful and beneficial.

The radioisotopes that are produced as by-products of nuclear fission are chemical products. They are materials not present in nature. They are not pushed into service with the same aggressiveness and sales promotion that assist the distribution and expand the use of other chemical products. Despite this, merely the quantitative production and the known availability of the radioisotopes have started a bewildering variety of research projects that some day will have very practical and beneficial applications.

The studies are amazingly diversified. One project is studying the sterilization of food in closed containers by subjecting the packaged foods to radiation that penetrates tinplate, glass, and paper. Hermetically packaged foods, some day in the future, may be radiated instead of heated, refrigerated, or chemically preserved.

Biologists are using radiocarbon in the study of photosynthesis. Radiomanganese, radioboron, and radiocobalt have been impressed into the scientific study of the molecular structure of steel alloys, thus giving metallurgists accurate pictures of the crystal structure of metals.

Radioactive compounds are making significant headway in medicine. Among these is radioiodine used in the treatment of the thyroid gland and in cancer research. Perhaps the latter application holds the most dramatic interest. The most encouraging results to date have come from the work on blood cancers, where differential absorption of radiophosphorus is localizing treatment, and in cancer diagnosis, where radiation signals the presence of hidden tumors.

Still other uses of radioisotopes have developed in the field of antistatic agents, in gages for controlling the thickness of paper, sheet metal, and rubber. At Mound Laboratory, Miamisburg, Ohio, research is being carried out on radioactive wastes, safety measures, and other topics assigned by the AEC. The chemical researchers there have developed a microtorsion balance so sensitive that it will weigh quantities as small as one microgram, approximately one twenty-eight millionth of an ounce. Here is a most significant instrument, developed in applied chemical research, that can serve in a number of other sciences. The design of the Mound instrument is based on a balance, developed in the Metallurgy Laboratory at the University of Chicago, that operated on the torsion of quartz fibers. In the laboratory at Mound, the chemists made major improvements in the methods of drawing uniform quartz fibers and fusing them into delicate beam assemblies. The balancing of the load is measured on the twist of a quartz fiber which can indicate one twenty-thousandth of a revolution of arc. The instrument has been built and is in operation. Now it is available for any kind of microchemical or microbiological research.

Radioisotopes are unique in the history of chemistry as examples of useful materials that are serving many of the sciences. They are without question the most spectacular of the materials on which chemistry has presumed to make an improvement on nature. But there are others. In the field of plastics and resins, a whole new department of creative chemistry is unfolding. Polymerization and condensation reactions have already produced three dozen commercially important polymers, copolymers, and resins. These materials run the gamut of properties which were formerly possessed only by diverse materials found in nature. They offer transparency, lightness, insulating properties, controlled flexural strength, and impact strength. They can be hard, they can be flexible, they can be elastomeric. Plastics have put at man's disposal new methods of manufacture: molding, extruding, and laminating. As little as fifteen years ago, plastics materials were unimportant in the lives of average people. Today they touch many of the sciences and contribute to work-a-day living almost every hour of the day.

Consider the growing wonder-boy of science, electronics. Here, too, plastics have made a contribution that is small but unique. The new and tiny transistors and the fixed circuits which are embedded in plastic are an example. Electrical engineers know that the little bead that holds the wire terminals to the crystal of germanium and the plastic that permanently embeds a whole amplifying circuit, for example, must be very special materials. These plastics were developed by industrial chemists, and they were ready for use when electronic scientists wanted them.

The next few years will see a phenomenal development of chemically made materials. One of these will be the ployester-glass fiber laminates which can be formed by low-pressure lamination into a variety of large products and large product sections. Even now these materials are making radar domes, automobile bodies, storage tanks, noncorrosive pipe, electrical panels, and even sail and motor boats. They are translucent, weatherproof, nonconductive, and chemically resistant. With special resins, they can withstand heat up to 500° F. They have tremendous impact and flexural strength, and are ready for use by the electronics industry, for signal equipment, circuit panels, and

housings. There is a long list of other applications where metal, wood, glass, or ordinary plastics will not serve. They will also serve agriculture, construction, and chemical engineering. These low-pressure laminates will soon be recognized in many fields as a real contribution to the materials available to scientists and inventors.

Closely akin to plastics is the development of synthetic fibers. This phase of industrial chemistry is hard to keep in sober perspective. From Chardonnet silk to nylon; from Italian staple made of casein to Orlon, Acrilan, and their brethren—the industrial chemist went from a transformation of natural cellulose and protein materials to a complete divorce from nature even as a supplier of raw materials! Just as nylon has superseded silk in most of its qualities as well as in price, so too will the wool-like staples of today be thoroughly acceptable improvements for the best that nature can supply.

While synthetic, wool-type fibers are making a place for themselves today in blends with natural wool and other fibers, their significance is far greater than the addition of another product to commerce. We are now operating purely on cost, under rules of a science we call economics. In time, the discipline we call economy will have to give way to ecology. Then, need will demand that we make the most efficient use of our resources—cost then will not be of major importance. When that time comes, the applied chemist's ability to make suprasilk, suprawool, and perhaps supracotton fiber will be given its just due.

In the years ahead, chemistry must contribute enormously and effectively to the science of food production. The present losses inflicted on growing crops by insects and weeds is appalling. It has been estimated that insects alone destroy as high as four billion dollars worth of crops annually. Plant fungi and other plant diseases destroy a like amount. Weeds, which rob the soil of nutrients, choke crops, clog irrigation ditches, and poison farm animals, cost farmers five billion dollars yearly. If these estimates are somewhere near right, there is a thirteen-billion-dollar loss in food and fiber products annually, an amount equal to 42 per cent of the 1950 value of crops grown. There is proof that this loss is preventable, but the problem goes beyond preventing losses, important as they are.

If population in the nation continues to increase at its present rate—an increase of more than two million a year—by 1975 there will be at least 25 per cent more people to feed and clothe, that is, there will then be five for every four now living. This extra 25 per cent must be fed three times a day with food that must be raised on the farms. It can be estimated roughly that it will take 15 billion more eggs a year, 20 million more hogs, and another 10 billion quarts of milk—about 30 million extra quarts a day—to keep the nation eating as well as it does today. To help them meet the demand the farmers are going to look to the science of chemistry.

That research will be effective, there is little doubt; but how efficient will it be, considering the lack of so much fundamental data? The successes chemists have had to date have been largely the result of persistent, empirical groping. The try-and-see techniques, however, have brought to light some rather wonderful products. In the field of insecticides, chemicals range from DDT through organic phosphates. There are roughly twenty-five commercially important insecticides, all largely unrelated chemically. They kill insects, certain ones, but there is need for more data on why and how.

As far as the chemist is concerned, perhaps the most intriguing developments among the chemical bug weapons are the systemic pesticides that the industry is studying carefully. These insecticides are compounds that plants can absorb through the foliage or roots and that make the plant itself toxic to insects. In effect, these compounds make the whole plant an insect killer. Perhaps this promises help with the problem that is beginning to plague—bugs are apparently developing immunity to some of the known insecticides. Witness the housefly's growing contempt for DDT. If the promise is fulfilled, why could not plants be made immune to blights, rusts, and other fungus attack as well? To make this possible, a vast amount of fundamental knowledge about the life processes of plants is needed. Meanwhile, systemic insecticides and the methods of using them are the subject of research and are being developed empirically.

Chemical soil conditioners are another achievement of applied research. They will contribute enormously to the science of agriculture. From the standpoint of specific research, here are some possibilities that are opened up by this discovery. With synthetically made soil conditioners, biologists can study more carefully the relationship of organic and inorganic fertilizing materials as each contributes to plant growth. Since the soil conditioner adds no food materials to soil and yet creates the loose, porous structure once obtainable only with the polysaccharides and polyuronides derived from decaying organic matter, it is now possible to measure the relative efficiency of plant foods obtained from manures

and plant residues, and plant foods obtained from mineral fertilizers. Synthetic soil conditioners open another avenue of research that can lead to more knowledge about the function of particular kinds of soil organisms. By supplying researchers with a soil conditioning agent that is absolutely sterile and inert to microorganisms, biologists can now more easily study the relationship between soil bacteria activity and its relation to plant growth and mineral nutrients in well-aerated soil.

In weed control, too, significant progress is made slowly year after year. Not too many years ago farmers had to rely on manual cultivation or oils, rock salt, or chlorates to burn above-ground growth. Today the hormone type herbicides which kill the entire plant, including the roots, are well along in their development. The commonest of these, of course, is 2,4-D. These translocated type herbicides have been of inestimable value in controlling perennial weeds and in destroying prolific annuals before they reach the seed stage.

Since there is not too much fundamental knowledge of why the hormone type weed killers work as they do, chemical weed control develops roughly along the following lines. A herbicidal compound is discovered; it is rough screened to determine on what types of weeds it is most effective and what types of crops will tolerate it. Economical and easy-to-use formulations are developed from its chemical characteristics. These are widely field tested at varying rates on a variety of weeds, and by this time a list of weeds resistant to its effects has started to grow. The compound is tried in combination with other herbicidal materials. Research then comes up, empirically, with an altogether different type of compound that shows promise of being effective on weeds that the first type does not control. The whole process is then repeated.

This pattern has been painstakingly enacted in industrial research for the past six years. At the outset 2,4-D proved to be extremely effective on broad-leaved weeds, but its effect on woody plants was not satisfactory from an economics viewpoint. Industry soon developed volume production of 2,4,5-T, another empirical hormone type herbicide that is highly successful on woody plants. It was shown conclusively in 1951 that this compound used with 2,4-D could conceivably eradicate the mesquite which has been progressively invading the grazing lands of the west and southwest since the turn of the century.

In time, the chemical industry may be able to supply a whole arsenal of herbicidal chemicals and techniques for using them that will destroy all the common species of weeds. This arsenal could be developed more quickly and more effectively if more is known about the life processes of plants.

Plant fungi are another chapter in the story. Fungus diseases ruin an estimated four billion dollars worth of fruit, grains, and fiber each year. A little headway is being made, but not much. Apple scab can be controlled and so can cherry leaf spot. Plant fungicides have increased the output of Anjou pears by an estimated three hundred million boxes a year. However, the surface has hardly been scratched in controlling the bewildering variety of rusts, blights, smuts, and rots that attack roots, stems, leaves, and fruit. There is a gleam of light in the darkness—the use of antibiotics in agriculture. It was reported recently that aureomycin, terramycin, and streptomycin proved toxic to the microorganisms responsible for halo blight on beans.

The empirical approach of applied science is dangerously slow, considering the seriousness of the problem. The potato famine of Ireland in 1845 and 1846 reduced the population of that potato-economy-society from 9 million to 6.5 million in 6 years. What was the fungus and how can it be fought? It took 100 years to make any progress, and even today knowledge regarding it is limited. In 1950 Chile had its entire potato crop wiped out by the same disease.

The problem that faces both pure scientists and applied scientists reduces to the question of how to secure greater productivity from every available acre with the least amount of human labor, and, then, how can the products be best protected fully until they are ultimately consumed?

This problem could be paraphrased to cover stock raising. Concomitant with upgrading herds and flocks by improving strains, there is an increase in productivity through scientifically fortified feeds. Today, the chemical industry is supplying pure phosphate mineral supplements, antibiotics, choline chloride, amino acids, and vitamin B_{12} to the makers of stock feeds. It soon may be supplying special wetting agents that preliminary studies have shown may increase feed efficiency.

The following figures will show what these chemical products contribute. In production of broilers, in 1941 it required twelve weeks to produce a bird weighing 3.83 pounds. To do this it took 3.50 pounds of feed per pound of meat. Today, with fortified feeds, a bird of substantially the same weight can be produced in ten weeks and requires only 2.87 pounds of feed per pound of meat. Rounded off, fortified feeds make it possible to

produce a three-pound fryer in nine weeks with 9 pounds of feed, whereas it formerly required twelve to fourteen weeks and 12 pounds of feed.

Similar remarkable results can be cited for cattle feeding, for milk production from dairy herds, and pig feeding. For example, it used to take an average of three hundred days to produce 100 pounds of pork, live weight on the hoof, and required 12 bushels of corn. Today, with fortified feeds, 100 pounds of live weight pork can be produced with $5\frac{1}{2}$ bushels of corn and 45 pounds of feed supplements in approximately sixty days.

The longer one is in the business of making chemicals, the more one is amazed by the breadth and variety of their application in the numerous practical sciences we call American industry. There are over 500,000 organic compounds known today and in a few more years this figure will be close to a million. Industrial research teams constantly prepare new chemical compounds, usually working with compounds that are intermediates in the established lines of products. As a result, a steady flow of completely new chemicals stream out of industrial laboratories. Many times there is not the faintest idea as to what they can do. They may be valuable medicinals, plasticizers, corrosion inhibitors, herbicides, insecticides, emulsifiers, or may have any one of the uses in the whole gamut of applied chemistry.

The advertisement that heralded the early mass production of silicones epitomizes the problem of a new compound. The advertisement said in effect, "What can you do with bouncing putty?" Many uses have developed for this compound. One of the major uses is not too much of a direct contribution to science—but bouncing putty it seems is an excellent center core for golf balls.

In the past year laboratories have synthesized a large number of new compounds. About one-third of these appear to have promise for further work, but of this one-third only one out of seven ever emerges as having commercial possibilities. These new chemicals are screened for familiar uses, but the aid of researchers in other industries is also enlisted. Samples are sent to the pharmaceutical industry, the dye industry, the paper industry, and to other chemical manufacturers for evaluation in their particular fields. Frequently these new compounds find a use in other chemical manufacturing as a stepping stone—what is called an intermediate—to production of another chemical product. Sometimes a demand for the chemical compounds grows without knowledge of the use to which the purchasers put them. From time to time while one group is working largely in the dark to find a use for a compound it is in a good position to make, some others develop an unusual and profitable use that was completely overlooked by the first group.

The industrial research man is faced with so many unknowns that block his progress that he may at times question the free and untrammeled pure researcher in some of his choice of projects. Perhaps this is a perennial dilemma. While the value of academic freedom is recognized, many problems of a basic nature that are simply crying for answers must be faced. The answers can only come from researchers in pure science.

If the magnitude of the job ahead is taken into account, it may be wise for the university men who lead graduates into thesis work and the research administrators in pure science to ask two questions:

"Is this project truly a work that has significance, or is it simply a challenge to the ingenuity of my specialty? How does this inquiry rate in the value of its findings when measured against the pressing need for basic information in the field of applied science?"

Scientific progress will accelerate primarily because each new advance establishes another point of departure for what is to follow. The chemical industry makes its greatest contribution in providing new tools for scientists in medicine, public health, agriculture, and other callings. These chemical tools may have tremendously broad applications across a variety of sciences—such as those described in discussing the radiosotopes. Or they may make a relatively selective contribution as in the case of the plastic used in transistors and fixed circuits. Nevertheless, in the exciting era that is opening, chemistry will have a prime role in the developments that will service the improvement of twentieth century food, clothing, and housing.

The chemical industry will even have a place in the sciences that are extending man's senses, carrying his voice around the world, extending his sight and hearing beyond the horizon, and increasing his calculating abilities beyond Euclid's wildest imaginings.

With radar, servomechanisms, and electronic brains, no one will deny that we are coming within reach of many of our wildest dreams. Certainly no one will deny that the chemical industry will produce the materials out of which these dreams will be made!

CARROLL A. HOCHWALT

Monsanto Chemical Company
St. Louis, Missouri

BOOK REVIEWS

SCIENTIFIC VOCABULARY

An Explaining and Pronouncing Dictionary of Scientific and Technical Words. W. E. Flood and Michael West. London, New York: Longmans, Green, 1952. viii + 397 pp. Illus. $2.25.

ONE of the difficulties that haunts makers of dictionaries is that of defining a word in language which can be understood by persons who need the definition but have only a limited vocabulary. The difficulty is increased if the word is in a technical area in which both vocabulary and experience are lacking. For this reason a serious attempt by qualified scholars to explain scientific and technical terms to laymen in a particular field deserves notice and commendation.

Such is the *Explaining and Pronouncing Dictionary of Scientific and Technical Words* prepared by Flood and West. The work includes "10,000 scientific and technical words in 50 subjects explained as to a person who has little or no knowledge of the particular subject." Explanations are often accompanied by pictures and diagrams, a total of 1300, we are told. Though explanations are designed to be "of such length as is necessary to give an idea of the meaning," the book is not an encyclopedia. The explaining vocabulary is limited to about 2000 words, 56 of these are "definitely technical" (such as, ampere, electron, hydrocarbon, protein, and spectrum). "About 120 others might be unknown to a child or English-speaking foreigner."

The following explanation of "absolute zero" in contrast with the definition in an unabridged dictionary will illustrate what the authors have tried to do.

Flood and West: "0° C. on a centigrade thermometer is the temperature at which water freezes; but ice has some heat; e.g. ice is hotter that liquid air. At absolute zero there is no movement of the molecules and so no heat at all; −273.13° centigrade." (There is a cross-reference to K degrees.)

An unabridged dictionary: "*Physics,* the beginning, or zero point, in the scale of absolute temperature. It is equivalent to −273.1° centigrade or −459.6° Fahrenheit, and is the temperature, never attained, corresponding to entire absence of heat."

The definition of Flood and West illustrates the help which may be given by a simple background explanation, viz., even frozen objects have some heat, and also the difficulty of knowing what to include in the explanation. The phrase "no movement of the molecules and so no heat at all" probably suggests a need of more information than can be assumed for the nontechnical reader.

It would be easy to find fault and even to poke fun at some of the explanations. For example, yeast is said to be a "living fungus-like substance used to make the holes in bread. . . ." (The definition continues but without further reference to bread.) To poke fun, however, would suggest that the reviewer has little insight into or appreciation of the difficulty of the authors' task. It is more appropriate to praise the work as a significant effort to explain technical terms to a nontechnical audience. The book should be helpful to students who have limited scientific training or, as in the case of foreign students, who have a limited English vocabulary. It should be an inspiration to those who by profession must teach the meanings of words. One of the authors, Michael West, has had an especially long and distinguished record in helping people to understand language and to use it effectively.

HERSCHEL T. MANUEL
Testing and Guidance Bureau
The University of Texas

GAMES

Introduction to the Theory of Games. (The Rand Series). J. C. C. McKinsey. New York: McGraw-Hill, 1952. x + 371 pp. Illus. $6.50.

PROFESSOR MCKINSEY has written the first systematic textbook on the Theory of Games since Von Neumann and Morgenstern brought out their original work on the subject. Starting with rectangular games for two players where each has a unique optimum strategy, the general theory of such games is developed, reference being made to graphical and approximate methods. More general two-person games are next considered, and their graphical representation and the concept of information sets are introduced. The consideration is first confined to the case where a finite number of strategies is available and then to the case of infinitely many strategies. Two chapters of necessary specialized mathematics—Distribution Functions and Stieltjes Integrals—follow, the latter being based on Widder's treatment. The way has now been paved for the introduction of continuous two-person games and the notions of "separability" and "convexity" of the "payoff" function. Applications of the theory to Statistical Inference and Linear Programming are next illustrated. The final chapters of the book consider the theory of n-person games, first with the zero sum restriction and finally without it. The book ends with a short chapter describing several important unsolved problems in the theory of games.

Although it is claimed that only a knowledge of advanced calculus and classical algebra is presupposed, it is doubtful whether anyone unfamiliar with certain other branches of mathematics would find this an easy book to read. Certain of the arguments used are of a type best understood by readers with the experience of manipulating inequalities in lattice theory or measure theory. A short summary of matrix properties, which is included, seems to fall between two stools. It is unnecessary for any reader with a basic knowledge of

them, and is too condensed and not in the best logical order of development for anyone unfamiliar with them. The notation used is unnecessarily clumsy in places, e.g., J_3^T to denote the column vector of three elements each unity, and it is not surprising to find a misprint in the midst of it (p. 69, line 4).

Because of the difficulty of certain sections, it is not quite clear for whom this book is best suited. A mathematics graduate wishing to do advanced work in this field would find it an excellent guide to what has been done and what remains to be done. This aspect is strengthened by the comprehensive bibliography of the subject which is included.

The earlier chapters, being an account of work done by others, are a bit stilted in places, and insufficient explanation is given of certain new concepts and symbols; but later the book becomes much more personal and stimulating. It is regretted that answers are not provided to the numerical questions in the sets of exercises which follow each chapter.

A possibly confusing misprint occurs on page 285, where γ appears as δ for half a page.

JAMES R. GRAY
Department of Statistics, United College
University of St. Andrews, Scotland

FOR MATHEMATICIANS

Introduction to the Foundations of Mathematics. Raymond L. Wilder. New York: Wiley, 1952. xiv + 305 pp. $5.75.

PROFESSOR WILDER's book is based on courses of lectures designed to introduce modern ideas on the foundations of mathematics to pure mathematicicans and to specialists in other fields whose work has a mathematical background. It will therefore have a wide appeal, and should prove interesting and instructive to anyone who has ever considered the problems underlying even the most elementary mathematics.

The book consists of two parts. The first is concerned with the material and methods involved in the foundations, and in the second there are descriptions of the various points of view as to the nature of mathematics. Chapters I and II are on the axiomatic approach, the second being devoted mainly to problems of consistency, independence and completeness of sets of axioms; the Law of the Excluded Middle is also discussed. There are several illustrations, some practical and some mathematical. The next three chapters are concerned with the theory of sets, the main emphasis being on infinite sets. The Russell contradiction is introduced at an early stage, and a section on the axiom of choice is included. Countability, the cardinal numbers, well-ordering and the ordinal numbers are all discussed in some detail. No mention is made here of the usual pitfalls in elementary mathematics associated with "infinity" and "infinitesimals." This is unfortunate, as a student suffering from the effects of haphazard teaching (which in this respect is all too common) should have the notions clarified before he starts a study of infinite sets. No doubt Professor Wilder's original students were well-trained, but other people likely to benefit from the book might not be. Chapter VI is on the real number system, and includes a discourse (leaving out many details) on the approach through Peano's axioms. Complex numbers are mentioned very briefly. The first part of the book ends with a chapter on algebra and geometry, including a section on topology. The approach to geometry is through Klein's Erlanger Program, though the author admits that this is now regarded as being inadequate. The definition given of projective geometry is not strictly correct. The transformations used are not, as stated, transformations of a plane, but only of portions of a plane.

The second part of the book begins with a chapter on the history of the foundations of mathematics. Mention is made of the parts played by such mathematicians as Kronecker, Cantor, Boole, Peano, Zermelo, and Poincaré. Then follow three chapters on three distinct schools of thought—the followers of Frege and Russell, the intuitionists, and the formalists. The author presents the different viewpoints in an unbiased manner, mentioning the advantages and disadvantages of each. The final chapter is entitled "The Cultural Setting of Mathematics." Here Wilder examines the position of mathematics in past and present civilizations. This is an interesting chapter, and it makes an excellent conclusion to a book which is rarely dull and frequently stimulating.

The style of writing is pleasing, and the author usually expresses himself clearly. There are several places, however, at which the reader may be misled. For example there is a vague definition on page 15 of the word "corollary," and an illustration is given; but many mathematicians would not call this a corollary, for the proof requires an additional axiom to those used in the proof of the theorem. On page 87, the theorem that the cardinal number of the plane is the same as that of the continuum does not seem to be proved correctly. The correspondence defined by the author does not relate the open unit interval to the open unit square, as he claims. A surprising statement (p. 4 and again on p. 271) is to the effect that Euclid's Elements are still used as textbooks in English schools. This may be true in isolated cases, but it is far from being a general rule. Geometry in English (or, more generally, British) schools is taught mainly from textbooks fashioned after Euclid's style.

The paragraphs and sections have been numbered by means of a decimal system which is much more complicated than is necessary, especially in a book of this kind. More annoying, however, are the footnotes, of which there are far too many. Some of them include quite important statements which might well have been incorporated in the main text. In any case, a superabundance of footnotes tends to make the reading uncomfortable.

However, these shortcomings are only on minor points. On the whole the book is well worth a careful

study, and it makes a pleasant contrast with the usual type of mathematical textbook. It is to be hoped that it will be used, as the author suggests, for courses similar to those he has given at the University of Michigan. The book could not be blamed if such a course did not precipitate much lively discussion.

E. M. PATTERSON
Department of Mathematics, United College
University of St. Andrews, Scotland

INNOVATIONS IN CALCULUS

Calculus, a Modern Approach. Karl Menger. Chicago: Illinois Institute of Technology, 1952. 255 pp.

THIS stimulating book is intended as a textbook in a beginning calculus course, a point of view which will arouse numerous cries of protest as well as a much smaller number of assents. It is the author's thesis that the conventional notation of calculus tends to obscure the essential ideas and make the subject excessively difficult for the beginning student. Professor Menger has systematically developed a notation for functions and their derivatives and integrals that he claims to be superior not only logically but also pedagogically. Whether or not his notational innovations will be accepted in their entirety seems to be doubtful, but certainly many of them are highly worthy of consideration by all teachers and writers. The book is recommended reading for teachers because of its unorthodox point of view and the freshness of its presentation. That the book is convenient to use as a textbook in an elementary class seems questionable but the reviewer is sympathetic with the author's aims and approach and would like very much to try it.

Some particular aspects of Professor Menger's notation are as follows. Instead of denoting a function by $f(x)$, as is common but logically incorrect as the symbol $f(x)$ denotes the value of the function at x, he uses merely the symbol f. The value of f at x then is denoted by fx. Similarly the derivative which is often denoted by $(d/dx)f(x)$ he denotes by Df, the other marks in the conventional symbol being superfluous. Likewise the conventional symbol $\int_a^b f(x)\ dx$ becomes merely $\int_a^b f$. In every case when dealing with functions as such, the symbol for the variable is omitted as being extraneous. This necessitates the introduction of certain new symbols for common functions, for example, the function whose value at x is equal to x is denoted by I and is called the identity function so that what is conventionally written $f(x)=x^3$ becomes $f=I^3$. These changes and others are logical consequences of the five principles of sound notation: (1) where it does not matter what we write, we write nothing at all; (2) what is derived for an arbitrary member of a class must be valid for any particular member; (3) in one sentence or equality the same symbol always has the same significance; (4) in every expression any symbol may be replaced by an equivalent symbol; (5) no unnecessary symbol should be created (Occam's razor), and all necessary ones must be.

As far as pure mathematics is concerned, Menger's innovations are natural and largely free from objection. It is in applications that difficulties appear. For example, if F is a force, then F may be either impulse or work, depending on whether F is regarded as a function of time or distance. This impasse led the author to introduce "descriptive" functions and "denominate" functions. The difficulty in physics, and in all applications, arises from insistence on the use of variables which often obscures the functional relationships involved. Doubtless, in the course of time, the majority of Professor Menger's notational changes will become standard, but only after education of a new generation of physicists and engineers who find the innovations natural. At the present time it is only in the more abstract parts of modern mathematics that one finds notation similar to his proposal. Professor Menger is to be congratulated for producing a stimulating book which should have a permanent effect on the teaching of elementary mathematics.

M. E. SHANKS
Department of Mathematics
Purdue University

PHILOSOPHICAL VIEWPOINT

An Introduction to Mathematical Thought. E. R. Stabler. Cambridge, Mass.: Addison-Wesley, 1953. xviii + 268 pp. $4.50.

A SOUND knowledge of high school algebra and geometry is all the mathematical background required for an understanding of this lucidly written and stimulating book, the nature and purpose of which are described by its author in the following words:

"The chief aim of the book is to provide a unified and substantial approach to the logical structure of mathematics, and to develop a corresponding philosophical point of view toward mathematical knowledge. This is carried out by emphasis both on postulational foundations and on the process of logical reasoning itself, together with applications to science and other fields of thought.

The presentation is introductory and exploratory in nature. It is not intended to supply a technical treatment either of logic, or of the foundations of mathematics."

The text is divided into two parts. In the first part the view that mathematical truths are not absolute but depend ultimately on the acceptance of postulates, definitions, and methods of reasoning is developed and supported by illustrations including Euclidean and non-Euclidean geometries and modulo arithmetics. An evaluation by modern standards of Euclid's elements and an assessment of the use of postulational organization in mathematical and non-mathematical fields is followed by an outline of those aspects of modern symbolic logic which pertain especially to mathematics.

This is followed by a discussion of scientific method, scientific theories, and the differences between scientific and mathematical truth.

The second part is considerably more advanced mathematically than the first. In it are developed, rigorously and at some length, important algebraic systems including groups, rings, fields, Boolean algebras, lattices, and also a finite geometry. Some of these are used to illustrate such properties of sets of postulates as consistency, independence, completeness, weakness, and categoricalness. The postulational systems of Hilbert, Huntington, and Peano, concerning, respectively, Euclidean geometry, complex numbers, and natural numbers are discussed, though not in full detail. There is also a brief description of the Whitehead-Russell, formalist and intuitionist approaches to the foundations of mathematics.

The text, which includes numerous exercises, is arranged and presented so as to make it possible to base various types of courses, with different levels of difficulty, on selected chapters, or sections within chapters. Schemes for general education courses, background courses for prospective teachers of secondary school mathematics, and introductory courses for specialists in pure mathematics are suggested by the author.

On the whole, this book gives an excellent and reasonably comprehensive yet elementary account of axiomatics and axiomatic systems. The author, however, does not attempt more than a cursory discussion of the role of definition and construction in the development of mathematics; for an adequate treatment of such topics as the Dedekind (or Cantor) definition of real numbers and, more generally, the foundations of analysis, he refers the reader to appropriate sources of information.

A bibliography lists all books and articles referred to in the text and some selected additional references.

D. Borwein

Department of Mathematics, United College
University of St. Andrews, Scotland

BRIEFLY REVIEWED

Possums. Carl G. Hartmann. Austin: University of Texas Press, 1952. x + 174 pp. Illus. $6.00.

THIS is the kind of book which can only be written by an able observer who has studied his subject over a long period of time, and who is literate and humorous as well as learned. Its essence is best summarized in a quotation from the end of Chapter Five: "In this chapter I have more or less taken the animal apart for closer analysis—which is one way of studying an animal. . . . I am not unmindful, however, of the oneness of a living organism and the advantages of studying a representative of a species of animal as a whole. . . ." The result is an absorbing account of "what manner of beast" the possum really is.

Essentially it is a series of essays on various aspects of possums. We are introduced to them, as the European community was, through the descriptions and pictures of the early sixteenth century. The wealth of legend and folklore that existed then and still exists today is thoroughly and entertainingly explored. A chapter on taxonomy is followed by a discussion of the possum's physical characteristics. Its habitat and behavior, both legendary and real, are discussed. Quite a large section of the book is devoted to the author's special field, reproduction. The scope of the work is, however, far wider than this summary would imply. The amount and variety of historical and comparative material included make most of the chapters of very general interest. The reader learns much about the early naturalists and is furnished with many a shrewd and wise comment on the history and procedures of animal observation and investigation. Each chapter is, in a sense, complete in itself. This has led to a certain amount of repetition, particularly of historical data, but in no way to detract from the value of the book as a whole. It also makes for a certain unevenness, depending on what is known about a chosen topic and on how much the author himself has explored it in detail. Thus, in the chapter on "playing possum," the comparisons of related phenomena are fascinating but one could wish to know more of the actual processes involved. The excellent photographs and reproductions of early plates with which the book is liberally illustrated add greatly to its interest and value. A fairly extensive bibliography seems somewhat less critically chosen.

Barbara Lawrence

Museum of Comparative Zoology
Harvard University

The Human Senses. Frank A. Geldard. + 365 pp. Illus. $5.00. New York: Wiley. 1953.

FROM his extensive experience in psychophysiology, Professor Geldard has drawn together into a single compact volume a remarkably unified presentation of man's senses. Almost half of the book deals with vision and hearing; the remainder covers the many senses in the skin, kinesthetic and organic sensibilities, functions of the labyrinth, smell, and taste. In each instance the nature of the stimulus is considered, the type of receptors involved, methods of measurement in each specialized field, thresholds, differential abilities, effects of mixtures, electrophysiological approaches, and theories relevant to the subject. Reference is made throughout to a carefully selected bibliography of 330 entries. Stress is placed on the importance of the senses in human engineering, particularly in reference to Air Force problems in World War II. As a result, the book is both wonderfully informative and interesting reading.

Lorus J. Milne
Margery J. Milne

Department of Zoology
University of New Hampshire

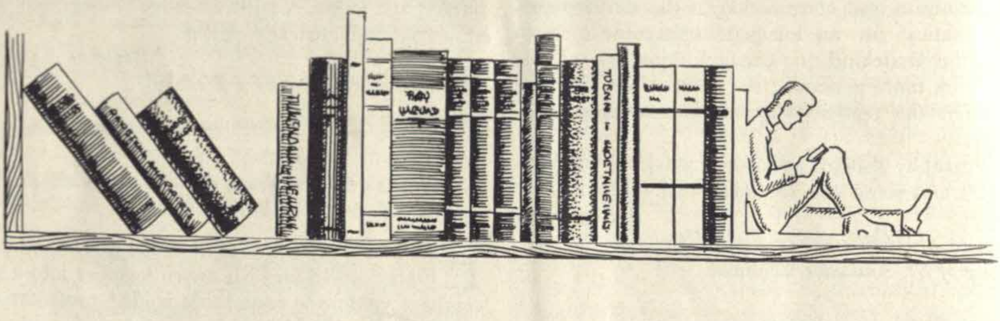
Methods and Principles of Systematic Zoology. Ernst Mayr, E. Gorton Linsley, and Robert L. Usinger. New York–London: McGraw-Hill, 1953. 328 pp. Illus. $6.00.

SYSTEMATICS is the branch of zoology (or botany) concerned with classification, especially the classification of individual species and genera. This work in systematics is conducted according to a rather rigid set of rules, whose interpretation and application often require the talents of a Philadelphia lawyer. It is this aspect of systematics, the restricted field of taxonomic (i.e., nomenclatural) procedure, that often irritates the bystander, to whom it appears to be a pointless game of name-changing. Although this book is intended for students and practitioners of systematics, it should also be welcomed by such innocent bystanders, who ought to consult it before penning an indignant letter to SCIENCE when some familiar name has been changed "for no apparent reason."

The broader aspects of systematics are far more interesting, and deserve to be more widely known. One of the most useful chapters of this book is that concerned with quantitative methods; together with the chapters on taxonomic characters and their discrimination it makes up the heart of the book. While this "manual of taxonomy" comes no nearer to defining a species than did Darwin, it does show what sort of information may be extracted from adequate samples or collections and indicates that taxonomy may be returning to favor in our graduate schools after a whilom absence from felicity.

JOEL W. HEDGPETH

Scripps Institution of Oceanography
University of California, La Jolla



LETTERS

THE EDUCATORS' DEBATE

It was a pleasure to read Professor Trow's moderate article on education (Scientific Monthly, 76, 149 [1953]). The constructive approach, and avoidance of a negative attitude, are most welcome in this controversial field! I do wish, however, that he had disposed of "Bestor's questionnaires" more adequately, applying the same constructive approach here as in the rest of the article. Clearly, he doesn't regard the questionnaires as characteristic: "Isn't it a rather dubious procedure to select two studies . . . and imply that they are typical? . . . The selection of data to fit one's thesis, with the neglect or suppression of contrary data, is a practice I am sure neither of you would follow in your own disciplines. Why do it when you step out of your field? . . ." I agree fully, but can't help feeling that this is negative criticism of Bestor's argument; it is not constructive.

What do I mean by a "constructive approach," in this connection? I mean simply exhibiting some widely circulated questionnaires, sponsored by professional educators, in which the intellectual disciplines receive the weight which is their due. I supposed all along that educators would disapprove of the emphasis in the Illinois study, just as other scholars do, and this expectation is confirmed by Professor Trow's article. The following is perhaps the sort of questionnaire that Professor Bestor would like to see—and Professor Trow, too, if one may extrapolate from his disapproval of the other. Such a poll, sponsored by educators, would give convincing evidence that the Illinois affair is really nontypical.—To get to the point: Could Professor Trow describe some questionnaires of the following sort that have been circulated by his profession? And what was the public response?

1. Are students taught to handle English well, in their writing and speech?
2. Should they be?
3. Are they encouraged to develop a good vocabulary?
4. Does the public school give its students a sound knowledge of American literature?
5. Of English literature?
6. Does it enable them to enjoy and appreciate such literature?
7. Is public school education in French (or other modern languages) adequate for the needs of one traveling abroad?
8. Does it give any significant acquaintance with good French (or other foreign) literature?
9. On the whole, do you consider instruction in handwriting better now than it was thirty years ago?
10. Spelling?
11. Reading?
12. Arithmetic?
13. Geography?
14. Grammar?
15. History?
16. Is the American history now taught adequate for present-day requirements of good citizenship?
17. Same, foreign history?
18. Does the school give its students a good understanding of the great ancient civilizations?
19. Should it?
20. Does the school help the student to know and enjoy good music?
21. Same, art?
22. Generally speaking, would you say that instruction in high school mathematics (algebra, geometry, trigonometry) is as good here as in England (or Germany, or France)?
23. Is an acquaintance with the exact sciences desirable for citizens of the present day?
24. Do most students obtain a fair knowledge of chemistry before leaving high school?
25. Of physics?
26. Of mathematics?
27. Does the present educational system develop the ability to concentrate and study effectively?
28. To analyze complex intellectual situations?
29. Are these abilities important in the modern world?
30. Do the schools make a serious effort to discover and develop our more gifted children?
31. Do students of elementary schools, in general, get as much "intellectual fare" as they should?
32. Same, high school?
33. Does the psychological atmosphere of our schools foster a respect for intellectual pursuits?

R. M. Redheffer

Department of Mathematics
University of California, Los Angeles

This letter concerns the article by Dr. Trow in the March 1953 issue of The Scientific Monthly. I doff my hat to him as a shrewd debater, but I do not find any plain refutations of the charges of Bestor and Fuller. He instead relies, in the first instance, upon attempts of defamation of character, and his use of the attack oblique is worthy of study. For example, in the third paragraph, he links Prof. Bestor with those "attacking the public school—seeking to destroy it." Not a direct accusation but a supposedly clever casting of doubt upon Prof. Bestor's motives. Instead of impugning motives, Dr. Trow's letter would be much more effective if he had, if possible, cited facts to negate the exhibits quoted by Prof. Bestor.

The case is understated in the fifth paragraph. Sometimes it seems the "educators" (such as our Regents in Albany) have their heads so far in the clouds that they have entirely forgotten the basic processes of teaching and learning.

It is quite true there are incompetents in theory and practice in other fields but they are soon recognized as such. A teacher in engineering, say, cannot hide very long an ignorance of the fundamental principles and their application. An engineer or a doctor or a mason or a carpenter soon has his ability assessed by colleagues and public and receives his reward in proportion. But a professor of education or a state school system under control of these "progressive" (?) educators is not readily accessible to the public, which in truth, pays for both. The deterious effects of the progressive system are beginning to be seen now. They have been increasing rapidly the past five years or so, but the public opinion is a slow maturing thing and the damage will continue until the public will becomes vociferous enough to bring back to our schools the ideas of intellectual disciplines so badly needed. Bestor, Fuller, Lynd are the prophets going before, and the progressive educators would do well to check their retirement income plans.

We readily concede the first of your "facts." The second can be amplified to point out that genuine education would seem to have as only one part of it, "intellectual training." It also has "physical training" and "moral training." It would seem also that the school is primarily concerned with the intellectual part of this training; it may be involved in the other two as being parts of the "whole man" but these should be incidental, not major, parts of the school program, and subordinate in every way in the normal school to the "intellectual" concern.

Trow's profound statement that half the people in the world are below average in intelligence, coupled with his subsequent remarks, insinuates that these people could be taught how to control a 200 H.P. automobile even though they could not necessarily add up a grocery bill. No wonder the accident rate and insurance rates continue to climb! It is not the purpose of the schools to train young people to meet "life" problems. These are probems of the home and the church. Our schools are set up, as our pioneer schools were, to teach children reading, writing, and arithmetic. Their results along these lines have been increasingly worse, particularly in recent years under the guidance of "progressive" educators. And until they can show themselves capable of training children in the basic skills, they should not be allowed to dabble in the high-sounding "meeting life" theories.

As for interpreting results, we may point to the yearly college aptitude tests. We claim to select our students from the top fifth. This appears fine but the joker is that the educators who run these tests apparently are not able to (or do not wish to) correlate the tests year by year. Consequently, it has been freely admitted that this top fifth is progressively poorer each year. The result is a demand that the colleges should reduce their entrance requirements, that students should be led more easily into the rigor of college work. Our colleges did not, at one time, have to offer remedial reading courses; now more than half of our freshmen need them and we wish such courses could be made "required." And why is it that matriculants have so little knowledge of what "study" means? Why is it that so many of them cannot write a sentence without a mistake in syntax or grammar or spelling? Our schools proclaim "self-expression" for the children; when do they learn to express themselves in speech and writing? Surely "self-expression" has no connection with untrammelled freedom of action.

Instead of the clever fending Trow indulges in, let us have some straight answers to these straight questions. If the public school educators cannot think of an out, let us get our schools back to the "intellectual training" so lightly regarded by Dr. Trow. Then when the faults implied in these questions are corrected, then only let the educators, by express consent of the parents and taxpayers, indulge in courses in basket-weaving, auto-driving and enriched verbiage.

P. D. Tuttle

School of Electrical Engineering
Cornell University

On first reading Professor Tuttle's letter, I did not think he could be serious, but a re-reading made me think that perhaps he was not indulging in a satirical piece after all. If he is serious, his statements are so fantastic and so emotionally charged that they hardly call for reply, though I would be glad to comment on them specifically should he so desire.

I do wish to clarify two points however. The first is that I had no intention of engaging in the "guilt by association" technique, but instead I wanted to point out the unfortunate fact that those who are actual enemies of the public schools do use some of the less carefully guarded statements of those who are not.

The second relates to his statements beginning: "It is not the purpose of the schools. . . . Our schools are set up. . . ." These, of course, are not statements of fact but of Professor Tuttle's opinion. Fortunately there are many other equally competent people who hold different opinions.

Professor Redheffer's suggested questions reveal that he recognizes some of the complexities involved in the problem of adopting the school program to the needs of the children and of the culture. It would be advantageous if we knew the answers to some of his questions, many of which would be helpful in aiding a particular school in evaluating its own program.

But the questions would be difficult to answer for schools in general partly because of the wide diversity in practice, and partly because many of the questions call for factual answers rather than opinions. For example, citizens generally have no basis for answering questions as to whether instruction in arithmetic or any other subject in American schools is "adequate," or whether it is better than it was thirty years ago. One could find out how many students take chemistry in high school, but how do the school patrons decide what is "a fair knowledge" of chemistry?

Those interested in the problem of the use of public opinion surveys in improving instruction (and I hope Professor Tuttle and other Bestor protagonists will do so before engaging in further public controversy) should send to the National Association of Secondary School Principals (1201 Sixteenth St., N.W., Washington 6, D. C.) for the *Bulletin* entitled, "A Scholar's Documents," by Professors Harold C. Hand and Charles W. Sanford. This bulletin, among other matters deals with Professor Redheffer's inquiry about questionnaire studies. It points out (pp. 476–477) that whereas Professor Bestor referred only to the so-called "Follow-up Study," of the holding power of the high schools, he made no mention of six other studies which are also in progress in Illinois. One of these is the series called "The Local Area Consensus Studies." These latter consist of twenty "local action projects . . . one for each subject. . . . Among these subject fields are art, English, foreign languages, mathematics, music, science, and social studies." Quoting further from the bulletin: Each of these questionnaire and interview studies "is or will be designed to enable local patrons, teachers, and older pupils to consider the following three things:

(1) What the purposes or objectives of the subject or service in question should be in the local high school.

(2) Which of these desired purposes are, and which are not, currently being achieved to an adequate degree.

(3) What should be done in the local high school to accomplish those of the desired purposes which are not being adequately achieved."

The Hand and Sanford bulletin goes on to show how the materials for these local studies are constructed taking mathematics as an example. Such studies as these are indicative of one kind of approach to the question raised by Professor Redheffer. There are, of course, others.

WM. CLARK TROW

School of Education
University of Michigan

LIBERALISM VS. LIBERALISM

FROM time to time over the last two years you have published articles by members of the faculty of the University of Illinois on matters of educational policy in the elementary and secondary schools. It appears that the current controversy over liberalism vs. traditionalism in American education has reached a level of bitterness in Illinois which most other sections of the country have thus far avoided.

One need not object to emotionalism in argument to disapprove the inclusion of such argument in the official magazine of American science. Whatever the merits of the issue, Messrs. Fuller, Bestor, and now Cairns do themselves no credit as scientists in the utterances of theirs which you have published.

I assume we are agreed that dispassionate inquiry into any area of intellectual interest is the very keystone of the scientific method. A dispassionate evaluation of the writings of the gentlemen to whom reference is made will, I believe, reveal little of the scientist's balanced unemotionalism in their theses.

As a scientist and educator I protest the inclusion in THE SCIENTIFIC MONTHLY of genuinely unscientific material.

IRVING C. WHITTEMORE

Washington, D. C.

A TUTELO CHIEF JOINS HIS ANCESTORS

THE March issue of SCIENTIFIC MONTHLY brought an article on the Tutelo Harvest Rite, which had been recorded by Chief Peter Buck. As a dramatic sequel the author received the following notice:

March 18, 1953

Dear Friend,

With extreme sorrow we inform you of the passing of my father, Chief Peter Buck, after a short illness. He died on Sunday March 15th at the age of 71 years. Was laid to rest at Onondagas on Tuesday March 17th.

We are sorry we were unable to let you know sooner.

Sincerely
Roy Buck and Family
and
Mrs. Eliza Buck

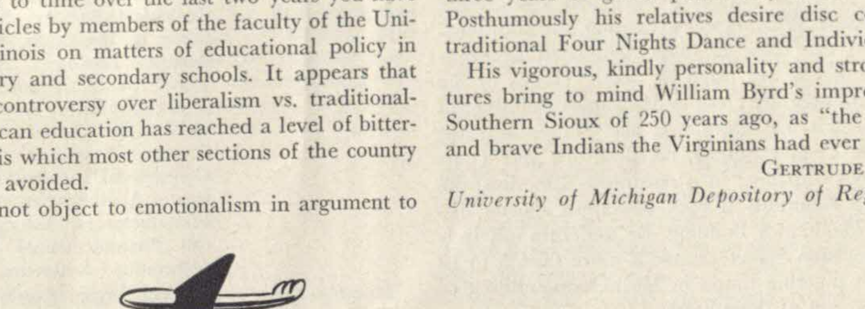
Two days later a letter from a close relative of his included a confirmation, " . . . and he passed away on Sunday morning on 15th. It's a real loss to our longhouse, he knew so many sacred songs. So we'll have a dress up dance, all-night feast for him on Tuesday, Tutelo rites. Too bad you cannot be here."

A conservative ritualist, Peter Buck confined his singing to the actual rituals. He recorded sparingly, after three years of gentle persuasion, in his last summer. Posthumously his relatives desire disc copies of his traditional Four Nights Dance and Individual Chants.

His vigorous, kindly personality and strong lean features bring to mind William Byrd's impression of the Southern Sioux of 250 years ago, as "the most honest and brave Indians the Virginians had ever known."

GERTRUDE P. KURATH

University of Michigan Depository of Regional Music



ASSOCIATION AFFAIRS

HOTEL HEADQUARTERS, SEVENTH BOSTON MEETING, DECEMBER 26–31, 1953

THE preliminary announcement of the Seventh Boston Meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE (SCIENCE, **117**, 613 [1953]; THE SCIENTIFIC MONTHLY, **76**, 315 [1953]), which was principally concerned with the programs of this year's 120th Meeting, named the headquarters hotels of the AAAS and the zoological societies (Statler), of the geneticists (Sheraton Plaza), and of the science teaching societies (Bradford). The detailed list of the headquarters hotels of all participating societies and of each AAAS section was scheduled to appear at this time when data on hotel rates could also be announced.

As in recent years, the center of the Association's annual meeting is a large convention hall—this year, the Mechanics Building—which houses the Main Registration-Information Center, the Visible Directory of Registrants, the Annual Exposition of Science and Industry, the AAAS Office, and the AAAS Science Theatre. In this building will be held the two general symposia of the Association, nearly all of the programs of the 18 sections of the Association—e.g., physics, chemistry, geology and geography, botany, psychology, the social sciences, engineering, and medical sciences (including medicine, dentistry, and pharmacy)—the illustrated lecture of the National Geographic Society, important conferences on scientific editorial problems and scientific manpower, and the Biologists' Smoker.

As usual, the sessions of the participating societies, principally, will be in four hotels chosen for their adequacy and relative convenience to the Mechanics Building, which is at 111 Huntington Avenue, two blocks west of Copley Square. The basic pattern of the session arrangements for the participating societies is the logical one of grouping specialists and closely related organizations in the same hotel to ensure maximum convenience for those attending concurrent sessions and similar programs. With respect to housing, the additional hotels named are close to those designated as headquarters. All hotels are listed by *zones*: Hotels of the Copley Square area are close to Mechanics Building; of the hotels which are downtown, the Statler is three blocks east of Copley Square, the Bradford and Touraine two blocks further; in a northwest direction, the Somerset and Kenmore in Back Bay are a shorter distance from Mechanics Building. By underground trolleys along Boylston Street, no one is more than 5 to 10 minutes from meeting rooms in Mechanics Building or from the extremes of these hotels.

The hotels of Boston have agreed to provide ample housing at moderate rates. Housing will be handled by the well-directed and efficient Boston Convention Bureau. Hotel room assignment-confirmation slips are typed in quadruplicate, and one of these will be sent directly to the applicant. Those who apply early are assured the hotel of their first choice, but it must be remembered that the supply of single rooms at minimum rates is always relatively limited—it pays to apply early for them. Higher priced singles and double rooms for single occupancy are more plentiful. It is desirable that *maximum rate* be stated, as well as desired rate, on application forms. Expenses can always be reduced if rooms or suites are shared with one or more colleagues or friends. Special attention is called to dormitory accommodations—3 to 5 comfortable rollaway beds in a large room with private bath—at \$2.75 to \$3.00 per person per night.

Beginning with this issue, the advertising section of THE SCIENTIFIC MONTHLY will carry page announcements of the hotels and their current rate schedules, together with a coupon which should be filled out and sent, ***not to a hotel,*** but to the AAAS Housing Bureau in Boston. Applications for hotel reservations will be filled in the order of their receipt.

Individuals of course have freedom of choice of hotels, but, in stating their preference, the desired rate, and the maximum rate, it is natural that those who apply latest may not secure their first choice of hotels. Usually a person's preference is for the hotel that has been named as headquarters for his section or society, but the hotels in the same zone are nearby. For the convenience of those planning to attend the 120th Meeting of the Association, the headquarters for the 18 sections and subsections and the participating societies are listed here:

Hotel Headquarters

Downtown Zone

Statler (1,300 rooms) Park Square — AAAS; Press; AAAS Sections C, F, I, Nm, Nd, Np; Alpha Chi Sigma; American Society of Zoologists, Herpetologists League, Massachusetts Zoological Society, Society for the Study of Evolution, Society of Systematic Zoology; Alpha Epsilon Delta, American Association of Hospital Consultants, American Institute of Nutrition, American Physiological Society; American Association of Colleges of Pharmacy, American College of Apothecaries, American Drug Manufacturers Association, American Pharmaceutical Manufacturers Association, American Pharmaceutical Association, American Society of Hospital Pharmacists; American Book Publishers Council, American Textbook Publishers Institute, Conference on Scientific Editorial Problems, National Association of Science Writers, Scientific Research Society of America, Society of the Sigma Xi, United Chapters of Phi Beta Kappa.

Bradford
(400 rooms)
275 Tremont St.

Academy Conference; AAAS Cooperative Committee on the Teaching of Science and Mathematics; AAAS Section Q; National Speleological Society; American Nature Study Society, National Association of Biology Teachers, National Science Teachers Association.

Touraine
(200 rooms)
62 Boylston St.

Parker House
(700 rooms)
60 School St.

Copley Square Zone

Sheraton Plaza
(500 rooms)
Copley Square

AAAS Sections G, H, L, O; American Eugenics Society, American Society of Human Genetics, American Society of Naturalists, Beta Beta Beta, Ecological Society of America, Genetics Society of America; American Society of Plant Physiologists, New England Section; History of Science Society, Institute for the Unity of Science, Philosophy of Science Association.

Copley Square
(124 rooms)
47 Huntington Ave.

Lenox
(175 rooms)
61 Exeter St.

Vendome
(300 rooms)
160 Commonwealth Ave.

Back Bay Zone

Somerset
(500 rooms)
400 Commonwealth Ave.

AAAS Sections A, B, D, E, K, M, P; American Meteorological Society; Association of American Geographers, Geological Society of America, National Geographic Society; National Academy of Economics and Political Science, Pi Gamma Mu; American Industrial Hygiene Association, Society for Industrial Microbiology; American Geophysical Union, Conference on Scientific Manpower.

Kenmore
(400 rooms)
490 Commonwealth Ave.

Advance Registration and Advance Copies of the General Program-Directory

As in past years, those who plan to attend the meeting may register in advance and receive both a Convention Badge and a copy of the General Program-Directory, by first-class mail, early in December. The registration fee of $2.50 includes postage. Those who cannot attend the meeting but would like an advance copy of the General Program-Directory may also obtain it by first-class mail early in December at cost—$1.50. A coupon covering both alternatives will be found on another page in the advertising portion of this issue of THE SCIENTIFIC MONTHLY. The appropriate square should be checked.

Sectional Sessions for Contributed Papers

Another section of the Association—Section L—has requested the publication of a call for contributed papers. The following is a complete list of the 10 AAAS sections which will receive short papers on current research. In general, a brief abstract should accompany each title and these should be sent each section secretary or program chairman, *not later than September 30*—preferably earlier.

C, Chemistry—Dr. Ed. F. Degering, George Washington Inn, New Jersey and C Streets, S.E., Washington, D. C.
E, Geology and Geography—Dr. Jack B. Graham, 3400 North Westmoreland Street, Falls Church, Va.
G, Botanical Sciences—Dr. Stanley A. Cain, School of Natural Resources, University of Michigan, Ann Arbor, Mich.
H, Anthropology—Dr. Gabriel Lasker, Wayne University, 1512 St. Antoine Street, Detroit 26, Mich.
I, Psychology—Dr. William D. Neff, Department of Psychology, University of Chicago, Chicago 37, Ill.
L, History and Philosophy of Science—Dr. Raymond J. Seeger, 4507 Wetherill Rd., Washington 16, D. C.
Nd, Dentistry—Dr. Russell W. Bunting, School of Dentistry, University of Michigan, Ann Arbor, Mich.
Np, Pharmacy—Dr. George F. Archambault, Pharmacy Branch, Division of Hospitals, Federal Security Agency, Public Health Service, Washington 25, D. C.
O, Agriculture—Dr. C. E. Millar, Department of Soil Science, Michigan State College, East Lansing, Mich.
Q, Education—Dr. D. A. Worcester, University of Nebraska, Lincoln, Neb.

RAYMOND L. TAYLOR
Associate Administrative Secretary, AAAS

THE *Theobold Smith Award* of $1000 and a bronze medal, sponsored by the Eli Lilly Company of Indianapolis under the auspices of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, will be given for the ninth time at the Boston meeting in December. The prize is given for "demonstrated research in the field of the medical sciences, taking into consideration independence of thought and originality." Nominations should be sent to Allan D. Bass, Department of Pharmacology, Vanderbilt University School of Medicine, Nashville, Tennessee, before September 15. They should be in triplicate and accompanied by full information concerning the nominee's personality, training, and research work. U. S. citizens less than 35 years of age on Jan. 1, 1953, are eligible. Nominations may be made by AAAS Fellows, and the Vice President for Section N, Medical Science, and four Fellows of the Section will form the committee of award. The last winner was Frank J. Dixon, Jr., of the University of Pittsburgh School of Medicine, for a paper entitled "The Dynamics of Immune Response."

Meetings

July 30–Aug. 1. Wyoming Geological Association. University of Wyoming, Field Conference. Laramie.

Aug. 3–5. Abnormal and Pathological Plant Growth. Brookhaven National Laboratory, Upton, L. I., N. Y.

Aug. 3–8. Photographic Society of America. Los Angeles, Calif.

Aug. 3–8. World Meteorological Organization, Regional Assoc. for North and Central America, First Session. Toronto, Canada.

Aug. 5. Symposium on Macromolecules. Uppsala, Sweden.

Aug. 5–12. International Congress of Zoology. Copenhagen.

Aug. 7–8. Pennsylvania Academy of Science (Summer). Thiel College, Greenville, Pa.

Aug. 9. International Veterinary Congress (15th). Stockholm.

Aug. 10–14. Society of American Bacteriologists (Annual). San Francisco.

Aug. 10–18. American Association of Colleges of Pharmacy (Annual). Salt Lake City, Utah.

Aug. 15–30. Summer Seminar-Workshop in General Semantics (10th). Institute of General Semantics, Lakeville, Conn.

Aug. 16–22. American Pharmaceutical Association. Salt Lake City.

Aug. 16–26. International Conference on Soil Mechanics and Foundation Engineering (3rd). Zurich and Lausanne, Switzerland.

Aug. 17–19. Society of Automotive Engineers (International West Coast Meeting), Georgia Hotel, Vancouver, B. C.

Aug. 18–21. American Institute of Electrical Engineers (Pacific General). Vancouver, B. C.

Aug. 18–21. International Union of Biological Sciences (11th General Assembly). Nice, France.

Aug. 19–21. Wescon (Western Electronic Show and Convention), jointly sponsored by IRE (7th Region) and WCEMA (West Coast Electronic Mfgrs. Assoc.) Municipal Auditorium, San Francisco.

Aug. 20–26. Congrès International de Philosophie. Brussels.

Aug. 20–30. International Congress of Limnology (12th). Cambridge, England.

Aug. 22–25. Joint Commission on High Altitude Research Stations. Boulder, Colo.

Aug. 23–28. American Dietetic Association. Los Angeles, Calif.

Aug. 24–28. International Congress of Rheumatic Diseases. Geneva, Switzerland.

Aug. 24–29. Oak Ridge Summer Symposium (5th Annual). Oak Ridge, Tenn.

Aug. 24–29. World Conference on Medical Education (1st). London.

Aug. 24–31. International Genetics Congress. Bellagio, Italy.

Aug. 26–28. American Mathematical Society (Sixth Symposium in Applied Mathematics). Corona, Calif.

Aug. 26–28. Gerontological Society, Inc. (Annual). San Francisco.

Aug. 28–Sept. 4. International Congress on Tropical Medicine and Malaria. Istanbul, Turkey.

Aug. 30. International Association for Hydraulic Research. Minneapolis, Minn.

Aug. 30–Sept. 1. American Sociological Society (Annual). University of California, Berkeley.

Aug. 30–Sept. 3. International Society of Orthopedics and Traumatology (6th Congress). Bern, Switzerland.

Aug. 31–Sept. 3. American Hospital Association. San Francisco.

Aug. 31–Sept. 4. International Congress of Physiology (Triennial). Montreal.

Aug. 31–Sept. 4. International Physiological Congress. Montreal.

Aug. 31–Sept. 5. American Mathematical Society, Mathematical Association of America, and Canadian Mathematical Congress. Queen's University and Royal Military College, Kingston, Ont., Canada.

Sept. 1–3. Fourth Symposium on Plasticity. Brown University, Providence, R. I.

Sept. 2–9. British Association for the Advancement of Science (Annual). Liverpool.

Sept. 4–9. Psychometric Society (Annual). Michigan State College, Lansing.

Sept. 6–10. American Institute of Biological Sciences. (Annual). University of Wisconsin, Madison.

Sept. 6–10. The Nature Conservancy (with AIBS). Madison, Wis.

Sept. 6–11. American Chemical Society (124th National). Chicago.

Sept. 6–12. Congresso Internazionale di Microbiologia. Rome.

Sept. 8–10. American Society of Limnology and Oceanography (Eastern Section). University of Wisconsin, Madison.

Sept. 9–15. American Meteorological Society, Royal Meteorological Society, Toronto.

Sept. 13–16. Electrochemical Society, Inc. Ocean Terrace Hotel, Wrightsville Beach, N. C.

Sept. 14–17. Illuminating Engineering Society. Commodore Hotel, New York.

Sept. 14–17. Society of American Foresters. Colorado Springs, Colo.

Sept. 14–24. International Instrument Congress and Exposition. Philadelphia.

Sept. 18–27. Congress of the International Scientific Film Association (7th). London.

Sept. 23. American Medical Writers' Association (Annual). Elks' Club, Springfield, Ill.

Sept. 27. American College of Dentists (Annual). Cleveland.

Sept. 28–Oct. 1. American Dental Association (Annual). Cleveland.

Sept. 28–Oct. 3. Alaskan Science Conference. Juneau.

Oct. 3–10. Sixth International Congress of Leprosy. Madrid, Spain.

Oct. 8–9. American Council on Education (Annual). Hotel Statler, Washington, D. C.

Oct. 10–16. American Academy of Ophthalmology and Otolaryngology (Annual). Palmer House, Chicago.

Oct. 15–17. Acoustical Society of America (Annual). Cleveland, Ohio.

Oct. 19–21. Entomological Societies of Canada and British Columbia (Annual, Joint Meeting). Empress Hotel, Victoria, B. C.

Oct. 30–31. Kentucky Academy of Science (Fall). University of Kentucky, Lexington.

THE SCIENTIFIC MONTHLY

AUGUST 1953

The Village in an Industrial World*

RALPH L. BEALS

The author is a native Californian and was educated at the University of California. He received his Ph.D. degree from that university in 1930 and is now chairman of the Department of Anthropology and Sociology, the University of California at Los Angeles. He was honorary professor, Instituto Politecnico, Mexico, 1939, and a member of the Faculty of Medicine, University of Concepción, Chile, 1948. In addition to his university work, Dr. Beals has done extensive editorial work and has been continuously active in many anthropological groups. His principal fields of work include California, Mexico, Arizona, and many South American countries.

LONG experience suggests that an anthropologist must explain himself more fully than must the chemist or the psychologist. For years one of my colleagues has believed my greatest interest to be the collection of arrow heads, while another has tried unsuccessfully to present me with a fossil elephant. Some of my colleagues have wondered how I could bear to live among a lot of dirty natives, while others have envied my opportunities to live among noble and unspoiled savages. Actually, the varied and superficially confusing activities of anthropologists have a very simple common theme. Anthropologists are interested in people, in their past, their present, and their uncertain future.

An interest in people explains the first part of my title, for most people are farmers living in villages. Statistics are poor or nonexistent for many parts of the world, but by the most conservative estimate more than two-thirds of the world's people live in villages; the figure may well be seventy-five or eighty per cent or more. Moreover, the village is a conveniently small unit for the study of many problems of theoretical importance.

Numbers and convenience are not the only reasons to study the village. Today the village world is in a ferment of change under the mounting impact of industrial civilization, and gives rise to some of the most critical problems of our times. The varied conditions of change likewise provide as near an approach to laboratory conditions as the student of man is likely to find. Both the needs of science and urgent practical affairs call for study of the village. It is in this framework that I review my own research in the villages of Latin America.

Whether it be in Latin America, Asia, Africa,

* The field work upon which this paper is based was supported by grants from the Social Science Research Council, the Wenner-Gren Foundation for Anthropological Research, and the Committee on Research of the University of California, Los Angeles.

or elsewhere, village communities untouched by the industrial world tend to have common characteristics in contrast with the city. Most economic life is organized about the production of food. Specialization of labor tends to be by age and sex, and within each family is to be found knowledge of the basic techniques to produce the minimal necessities for food and shelter.

The members of the village community are known to one another. Social relationships tend to be on a face-to-face basis. Chiefs or headmen often are mediators rather than figures of authority. Although conflicts and methods of adjudicating them are always found, social controls tend to be indirect and informal. There is less need for a judge when everyone knows the rules. Social status and prestige are related to family size and standing and individual abilities. The social system tends to be limited to the village, even though integrated into some larger system governing relationships between villages. Religion or ritual affects most aspects of culture. Even if specialists are found, each family head is on occasion his own priest.

The village world tends to be stable in its social structure and adequate in fulfilling its cultural functions. It maintains its membership and satisfies in tolerable measure its basic physiological needs. Both birth rates and death rates are high, but the population tends to be in balance with resources and technology. Demands upon the individual for adjustment are limited to changes in age and status. Child-rearing practices tend to prepare individuals for adult life and to shape personalities to fit the existing social and cultural demands. The way of life appears to the individuals in it, and conditioned by it, to be rational, stable, satisfying; for them it is a proper way of life.

The urban way of life is very different, especially now that industrialism creates ever broader demands for markets and raw materials. Economic life involves far more than the production of food and raw materials. The family no longer is the economic unit, and shrinks toward the parents and dependent children. Labor grows increasingly more specialized and dependent on complex organization. No man can know or comprehend the sources of all the economic goods for which an industrialized society has created secondary needs; men grow increasingly interdependent.

Face-to-face social relationships of importance are no longer with relatives and neighbors, but with chance associates or in the many voluntary associations. Formality and impersonality mark social intercourse, just as formal controls, police, and judiciary replace the more informal machinery of village justice. When no man knows his neighbor, he tries to make him take an oath.

On reviewing my own research in Latin America in retrospect, it is increasingly evident to me that its central problem is the impingement of the urban industrial world upon the village. Among the Yaqui and Mayo of Sonora, the sacred clowns perform traditional antics in the midst of machines. Tractors and irrigation systems compete in interest with the insistent demands of a complex and interdependent social and religious system. In some Tarascan villages in central Mexico half the adult males have worked in the United States, and highways knit once remote villages to urban centers. Among the isolated Mixe of southern Mexico, schools and sewing machines compete for attention, while pre-Columbian rituals that survived centuries of missionary hostility are quietly disappearing. Only one in five Mazatecs speaks Spanish, but in their midst is taking place one of the greatest river development projects in the Western hemisphere.

The impact of contemporary industrial civilization upon the village world is the problem that I propose to explore, a problem of primary importance to us. To demonstrate this, I should like to describe an anthropologist's field trip to the exotically remote Ecuadorian village of Nayon. Here, scarcely six miles from the Equator, one can see in microcosm some of the problems that today face the industrial world and the yet uncounted millions of village dwellers.

The Village of Nayon

The romantic may find it disconcerting to arrive by airplane in Quito, the capital of Ecuador, a rapidly growing city approaching a quarter of a million. But soon the suburbs are passed and one comes unexpectedly to the top of a steep escarpment overlooking one of the great central valleys of the Andes. A thousand feet below lies the village where slightly over 2,000 souls live among their farm lands in the apparent changeless peace of centuries. To the west, Nayon is isolated by the heights from which it is viewed, and in other directions it is separated from its neighbors by deep, rugged barrancas traversed only by rocky trails.

At first glance little has changed or will change here. People are at work in the fields with oxen and wooden beam plow, or simple spade and hoe and machete. An elevation of 8,500 feet provides a mild climate with occasional frosts, despite the nearness of the Equator. Miles across the valley the great eastern range rises in vast bleak *paramos* where sudden blizzards may trap the unwary

traveler. North, east, and south, glacier-crowned volcanos overlook deep barrancas where tropical vegetation flourishes. Warmed by tempered tropical sunshine, its rich volcanic soil watered by 60 inches of rain, its lands relatively free of the dominance of the hacienda, the romantic might expect Nayon to be an idyllic representative of the self-contained independent agricultural village. Nothing could be further from reality, for the industrial-urban world has pressed hard upon it for over a quarter of a century. Let us see what this means.

The people of Nayon are Quechua-speaking Indians, as are their neighbors. Under missionary control after the Spanish conquest, Nayon was organized as a parish. The present townsite is in the typical colonial Spanish grid pattern of rectangular blocks about a central plaza, on which face the church and government offices. About half the population of the parish lives in the townsite. The grid of streets and trails at present includes approximately half of the most fertile land in the parish.

Until about thirty years ago, the village seems to have been a self-sufficient agricultural community with a mixture of native and sixteenth century Spanish customs. Lands were abandoned when too badly eroded. The balance between population and resources provided a minimum subsistence. A few traders exchanged goods between Quito and the villages in the tropical barrancas, all within a radius of ten miles. Houses were dirt-floored, with thatched roofs, and pole walls, sometimes mud plastered. Guinea pigs ran freely about each house and were the main meat source. Most of the population spoke no Spanish. Men wore long hair and ponchos and concerned themselves chiefly with farming. Most formal controls and external relationships were managed by a resident parish priest, but informal leadership and familial controls governed social relationships. From the consolidation of the Spanish conquest until about 1920, no significant change occurred. In short, people lived within an integrated, internally consistent system of social relationships, habits, customs, values, and living techniques which were satisfactorily in balance with ecological conditions, and which supplied the necessary requirements for survival of the society.

Recent Changes in Nayon

The completion of the Guayaquil–Quito railway in 1908 brought the first real contacts with industrial civilization to the high inter-Andean valley. From this event gradually flowed not only technological changes, but new ideas and social institutions. Feudal social relationships no longer seemed right and immutable; medicine and public health improved; elementary education became more common; urban Quito began to expand; and finally—and perhaps least important so far—

Typical vegetation in the barrancas.

Arrival of bus from Quito. Church in background.

One of two remaining old-style houses.

modern industries began to appear, although even now upon a most modest scale.

In 1948–1949, the date of our visit, only two men wore long hair, and only two old-style houses remain. If guinea pigs are kept, they are penned; their flesh is now a luxury food, and beef is the most common meat. Houses are of adobe or fired brick, usually with tile roofs and often containing five or six rooms, some of which have plank or brick floors. Most of the population speaks Spanish. There is no resident priest, but an appointed government official who, with a policeman, represents authority. A six-teacher school provides education. Clothing is becoming city-like; for men it often includes overalls for work, and a tailored suit, white shirt, necktie, and felt hat for trips to Quito. Attendance at church is low and many festivals have been abandoned. Volley ball or soccer is played weekly in the plaza by young men who sometimes wear shorts, blazers, and berets. There are few shops, for most purchases are made in Quito. Much of the farm produce is sold in Quito, and from there comes most of the food, so that there is a far more varied diet than twenty-five years ago. There are piped water and sporadic health services; in addition, most families patronize Quito doctors in emergencies. Since 1949 the road has been paved, and bus service is more regular. There is one reputed millionaire (in sucres, the national currency, and the equivalent of about $150,000 U.S.), and several are classed as wealthy.

Thus, although to the casual observer Nayon still seems a timeless, sleepy farm village, in little more than a quarter century the changes in the direction of what North Americans call progress have been enormous. Let us examine the meaning of these changes in terms of the pattern of living.

Loss of Self-Sufficiency

The changes in housing and clothing mean greater dependence upon the outside world. Masons must be hired to lay adobe or brick or stone walls, and so a new kind of specialist has come into being, one who works for wages. For fine work, outside craftsmen are imported along with such materials as cement and tile. Sewing machines are now considered necessary, although much clothing is purchased.

Except for some new specializations, tools for the primary occupation of farming have undergone little change. The wooden beam plow drawn by oxen, and the dibble, hoe, shovel, mattock, and machete are the universal tools. But the crops and their use have undergone notable change. Maize or Indian corn is still the primary crop, but very little is harvested as grain. Almost all is sold in Quito as green corn to eat boiled on the cob, and a considerable amount of the corn eaten as grain in Nayon is imported. Beans, which do poorly here, are grown on a small scale for household consumption. Though some squash is eaten, most is exported. Sweet potatoes, tomatoes, cabbage, onions, capsicum peppers, and, at lower elevations, sweet yucca or arrowroot are grown extensively for export; indeed, so export-minded is the community that it is almost impossible to buy locally grown produce in the village. People cannot be bothered with retail sales. Although areas devoted to fruit are small, quantities in excess of household needs are sold in Quito. Oxen are kept for plowing, but there is no dairying; milk, if used, is brought from Quito. Donkeys, mules, and horses are kept by some as pack animals. A few people buy shoats and fatten them, but not many pigs are butchered locally; again they are sold in Quito. A few others do the same with cattle, buying in Otavalo to the north, fattening, and then selling in Quito.

Clearly, then, Nayon is no longer a self-sufficient village. It is now deeply enmeshed in the money economy of a larger region, and especially with the city of Quito. The extent of these involvements is perhaps best underscored by the foods imported in significant amounts, as revealed by a thirty-day check of six families. (Table 1). In a thirty-day period these six families purchased outside the village most of their animal proteins, all fats, and a substantial part of their carbohydrates. Moreover, should outside sources be cut off, only the starches

TABLE 1

Foods Imported into Nayon in Significant Quantities

Beef	Milk	Peanuts
Cheese	Bean Flour	Lard
Bread	Spaghetti	Brown sugar
Oatmeal	Some corn	Some fruits
Barley Flour	Potatoes	Chocolate
Rice		

from corn, manioc, sweet potatoes, and squash could be supplied locally. The village shows the dependency, insecurities, and frustrations attendant upon a national and world economic system. Expressed in money values, these six families purchased from thirty-seven per cent to seventy-four per cent of their food. In absolute figures, the least spent in purchasing food supplies was $26.00, the most $60.00. In addition, during the month many meals were purchased away from home.

The sale of most local farm products and the purchase of imported foods, clothing, and building materials gives new importance to transportation. Although many goods still travel by pack animal or by human carrier, increasing amounts are transported by bus and truck so that a major community undertaking was the construction of the road across the rugged and steep hills to Quito.

Changes in Family Life

In the village world the family is characteristically one of the most important social units. Usually the family is extended to include a larger group of relatives than in our society, and close ties exist with related kin groups. People live primarily in contact with relatives. The individual is reared within the family, and it provides his security system and his major social life. Economically, the family tends to be the basic unit of production and consumption.

With these points in mind, the activities of six families were recorded in detail for the period from June 15 to July 15, 1949. In one family, consisting of the parents, an adolescent boy and girl and four children in primary school, the father spent 13 days in Quito during this period and another 13 days away on a trading trip to the Amazonian lowlands. The mother spent 12 days in Quito and four other days away from home on trips. The father was absent for 71 meals out of a possible 90, the mother was absent for 27 meals. The mother spent six days on farming activities during the month, the father none. The goods carried by the father on his trading trip had a wholesale value of over $1,200. Among household expenditures we find nearly $100 spent at one time to provide new outfits for the children in school to wear at the closing exercises at the end of the school year.

In a less active family, specializing in buying and shelling peanuts for the Quito market, the father was absent on business 15 days out of the month, the mother six days. Incompletely recorded figures on gross sales amount to about $2,000 for the month. In a family with older children, specializing in buying and selling fruit, the father was absent 15 days, the mother eight days, and the two older sons and the oldest daughter were absent 19, 14, and 15 days respectively. Other family records are comparable.

These examples suggest how thoroughly many Nayon families are involved in the market economy. Agriculture played but a small part in their activities, although all would describe themselves as farmers. Clearly the social relationships of Nayon are no longer confined to the village. Indeed, impersonality characterizes many of the social contacts within the village. In the detailed schedules, we find only a few visits to relatives and only one visit to a non-relative. Characteristically, people in Nayon do not know what their neighbors are doing; moreover, they have comparatively little interest in knowing. Members of the community are judged on their public conduct, especially in relation to public affairs. Gossip and discussion there is, and opinion may be mobilized for or against individuals, and sharp conflicts exist. But, essentially, social relations at Nayon are impersonal in a form more characteristic of the city than of the village.

A modern house.

Visitors to Nayon showing dress once common in Nayon.

Changing Social Relationships

These attitudes are well illustrated in relations with government, represented primarily by a political agent appointed by the federal authorities. When new laws permitted Indian communities to recommend their own choice of political agent, Nayon immediately closed ranks when its recommendation was ignored. The new political agent contributed to this by unusually arbitrary actions. Consequently, not only did residents cease to make the customary gifts, but all refused to sell him food or services. The owner of the bus line refused him passage. As a result, the political agent had to walk to Quito and back, not only on official business but to purchase all his family's food supplies. As an ultimate outrage to a Latin American male, when the water supply failed he had to carry water from the barranca—an indignity even the foreign ethnographer was spared. But in addition to these essentially village-like procedures, community members kept lists of the fines levied, searched government records to prove that the political agent had not paid the proper amounts into the municipal treasury, and brought charges of embezzlement against him which resulted in his removal from office and replacement by the village nominee. In the same year the village also brought about an inspection trip by the mayor of Quito, pavement of the road, the contribution of materials and engineering supervision for extension of the water system, and the public promise of electric lights as soon as additional transformers became available.

This looks like, and is, unity in action of a very efficient sort. Without going into the complex details involved, the significant point is that the accomplishments were political in nature and were achieved by effective political organization of urban type. Men who saw little of each other under other circumstances—that is, men who were not friends or who might in some respects actively dislike one another—utilized sophisticated political machinery effectively to achieve goals which in considerable part involved the improvement of urban-type facilities. Essentially, their action was that of a neighborhood improvement association in a large city, in marked contrast to the helpless bewilderment and internal bickering found when villages unfamiliar with urban ways are faced with new external problems.

A characteristic of the village is its homogeneity; there are few specialists and most people share the same skills and knowledge. Nayon, once primarily agricultural, is today a village of traders. The examples given are not extreme, for some traders handle single transactions of $6,000 or more. Others travel from Peru to Colombia. In addition, we find scattered individuals who are oil-well riggers, construction workers of at least medium skills, and mechanics. There are also two school teachers. No Nayon youth has yet reached the University, but half a dozen are in advanced technical schools and one is in the Ecuadorian naval academy. And, to keep the picture in some sort of balance, mention should be made of the persistence of older kinds of specialists such as blanket and belt weavers, and the development of a small, essentially landless class

A uniformed policeman represents modern authority.

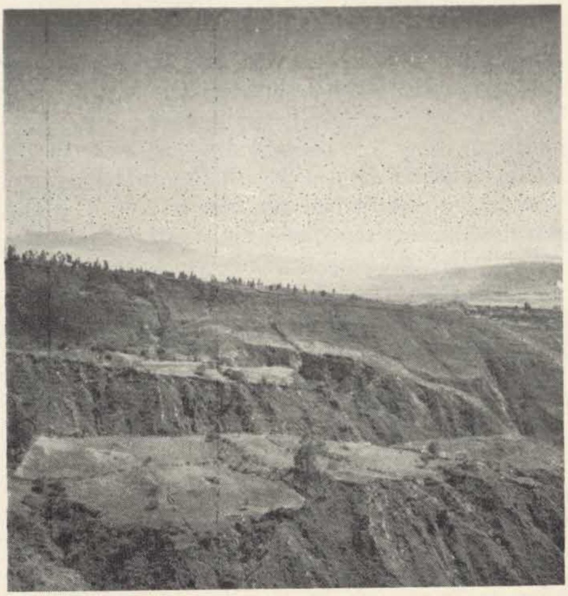
which survives as unskilled laborers, mainly on construction jobs in Quito.

The nature of Nayon religious life and participation has likewise changed. Nayon is still Catholic in the main, and the few who have become Protestants are disliked and distrusted. Indeed, despite the intelligent and wholehearted assistance of the parish priest, the greatest obstacle to securing cooperation for the Nayon study was the persistent suspicion that we were Protestant missionaries. Nevertheless, religious influence is declining in Nayon, especially among the younger people. The parish priest today must care for half a dozen parishes and no longer resides in Nayon. Few, indeed, wish him to. Although mass is held every Sunday, attendance is usually small. Young men rarely participate, although they do not start their Sunday games of volleyball, soccer, or pitching coins until mass is concluded. On occasion though, they have spoken rudely to the priest. Most religious festivals have been abandoned or reduced to celebration of the mass. Those still retained become increasingly secular, with emphasis upon social aspects, drinking, and dancing. Even the festival of the patron saint is a poor thing, shorn of most of its color and traditional folk dances. Religion in Nayon is essentially as formal and as restricted in its function and meaning as in any city.

Penalties of Success

So far, perhaps, this account sounds like a success story entitled, "Indian Village Adapts Rapidly to Modern Life." Many members of the group have

A trail across the north barranca.



View eastward across the village toward the volcano of Pichincha. Eroded and abandoned land in foreground.

accepted major shifts in the socio-cultural system with little difficulty and look forward to additional changes. Most people in Nayon are conscious of change and of its attendant difficulties, and believe they have met the situation well.

But a major finding of modern anthropology is that culturally established technologies, behavior patterns, value systems, and social structures tend to form closely integrated socio-cultural systems. If this be true, have Nayon people understood all the ramifications of change? Has all the population adapted equally well? Let us look for some entries on the debit side. What are the problems Nayon faces, and what are the forces released by its abandonment of a way of life that has been moderately successful for four hundred years? And what has impelled Nayon citizens to make this change? These are questions Nayon shares with the thousands of villages on every continent.

One characteristic of the village world is the existence of a balance between resources and population. Birth rates and death rates are in balance unless nearby cities absorb surplus population. Living standards tend to be relatively unchanging. In Nayon improved health conditions, erosion, and new living standards have between them destroyed this resources-population balance.

On the surface, it seems difficult to maintain that health conditions in Nayon have improved. Inadequate surveys of school children indicate that at least sixty to eighty per cent of the population is infested with one or more intestinal parasites.

Goiter, avitaminoses, and other forms of malnutrition are prevalent. Measles, mumps, and chicken pox are considered normal for all children. Tuberculosis, malaria, and venereal disease are increasing. On the other hand, smallpox and whooping cough are fairly well controlled.

Unsatisfactory as the health situation may appear by our standards, the best test of it is the relationship between births and deaths. From the beginning of the registration of vital statistics in Nayon in 1936 through 1948, there were 287 more births than deaths. This is a population increase of nearly thirty per cent in thirteen years.

Man and the Land

Natural resources have diminished as population has increased. Great gullies are destroying the most fertile land. Sheet erosion has removed all the top soil from large areas, exposing a hard and infertile clay subsoil. In areas under cultivation each torrential tropical rain removes top soil with frightening rapidity; streets are becoming gullies. Consequently, much land has been abandoned, and even the forested areas have been planted recently with the alien eucalyptus.

Diminishing resources and expanding population have caused excessive fragmentation of land holdings. This process is clearly shown in Table 2. In the oldest generation couples owned on the average 9,192 square meters (about two acres) in the central area. They have sixty-four living grandchildren who will inherit on the average 1,757 square meters. Only five of the wealthier grandchildren are married but they already have 32 living children. As additional children will probably be born to these parents, even the most fortunate group of children of the fourth generation probably will inherit less than one-eighth of an acre on the average.

View across a barranca showing islanded remnants of cultivable land.

TABLE 2

LAND HOLDINGS IN NAYON
Central District—12% of Population

Generation	I	II	III	IV
No. Individuals	7	24	64 (some families incomplete)	32
No. married	7	24	7 (5 with children)	0
Average inheritance of married couples (sq. m.)	8,054	4,415	7,189	—
Total land owned, including purchases (sq. m.)	9,192	5,264	Incomplete	Incomplete
Average potential inheritance, not including spouses' land (sq. m.)	—	2,681	1,757	1,485*
Range of actual or potential inheritance (sq. m.)	2,205 to 12,789	—	—	0 to 2,668

* For present offspring of 5 families with children. All families incomplete. All families above average in inheritance.

Another measure of pressures on land is the changing price structure. Although the figures in Table 3 are uncorrected for inflation, they are striking. The greater increase for land near the plaza reflects the adoption of urban ideas; except for the largest cities, residence on the central plaza carries the highest social prestige in Latin America.

These few data make it clear that the traditional balance with the environment has been destroyed at Nayon. Even if improved agricultural techniques and erosion control are introduced, approximately one-half of the Nayon children who reach adulthood must emigrate if the village is to maintain its present relatively low living standards on the basis of agriculture. Moreover, the Nayon standard of living is rising, and aspirations for a continued betterment are strong. Any further improvement of health conditions must mean an even greater emigration rate. Alternatively, other sources of livelihood must be found.

To some degree both of these solutions are already in operation. A few families have emigrated, mostly to the coastal lowland areas now being developed in Ecuador. Others have purchased land elsewhere and may soon migrate. This trend probably will be accelerated. Many have resorted to

TABLE 3

CHANGE IN LAND PRICES IN NAYON

	No. parcels	Average price per square meter (dollars)	Percentage increase above pre-1918 average
Sales before 1918	7	$0.0052	
Sales, 1943–1948, total	7	$0.1521	2,925%
Parcel nearest Plaza		$0.6090	11,711%

more extensive trading and entrepreneurial activities. Others work for wages in the nearby city.

The economic problems created in Nayon by increasing population, diminishing land base, and demands for a rising living standard are worldwide in scope. Whether on Pacific islands, in Africa, or in Asia—wherever modern public health has begun to penetrate, or where outside agencies have put a stop to such population-limiting devices as war, famine, and infanticide—population is pressing hard upon resources and native technology. Everywhere, too, the solutions being tried are creating new social problems, for the fabric of any society and its culture is composed of functionally interrelated parts; alteration of any significant part must affect all the others.

New Social Problems

In Nayon some of the social problems have already been recognized by Nayon residents. Wage labor in the city is clearly seen as leading to personal disintegration, alcoholism, and low living standards. Most parents will make great sacrifices to keep their sons from being forced into wage-work. Perception of other problems is still rare.

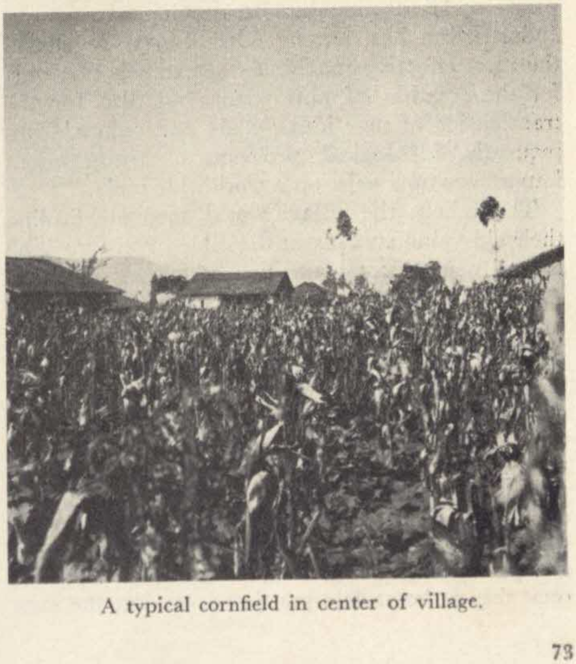
Emigration destroys community ties and further diminishes the importance of the extended family. Moreover, those who emigrate move into new kinds of communities where traditional guides to behavior no longer operate and where greater impersonality characterizes social relationships. The individual is more on his own.

The social effects of trading enterprises are even less clearly seen by the Nayon population. Not only do women take some part in trade, with long absences from home, but they also are carrying on an increasing amount of the agricultural work. Today it is not uncommon to see women plowing—a sight very rare, if not unknown, ten or fifteen years ago. Small children are commonly relegated to the care of older siblings, who at early ages may be in complete charge of the home for large parts of the day. More and more the school is a major factor in the training of the young, and it is inculcating the values of the city rather than of the village. Even in child training, the church now occupies only a minor role in the community.

The long absences of parents and the increasing shift to a money economy result in a great decrease of mutual aid and cooperative activities. The *minga* or cooperative labor for public improvements seems clearly destined to disappear before long. There is still some cooperative work among friends, neighbors, and relatives, but there is a marked decrease in aid that now can be measured in money. Individualism and self-reliance are prized; many will not visit the free health clinic because it is "charity."

Future of the Village

Because of its nearness to Quito, Nayon will soon be engulfed in the expanding and industrializing city. Most of its population either will emigrate to other agricultural zones, enter upon commerce and other urban occupations, or become merged in the urban proletariat. It is conceivable, indeed, that in the not too distant future the leaders in commerce and industry may come from such Indian communities as Nayon. In Ecuador, as in much of the village world, the dominant elite both shun labor as degrading and give low social status to the new occupations of trade and industry. Hardworking, shrewd, and enterprising, the traders of Nayon seem indeed more likely to develop into the leaders of industry in Ecuador than do the members of the entrenched elite. Past history, however, does not suggest that the latter will surrender their positions of power without a struggle.



A typical cornfield in center of village.

The implication of the picture of Nayon that I have presented is that the village will adapt to the industrial-urban society emerging in Ecuador. But problems of individual adjustment and the continued functioning of the village socio-cultural system will become increasingly difficult. The old trading background has made it relatively easy to adjust to trading over a wider area and on a larger scale. If individuals own land they still can pretend they are farmers. However, they are only beginning to recognize the resultant effects on children whose fathers are away for long periods and whose mothers must frequently leave them alone for the better part of many days. Nor, as the population outstrips its land base, are young people deceived. Already some ask: "Without land to be farmers or capital to become traders, what are we to do?" Too educated to accept the role of unskilled laborer in Quito, too little educated for better jobs, still barred as Indians from skilled jobs by the persisting caste system, they go increasingly to distant places, to the coast or to the oil fields, where they may escape identification as Indians.

In my discussion of Nayon I have tended to emphasize technological and material changes and some of their concomitant social changes and problems. It is true that these are more easily observed and presented, but perhaps far more important are shifts in values, attitudes, and ideologies. Herskovits has pointed out that "no economy has been discovered wherein enough goods are produced in enough variety to satisfy the wants of all the members of any society."[1] He might have added that no society, including our own, has exhausted the capacity of all its members to develop new wants. Industrialism has, among other things, developed the most effective machinery yet known not only for the creation of new wants, but also for the transmission of new ideas, goals, and values. Consequently, ideological problems of transcendent importance now exist on a worldwide scale.

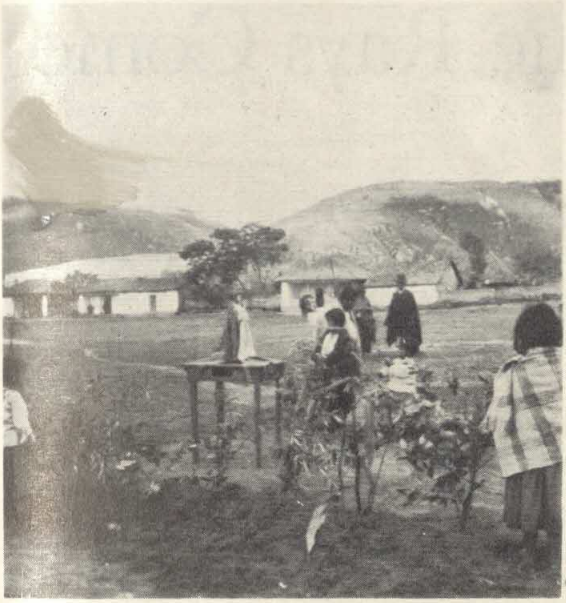
Throughout the village world, men are finding their old value systems unworkable or are groping toward new stystems and new social relationships. As Opler and Singh remark of changes occurring in a village in Northern India:

> When we come to the question of the areas in which the greatest amount of change has taken place, it is our impression that the most extensive and pervasive shifts have occurred in outlook and political and social relations rather than in the realm of technology, possessions, and work habits. The few metal plows, mechanical chaff cutters, improved sickles, and bicycles which have appeared in the village, while important, are, perhaps, more significant as tokens of things to come than as immediate practical assets. But the mass education of "untouchable" children, the introduction of co-education, the tolerance of widow remarriage, the wresting of political power from the land owners and high caste groups by the artisans and lower castes —these are momentous changes that penetrate to the core of village life, and indeed, of Indian life. They bespeak the end of an era and a reorganization of intellectual and social energy.[2]

Group assembled for mass and procession during festival of patron saint.

Nature of Cultural Revolutions

Events in Nayon, then, have significance primarily as an example of the effects of a worldwide cultural revolution in which industrialism is altering the technology of the village. Through its communication systems, industrialism is carrying urban patterns of life and thought into every part of the world, with far-reaching effects upon village social life. The basic processes of this cultural revolution are not new to human history. The village world itself was a product of the Neolithic revolution, some ten thousand years ago, that converted man from a hunter and a gatherer into a village-dwelling food-producer. Like the Neolithic revolution, the urban-industrial complex had its center of origin, its period of growth toward an integrated system, and its subsequent rapid and inexorable spread over a large part of the earth's surface that involved most of the world's population. The events through which we are living, differ primarily only in magnitude and speed from processes which have occurred many times in human history, from the discovery of fire to the discovery of metallurgy.



Festival of the patron saint. Notice the small crowd and lack of pageantry.

The urban-industrial revolution nevertheless carries with it one new ingredient, the methods of science in the solution of problems. It is not my contention that the social sciences can stop the present cultural revolution any more than some primitive philosopher could have prevented the Neolithic revolution. Nevertheless, the social scientist today has techniques for understanding the processes involved. Once the processes are understood, both in their general form and in their particular application, social science today can define various alternatives which may lessen the impact of the revolution upon the village world and permit rational choices between various adjustment patterns.

Basically, the problem confronting our times is whether the urban-industrial revolution shall take place under democratic or authoritarian auspices. In the past the village world has fitted into both types of pattern. The internal patterns of relatively independent villages tend on the whole to be democratic. Where the village has fitted into a pre-industrial society dominated by urban centers, the authoritarian solution has frequently occurred. Often such societies are accompanied by relatively rigid class or caste groupings of population. India is an extreme example, but class groupings were markedly developed in feudal Europe and still persist in many parts of Latin America and Asia. Consciously or unconsciously, it is such patterns that Nayon is endeavoring to break, with some success.

If Nayon represents a relatively successful adaptation to the contemporary cultural revolution, anthropological literature is full of less successful examples. In some cases the difficulties of adjustment have led to rejection and attempts to return to the past. Such a nativistic movement characterized some aspects of Ghandi's program in India. Alternatively, the frustrations attendant upon a changing culture have frequently resulted in a blind nationalism which places all blame for difficulties upon the already industrialized West.

The industrial-urban revolution, as we have seen, brings with it far-reaching changes in social relationships and institutions. At best it calls for more varied types of adjustment by the individual, and in the village world in transition it often creates intolerable frustrations. Under such circumstances the individual may reject reality and retreat to the self-created world of the psychotic. Another alternative is to surrender to authoritarian leadership, a solution usually accompanied by deep emotional appeals and increasing recourse to scapegoats to relieve frustrations. The democratic solution is to develop individuals with that confidence in themselves which allows them to respect others, coupled with individual responsibility and great capacity to adjust to new situations.

Whether the village world will turn to the authoritarian or the democratic solution of the problems created by the present cultural revolution depends in large measure upon how rapidly the newly created aspirations in the village world can be satisfied and its adjustments facilitated. Men of the village world will turn in those directions which seem to offer them the greatest satisfactions. The social sciences may yet be immature, but it is my contention that today they have the tools to discover the major problems, to make apparent the predictable directions of change, and to present clearly the available alternatives. Upon our ability to understand the nature of the cultural processes at work today, and upon the efficiency with which we meet the problems they raise for Nayon and its thousands of counterparts, may depend our future for centuries to come.

References

1. Herskovits, Melville J. *Economic Anthropology*. New York: Knopf, 17 (1952).
2. Opler, Morris E., and Singh, Rudra Datt "Economic, Political and Social Change in a Village of North Central India," *Human Organization*, **11**, 12 (1952).

Where do Cosmic Rays Come From?

ARTHUR BEISER

The author is completing work for his Ph.D. degree at New York University where he is a director, Nuclear Emulsion Research Project; Field Director, Cosmic Ray Research Project; and part-time instructor in Physics. He has just returned from helping establish a cosmic-ray laboratory at Mount Wrangell.

WHEN A. GOCKEL took off in 1910 from Switzerland in a balloon loaded with instruments he expected to find that the radiation they recorded decreased as he got farther and farther away from the earth and its radioactive mineral content. This seemed a reasonable prediction since radiation is absorbed in the atmosphere. To everyone's surprise he found that the radiation intensity *increased* with altitude; in the next few years this result was confirmed by other investigators. But if the thorium, uranium, and radium in the earth's crust did not produce the observed effect, what did? Victor Hess suggested in 1912 that radiation of some kind was reaching the earth from somewhere (or everywhere) outside, and in the twenties these still mysterious rays were termed "cosmic" rays by Millikan. Today it is known conclusively that cosmic rays indeed exist, it is known what they are when they enter the earth's atmosphere from space, and it is known how they interact with the atmosphere to produce various kinds of secondary radiations. But, after forty years, we know only what Hess knew about the origin of cosmic rays—they come from "outside."

To begin with, primary cosmic radiation, that is, the rays as they exist in space, is composed of atomic nuclei traveling with speeds so enormous as to approach that of light (186,000 miles per second). The relative proportions of the different atoms parallel those found in nature generally: thus the majority are hydrogen and helium atoms, with heavier elements such as carbon, nitrogen, and iron also present but in much smaller numbers.

Before these particles can reach the earth's surface they must pass through the atmosphere. The blanket of air covering our planet is heavier than many realize—equivalent to a layer of water thirty-four feet thick. Even the tremendous energy of the primary cosmic rays is not sufficient to enable them to get through this much matter unchanged. However, the debris resulting from their collisions with air atoms does reach the surface of the earth and in fact has even been detected several hundred feet underground. This debris, in addition to the protons and neutrons of which the struck atoms are composed, includes mesons, unstable particles associated with nuclear structure that are not very well understood at present, gamma rays, like those given off by radium only more penetrating, and positive and negative electrons.

The number of cosmic rays actually reaching the earth is very small, and the rays are detectable only with highly sensitive instruments. Hence they are entirely harmless on earth, although there is some question what their effect would be on people traveling through space for long periods of time.

The cosmic ray stream in its progress through the atmosphere presents a marvelous and exciting picture of one aspect of the workings of the physical universe. But this particular picture is not mysterious any longer, although, to be sure, there are many details that are not entirely clear at present. It is the element of mystery still surrounding the origin of cosmic rays, not only where they come from but also how they acquire their fantastic energies, that gives to research in this field a unique sense of adventure quite unfamiliar in other regions of scientific endeavor. Therefore we will not go any farther into the behavior of cosmic rays near the earth, but will concern ourselves exclusively with their "cosmic" aspects about which so little is known.

We have already called the energies associated with cosmic radiation "tremendous" and "fantastic." To justify these words one need only look at the highest particle energies attainable in the laboratory, by which is not meant the traditional small basement room of the cloistered scientist but rather the factory-size structures becoming more and more common in modern physics research. The unit of energy in nuclear physics is the electron

volt, abbreviated ev. The new four million dollar cosmotron accelerator at Brookhaven National Laboratory on Long Island, the largest now operating, contains 2200 tons of iron and 70 tons of copper, is over sixty feet in diameter and uses forty million watts of electric power. It is ultimately expected to produce protons of three billion ev (or three Bev), and an even larger machine being constructed at the University of California is planned for about six Bev maximum energy.

But energies as high as 100,000,000 Bev have been found in cosmic rays!

The measurement of such high energies is an interesting subject in itself. Less energetic particles can be examined by using "cloud chambers" with magnetic fields applied. The paths of particles passing through such a device can be photographed as they are bent by the action of the field, and their energies can be determined with fairly good accuracy from their curvatures. A more indirect method is required for a particle in the Bev range, however, based on the avalanche of secondaries it creates in the atmosphere. These secondaries, mostly mesons, tend to spread out as they go through the air, much like an avalanche of snow. The daughter particles of one primary ray may cover an area of several acres of the earth's surface. Cosmic ray shower theory tells us how to relate the width of such a spray of particles to the energy of the primary that initiated it. By observing several Geiger counters widely spaced—when they all record the passage of particles through them at the same instant, a giant avalanche must have been responsible—the primary energy can be calculated from the known counter spacings.

No information is available at present on the existence of energies greater that 100,000,000 Bev, because the enormous counter spacings needed to detect them introduce complications in the associated electronic instruments that so far have not been satisfactorily solved. However, a continuous spread of energies is present between the lowest that are detectable and the highest, and so there is good reason to suppose that even more energetic cosmic rays exist than have been measured thus far.

Now "outside," where cosmic rays come from, is a pretty big place. If we can narrow it down to a specific region of the universe, for instance the solar system or our galaxy, we will have simplified the problem of their origin greatly and perhaps even have gotten some hints about the mechanism that generates cosmic radiation.

Consider first the solar system and its principal body, the sun. Nearly all the energy reaching the earth as light and heat is provided by the sun, and so it is natural to think of it as the source of cosmic rays as well. But if this were the case, cosmic ray intensity would vary from a maximum during the day to almost nothing at night, just as with the light rays of the sun. No such marked variation is observed. Add to this the inability of physicists to conceive of a convincing solar mechanism for producing them and the result is clear: cosmic rays do not come from the sun.

Other things besides light and heat do come from the sun. Low-energy electrons and protons (hydrogen nuclei) are emitted by it at times in vast streams that cut across the earth's orbit. Their interactions in the upper atmosphere produce the startling and beautiful phenomenon of the aurora, the "northern lights."

It should be mentioned in all fairness that there are a number of physicists who still are sure that a solar cosmic ray origin is possible.[1] They contend that magnetic fields could exist in the solar system that would "scramble" the cosmic rays so that as many would reach the night side of the earth away from the sun as reach the day side toward the sun. Now magnetic fields that could do this are certainly conceivable; however, such fields, if they could scramble particles with energies of millions of Bevs, must be so strong as to permit their detection on the earth. And no magnetic fields in the solar system stronger than the relatively feeble one on the sun itself have ever been found.

The next subdivision of space to be looked into is our galaxy. Our galaxy, the Milky Way, is shaped like a fried egg, with a dense concentration of stars in the center that becomes more and more spread out toward the edges. The entire galaxy, roughly 100,000 light years across, rotates. Since the solar system is two-thirds of the way out from the center, this rotation means that we are traveling through space with a speed of 180 miles per second (superimposed, of course, on our motion around the sun). The apparent direction of this flight through the sky is the constellation Cygnus, the Swan.

In 1935 Arthur H. Compton, now Chancellor of Washington University in St. Louis, and Ivan Getting proposed an experiment based upon the rotation of our galaxy to determine whether or not cosmic rays originate somewhere inside it.[2] If cosmic rays come from *outside* the galaxy, either from other galaxies, or spiral nebulae as they are sometimes called, or from extragalactic space, then more of them will strike the forward side of the earth than the backward one. Just as in the case of solar particles this means that cosmic ray intensity should

vary every twenty-four hours, only to a much smaller extent since many rays still could enter at night. The magnitude of this variation, somewhat less than one per cent, and the time of maximum, 20 hours 40 minutes sidereal time, were predicted by Compton and Getting, whose calculations were subsequently modified slightly to take into account the disturbing effect of the earth's magnetism on the paths of incoming cosmic ray particles.[3]

Slight variations in cosmic ray intensity definitely exist. Unfortunately for the extragalactic origin hypothesis these variations result from changes in the temperature and pressure of the atmosphere, and can be predicted from a knowledge of these quantities as measured with standard Weather Bureau meteorological balloons. No correlations with the Compton-Getting calculations have been found; most, if not all, cosmic rays therefore originate within our galaxy.

At this point, although we still do not know anything substantial, the problem of cosmic ray origin is at least out in the open where we may hope to attack it realistically. We know that a galactic mechanism of some sort is probably responsible, and that it must be capable of generating with extremely high energies atomic nuclei as heavy as iron. Precisely how does this generation process work?

Serge Korff computed twenty years ago that the "energy density" of cosmic rays in space was approximately that of starlight, about 10^{-12} ergs/cm^3, light also being a form of energy.[4] This conclusion is still valid, and it poses a further question that must be added to the above one: is it possible to conceive of a mechanism that can give to cosmic radiation as much or more total energy than the entire light output of all the stars in the universe? To be aware that the magnificence of the heavens on a starry night is caused by rays of light that took thousands and even millions of years to reach the earth is to be aware of the colossal scale of this energy output. Now we are asked to believe that invisible, all but undetectable particles swarm through the Milky Way and are just as important in the scheme of the cosmos as the familiar and ever present stars. We can do nothing but believe this, of course, and it might be pointed out to the romantics among us that any number of scientists have found the contemplation of cosmic rays fully as satisfying an emotional experience as poets have found in gazing at the visual wonders of the sky.

Two possibilities exist, at first glance, for cosmic ray generators. Robert Andrews Millikan, who received a Nobel prize for the first accurate measurement of the charge on the electron, one of the fundamental constants of nature, suggested that cosmic ray particles get their energies all at once, in single acts. He proposed that this act was the annihilation of atoms and the complete conversion of their mass into energy according to Einstein's formula that energy equals mass times the square of the speed of light. This energy, in the form of a gamma ray, would presumably strike another atom in space and be transferred to it, giving the cosmic rays that we are familiar with. For example, the so-called mass energy of a helium atom is 4 Bev; of a carbon atom, 12 Bev; of an aluminum atom, 27 Bev. The heaviest atom known outside the laboratory is that of uranium, with a mass energy of 120 Bev.

This is a lot of energy. However, it is not nearly enough to account for cosmic rays, and Millikan's theory, after a period of doubt, had to be discarded.

Another single-act idea is startling in its simplicity. The universe is expanding, and it is thought that this expansion results from the explosion that was the creation of the universe about three billion years ago. Could not cosmic radiation be radiation left over from that primal event? It is hard to say in the absence of detailed information about the formation of the universe whether this suggestion should be taken seriously, but no great enthusiasm has yet been manifested in its favor. The principal difficulty lies in the history of such rays in the three billion years since they were assumed to be created; there is a good chance that all of them would long since have been absorbed by interstellar matter.

The second possibility is that the particles acquire energy gradually, a little at a time, until their ultimate energies reach the enormous values that seem to be characteristic of this radiation. Specific processes that could do just this are known that need no unusual assumptions to justify their existence and operation. It is a matter of examining them in detail and choosing the one or ones that seem adequate; something, it should be stated, that is easier said than done.

Literally dozens of schemes have been proposed for accelerating cosmic rays by electric and magnetic fields somewhere in space. Electric fields, which would act to speed up charged particles in much the same way as the high voltages used with television picture tubes speed up the electrons whose impact on a luminescent screen produces a visible spot, can be ruled out at once. Space is a good conductor of electric charges since it is empty, and so positive and negative charges will

come together and neutralize each other very rapidly if they exist separately at any time. An electric field is present around an isolated charge; if no such charges exist neither can the field, and fields that do not exist can have little to do with cosmic rays.

The situation is more promising when we come to magnetic fields, in particular varying magnetic fields. These fields can occur in individual stars or in the diffuse clouds of interstellar matter that float through space. Nothing that we know contradicts the possibility of particles with cosmic ray energies being generated in stars, for instance in giant "spots" like the sunspots of our sun, or in Babcock stars which have large and rapidly varying magnetic fields. But the number of stars that can generate cosmic rays by these mechanisms is nowhere nearly enough to account for the total number of cosmic rays present in space.

Little is really known about magnetic clouds in the galaxy; perhaps this is why theories of cosmic ray origin based upon their supposed properties seem to be very promising. Now clouds of matter are almost certainly present in space, and it is not unlikely that they have magnetic fields associated with them.[5] Fields of only about 10^{-6} gauss are sufficient for the acceleration process described below. A cubic inch of such a cloud may contain several atoms, whereas a cubic inch of air consists of atoms numbering ten followed by more than twenty zeros! Compared with ordinary space, though, such cloud densities are appreciable.

These clouds move with velocities of around twenty miles per second. A moving charged particle that collides with an irregularity in a magnetic cloud caused by turbulence either increases or decreases in speed, an excellent analogy being a tennis ball striking a moving racquet. If the racquet is moving away from the ball when it hits, the ball will rebound much more slowly than it was going before it touched the racquet. If the racquet is moving toward the oncoming ball, the ball goes faster meeting a magnetic cloud experiences a change in its speed and hence in its energy. By the application of a fundamental law of physics known as the principle of equipartition of energy it can be proved that, on the whole, more particles gain rather than lose energy in encounters with such clouds. After enough collisions, enormous energies can be produced.

The above ideas were thought about more or less vaguely for many years and were finally put into concrete mathematical form by the famous nuclear physicist and Nobel prize winner Enrico Fermi.[6] It is definitely possible that this process contributes most or even all of the protons present in cosmic radiation. And protons make up about ninety per cent of the cosmic rays reaching the earth. The trouble is that Fermi's theory fails completely to explain how particles heavier than hydrogen atoms can be accelerated to high energies.

The reason for this state of affairs lies in the fact that heavy particles lose energy more rapidly in their travels than light ones do; in order not to be stopped completely before receiving another "kick" from a passing cloud an iron nucleus, for instance, must have an initial energy of three hundred Bev. For protons the comparable figure is only two hundred Mev (million ev, not billion ev), and such comparatively low energies, in the cosmic sense, may be produced in various ways. But three hundred Bev is another story, and no one has yet been able to think up a way of arriving at energies like this *before* magnetic clouds can take over the work of acceleration.

This is where the subject stands today. Very little is known for certain about cosmic ray origin, despite the fertile imaginations that have been brought to bear on it. The stimulus provided by this problem and by cosmic ray studies in general has led to many unexpected results of great value. Perhaps in a way it is good that a puzzle of this magnitude still remains unsolved; certainly it prevents any complacency about the completeness of our knowledge of the universe we live in. And we can be sure that, when we finally learn just where cosmic rays come from and how they acquire their characteristics, we will not only have unraveled one of the knottiest of problems but perhaps will also have discovered a fundamental secret of the universe.

References

1. Richtmyer, R. D., and Teller, E. *Phys. Rev.*, 75, 1729 (1949).
2. Compton, A. H., and Getting, I. A. *Ibid.*, 47, 817 (1935).
3. Vallarta, M. S., Graef, C., and Kusaka, S. *Ibid.*, 55, 1 (1939).
4. Korff, S. A. *Ibid.*, 44, 300 (1933).
5. Alfvén, H. *Cosmical Electrodynamics*. London: Oxford University Press (1950).
6. Fermi, E. *Phys. Rev.*, 75, 1169 (1949).

General References

Leprince-Ringuet, L. *Cosmic Rays*. New York: Prentice Hall (1950).

Wilson, J. G. *About Cosmic Rays*. London: Sigma (1949).

Montgomery, D. J. X. *Cosmic Ray Physics*. Princeton: Princeton University Press (1949).

Biermann, L. "Origin and Propagation of Cosmic Rays," in *Annual Review of Nuclear Science*, Vol. 2. Stanford: Annual Reviews, Inc. (1953).

The Case for Interlingua

ALEXANDER GODE

Dr. Gode was born in Bremen, Germany, and did graduate work at Vienna, the Sorbonne, and Columbia. He came to the United States in 1927 and in 1939 was granted a Ph.D. degree in Germanics by Columbia University. He has taught modern languages at Columbia and Chicago and has been active in interlinguistics since 1934. He was editor of reference books, 1943–46, and in 1946 founded Storm Publishers, of which he is president. Dr. Gode was Director of Research, International Auxiliary Language Association, and is now Chief, Interlingua Division of Science Service.

OBSERVATIONAL STUDIES of the phenomenon "language" are of extraordinarily recent date. Just as human beings had been using their anatomy for millions of years before they got around to looking at it with critically objective curiosity (i.e., scientifically), so they were also slow in upsetting the millennial obviousness of language by asking a string of sophisticated questions about it. The scientific study of language and languages is hardly older than 150 years. What might be termed learned preoccupation with language in earlier centuries cannot be classed as science. It was at best an endeavor to describe language in terms of one specific language, that is, of course, the language of the author or speaker concerned. This is completely as though a dendrologist were to conceive of all trees as more or less surprising deviations from the apple tree in his parents' backyard (assuming that that was the only tree he knew as a child).

The prescientific attitude toward language is still very much with us. It manifests itself in the notion that language is somehow a sacred cow that must not be tampered with. Language, that is to say our language, is perfect or at least would be so if we observed all its laws faithfully, if we never split an infinitive, said "it is me," or used a preposition to end a sentence with.

As for the nontamperability of language, there is indeed a great deal to be said in support of it. There are not many instances on record where conscious interference with a linguistic situation led to a change that stuck. The word "gas" is said to have been invented by Van Helmont; the American term "blurb" was coined by Gelett Burgess; the term "bil," which the Scandinavian languages use to designate a motorcar, resulted from a prize contest organized in 1902 by a Copenhagen journal; and so forth. Such a list could surely be extended, but all its items would still remain exceptions and merely serve to emphasize that the norm is otherwise.

There is a drastic story which exemplifies at least metaphorically the fate that befalls most conscious would-be changers of language. It is found in the appendix volume to Adelbert von Chamisso's diary of the trip around the world which he took in 1815 to 1818 as the botanist attached to the Romanzow-financed expedition by Captain Otto von Kotzebue on the brig *Rurik*. Chamisso relates the story on the authority of a certain Mr. Marini who had lived on Hawaii up to about 1800. Shortly before his departure, the ruler of the island, by name of Tameiameia, found himself blessed with a son. This joyous climax to a long and grievously disappointing series of exclusively female offspring, called for a special celebration. It was customary, it seems, to celebrate special events in Tameiameia's realm by outlawing some special word in the language of the island and replacing it by another of the ruler's invention. But this, Tameiameia thought, was not enough in the present case. He decided to mark the birth of his son by radically reforming the entire vocabulary of his subjects. Among the examples cited by Chamisso are the terms for man, woman, and dog, which used to be *kanaka, waheini,* and *irio* and which Tameiameia replaced by *auna, kararu,* and *japapa*. When Marini returned to the island several years later, he found to his surprise that man, woman, and dog were again *kanaka, waheini,* and *irio*. There had been an uprising of reactionary elements in the course of which all of Tameiameia's linguistic inventions and his son were killed.

For different reasons but with comparable royal intrepidity Frederick the Great of Prussia would have loved to change the German language, which he considered quite inadequate for civilized intercourse. He was especially displeased with the frequent consonantal endings of German words which lent themselves but awkwardly to composition in French Alexandrines. Frederick proposed that the situation could be remedied by at least replacing by an *e* all the *t*'s which in German (corresponding to an English *-s*) distinguish almost all third-person singulars of the present tense. Frederick had the good sense not to embody his proposal in a royal decree, and no rebellion was needed to dispose of it. But an amazing footnote to this story is that in our days the poet Stefan George, while translating into German parts of Dante's *Divina Commedia,* re-experienced Frederick's worry about the final *t* and, being a poet and not merely a king, did replace it by an *e*, at least in that one work of his.

The superstition that conscious interference with language is impossible would seem to have at least a basis in fact. But the fact is simply that *arbitrary* interference is impossible. As in grafting and crossbreeding of plants, there are definite limits to what is possible by way of consciously manipulated change, and the successful products mingle smoothly with natural growths because they are naturally possible.

However, the far-spread idea that language is above man-willed interference has its origin in the still more general superstition that language reflects somehow the laws of logic, which in turn are the laws of nature or at least their basis. Criticism of linguistic features in the name of logic is a favorite sport of the linguistically innocent. The only thing that is still more depressing is the pseudo-informed counter-assertion that language has nothing to do with logic.

To analyze this dilemma halfway satisfactorily we must accept the conceptual system underlying every human language as the distinguishing feature setting it off from all forms of expressive reactions to present stimuli which even non-human animals can communicate and comprehend. This is not the place to pursue the question whether the presence of a conceptual system that characterizes human language might not be traced to beginnings in the animal world. But we may note in passing that the most tempting positive evidence so far has been accumulated by animal-language researchers investigating communication among bees. There can be little doubt that bees do communicate with other members of their hive. What is not known as yet is whether their system of communication is inherited or learned. And this is a crucial matter, for only in the latter case would it be possible to suspect a conceptual system underlying the bee's system of communication.

It is a distinctive, and on second thought a fairly astonishing, trait of human language that it is not biologically inheritable. That a Chinese baby growing up in an American environment should not develop Chinese speech habits is too obvious to be surprising. But the important point is really that, in learning a system of English words, this Chinese baby of ours would simultaneously be acquiring and learning to manipulate a system of concepts.

Would this system of concepts have been the same if the Chinese baby had been allowed to grow up in a Chinese environment? The question may sound naive, but it is of fundamental importance.

I remember clearly when I was taught the concept vitamin. I also recall how I acquired the concept turbine. My memory is not good enough to let me remember when and how I learned the concepts cheese, sky, play, rain, snow, but I do know that I learned them all at some time in my education. The total set of concepts which I manipulate when I speak or think represents a slowly acquired thesaurus of educational coins whose exact denomination need not be paralleled in the conceptual holdings of speakers of other languages. The concept a speaker of German uses where I use "sky" covers as well what I conceive of separately as "heaven." An Eskimo cannot understand that I have a single concept "snow" to cover what for him is a variety of different things. And as I begin to look critically at my individual concepts, I am struck by such strangely arbitrary delimitations as that I should stop conceiving of rain as rain when it hits the ground and turns for me into water while I do not carry out a similar shift in the case of snow.

The arbitrary delimitation of the concepts in my conceptual system together with the knowledge that they are acquired through education and experience and can be added to through further education and experience imposes the conclusion that they are characteristic of my thinking and speaking and probably to a large extent of the thinking and speaking of other members of my speech community, but not necessarily of the thinking and speaking of a Chinese, a Tibetan, a Hopi, a Choctaw.

The world of experience may be objective; but the system of concepts into which we organize it

is a product of education, i.e., of culture. It represents a segmentation of nature which differs from culture to culture.

The functional interdependence of culture and language here envisaged has been succinctly stated by Benjamin Lee Whorf. "We cut up and organize," he wrote, "the spread and flow of events as we do largely because, through our mother tongue, we are parties to an agreement to do so, not because nature itself is segmented in exactly that way for all to see. Languages differ not only in how they build their sentences but in how they break down nature to secure the elements to put in those sentences."

If the ideas set forth in the foregoing paragraphs seem self-evident to many readers, they should acknowledge that this is so because as scientists they are constantly concerned with the formulation of new concepts and thus experience directly the fact that a concept is a mentally achieved organization of an arbitrarily circumscribed bit of reality. They are participants in a drama of revolutionary change which since about 1890 has produced in the world of science an unprecedented number of radically new concepts. As a linguist I may well wonder whether the discipline of linguistics in its recent phases could have developed its new insights and attitudes without the contemporaneous goings-on in the sciences of nature. As for the indebtedness of modern linguistics to cultural anthropology, there is no room for any doubt whatever.

The opposite view, which clings to the strict factuality of our concepts, has the backing of a century-old tradition and is by no means dead in our time. Benjamin Lee Whorf characterizes it well as upholding the absolute rule of "natural logic"—a term he uses in lieu of "common sense." Whorf then goes on: "According to natural logic, thought does not depend on grammar but on laws of reason which are supposed to be the same for all observers of the universe—to represent a rationale in the universe that can be 'found' independently by all intelligent observers, whether they speak Chinese or Choctaw. In our own culture, the formulations of mathematics and of formal logic have acquired the reputation of dealing with this order of things, i.e., with the realm and laws of pure thought. Natural logic holds that different languages are essentially parallel methods for expressing this one-and-the-same rationale of thought and, hence, differ really in but minor ways which may seem important only because they are seen at close range. It holds that mathematics, symbolic logic, philosophy, and so on, are systems contrasting with language which deal directly with this realm of thought, not that they are themselves specialized extensions of language."

In a nutshell, then, the clashing views hold on the one hand that our conceptual systems are culture-determined and variable and on the other that there is an absolute hierarchy of concepts which is identical for all and a deviation from which must be branded an error of logic.

It is doubtless correct to speak of the absolute hierarchy of concepts as the traditional notion and to claim that the notion of culture-determined concepts is a matter of twentieth-century emancipation. Their comparative importance is clearly reflected in the history of auxiliary-language theory. A sketch of this history can show that the notion of culture-determined concepts cannot trace its ancestry beyond vague beginnings in the early decades of the nineteenth century.

The fact that this same notion had been clearly formulated at least once in the thirteenth century is then of merely anecdotal importance. But this isolated curiosity is so striking that I will be forgiven for referring to it here in somewhat greater detail.

The passage in question occurs in the early chapter of Dante's *De Vulgari Eloquentia* and must be appreciated in its linguistically fascinating context of elements which the modern reader should not be too hasty in calling quaint or medieval or otherwise "scientifically unsound." "If we clearly consider," Dante wrote, "what our intention is when we speak, we shall find that it is nothing else but to unfold to others the thoughts of our own mind. Since, then, the angels have, for the purpose of manifesting their glorious thoughts, a most ready and indeed ineffable sufficiency of intellect, by which one of them is known in all respects to another . . . , they do not seem to have required the outward indication of speech. . . . The lower animals also, being guided by natural instinct alone, did not need to be provided with the power of speech, for all those of the same species have the same actions and passions; and so they are enabled by their own actions and passions to know those of others. But among those of different species not only was speech unnecessary, but it would have been altogether harmful, since there would have been no friendly intercourse between them."

Thirdly now we come to man. "Man," Dante continued, "is not moved by natural instinct but by reason, and reason itself differs in individuals in respect of discernment, judgment, and choice,

so that each one of us appears almost to rejoice in his own species." Thus "we are of the opinion that no man has knowledge of another by means of his own actions or passions, as a brute beast; nor does it happen that one man can enter into another by spiritual insight, like an angel, for the human spirit is held back by the grossness and opacity of its mortal body. It was therefore necessary that the human race should have some sign, at once rational and sensual, for the intercommunication of its thoughts, and this sign, having to receive something from the reason of one and to convey it to the reason of another, had to be rational; and since nothing can be conveyed from one reason to another except through a medium of sense, it had to be sensual."

Thus, within Dante's frame of reference, the existence of human speech is sufficiently accounted for. The immediate sequel to this discussion has no bearing on our concern. It tries to determine whether it was Adam or Eve who spoke first and concludes in favor of Adam because "it is unseemly to think that so excellent an act of the human race proceeded earlier from woman than from man." The important point is to determine what Dante meant by the assertion that "reason itself differs in individuals in respect of discernment, judgment, and choice," for in its specific context this assertion seems to imply that reason and speech are functionally interdependent.

The answer is supplied by Dante in a most original reinterpretation of the story of the confusion of tongues at Babel. To appreciate Dante's conception fully, let us remember how we conceived of the event in our earliest childhood. When after the Lord's intervention some workmen shouted "Fire! Fire!" some others understood "Feier! Feier!" and walked home because the word meant "curfew" to them.

Here now is Dante's account: "Almost the whole human race had come together to the work [of the tower of Babel]. Some were giving orders, some were acting as architects, some were building the walls, some were adjusting the masonry with rules, some were laying on the mortar with trowels, some were quarrying stone, some engaged in bringing it by sea, some by land; and different companies were engaged in different other occupations, when they were struck by such confusion from heaven, that all those who were attending to the work, using one and the same language, left off the work on being estranged by many different languages and never again came together in the same intercourse. For the same language remained to those alone who were engaged in the same kind of work; for instance, one language remained to all the architects, another to those preparing the stone; and so it happened to each group of workers. And the human race was accordingly then divided into as many different languages as there were different branches of the work."

Thus the confusion of tongues turns out to be a confusion of reason, i.e., a confusion in man's viewing the world about him. One looked at things in terms of architecture, another in terms of bricklaying; in more modern terms: one looked at the world in terms of nuclear physics, another in terms of Darwinian evolution; or in terms of Whorf's formulation: each segmented nature according to a conceptual system dictated by his education and the culture of his environment.

Supranational Languages

The history of supranational languages needs to be reviewed in terms of the dichotomy of absolute versus culture-determined conceptual systems. The problem remains well hidden in the case of all natural supranational languages. When the Greek Koiné came into vogue as an international means of communication throughout the Mediterranean basin in the Alexandrian Age, it imposed not merely its word forms on millions of speakers of the most heterogeneous languages but also its characteristic conceptual system. It had the force to do so because it owed its spread not to the consensus of heterogeneous-minded peoples but rather to a powerfully expansive cultural dynamism. The international validity of English in our time is promoted and established by a sort of cultural hegemony of the Anglo-Saxon peoples. The same holds true for the *Kuan Hua* ("speech of officials") or Mandarin Chinese, which functions as a superimposed business language in all of China with its multiplicity of diverging dialects. A simplified Malay functions in a similar way for some forty-five million people in the Malay archipelago; so do Swahili, the best-known of several African intertribal languages, and Hindustani, the "popular" speech of North India, which functions as a common tongue for some 230 million people.

All these languages impose their culturally determined conceptual patterns along with their systems of forms upon all those who find it possible to benefit practically or culturally from adopting an originally alien speech. In this they differ from colonially imposed languages only in the kind of coercion they exert.

It is easily understood that the linguist should

be particularly interested in supranational languages that owe their characteristics to the merger of influences from two or more centers of cultural radiation. This is the case in pidgin English which is (roughly characterized) a scanty stock of English word material forced into a non-European linguistic structure. A drastically simplified Italian with a strong admixture of Catalan, Spanish, French, Greek, and Arabic words originated during the Crusades in the Mediterranean countries and is still used in parts of Tunisia and Tripolitania under the name of Lingua Franca (a term which is often generically applied to all languages of the type here described).

Peculiar interest attaches to the supranational languages that serve as vehicles for religious expansion and carry with them the conceptual system of the culture in which a particular religion has its roots. In addition to the above-mentioned Greek Koiné, which came to be a religious language when its supranational standing had led most naturally to its being used as the language of the New Testament, we must list here Sanskrit and Pali of Hinduism and Buddhism and Church Slavonic and Latin of the Christian world.

The impact of Latin on the Western world is more talked about than appreciated in its implications. Its most striking characteristic is the fact that it comes in at least three clearly distinct phases. The first is the sweep of Roman colonization. Through it Latin became the language of Gaul and the Iberian peninsula, of the entire territory in which it is still spoken today in the form of the various Romance languages. But beyond these confines it affected also the language of people who emerged with linguistic autonomy from the crumbling empire: the Germans east of the Rhine and the Anglo-Saxons as conquerors of the Britons in the North.

There follows a Christian phase of the Latin hegemony over the Occident. This covers once again the lands of Imperial Roman colonization spreading the use of a Rome-centered Latin which in its ecclesiastic form becomes more and more differentiated from the vernaculars spoken in Italy, Gaul, Spain, and so forth. But in this form Latin establishes itself far beyond the realm of the once unified empire. It becomes the language of culture throughout vast regions where the language of the people is a Teutonic or Slavonic dialect. Even today the confines of this territory are visible in sharp outline marked by the range of the use of the Roman alphabet.

The third phase is the resurrection of classical Latin through the Renaissance. And this now claims our attention to an extent beyond the limits of a mere sketch.

Throughout the Middle Ages, Latin—Church Latin, that is—had been the vehicle of all learned discourse. It had been the language of European culture in all its aspects. With the Renaissance this situation underwent a radical change, and—paradoxical though it may sound—the "reclassicalization" of Latin was not the least of the factors that contributed to the collapse of Latin as the common cultural language of the Western world. Medieval Latin had been a living language. You could swear in it. You could—as one authority puts it neatly—go whoring in it without feeling that you were indulging in a display of erudition. The classical Latin of the humanists, on the other hand, was—if not dead—at any rate a resurrected language. It prided itself with being exclusive. And that at a time when participation in cultural endeavors became democratized.

If during the Middle Ages the centers of cultural endeavor had as a rule been centers of Church learning, now the middle classes in the cities had become firmly enough established to begin taking over from the clergy the function of a culturally representative class. It is a moot question whether the culturally rising middle class would have continued to use a living Latin—an organic continuity of medieval Latin—as its natural medium of intercourse, if the linguistic revolution of the Humanists had not substituted for it a superrefined instrument for exquisitely distinguished minds. The fact is that the resurrected classical Latin of the Humanists lent itself arduously to the cultural purposes of the expanding culture-bearing class.

But a third factor contributed simultaneously to the weakening of Latin's claim to being the natural language of European cultural exchange and intercourse. This is the rise of nationalism and the concomitant national pride in the excellence of the national tongue.

The centers of culture and learning which in pre-Renaissance centuries had been ecclesiastic in spirit and purpose assumed after the Renaissance a more and more secular demeanor. We may well speak of a progressive secularization of the universities, and if we consider the part played in post-Renaissance times by princely courts and middle-class-initiated academies, we are struck by the fact that for them Latin was never a natural workaday medium of expression. To put it pointedly: When Roger Bacon got up from his studies, in the pursuit of which he naturally used Latin as the normal

linguistic instrument, he used the same Latin to order his famulus to bring him something to eat. When, on the other hand, Sir Isaac Newton interrupted the composition of his Latin *Principia,* it was roast beef and not *caro bubula tosta* he called for.

The progressive recession of Latin as the common cultural language of the Western world set the stage for the earliest conscious preoccupation with the problem of a universal auxiliary language. The most important works here to be cited are Leibnitz' *De arte combinatoria* of 1666 and John Wilkins' *An Essay Toward a Real Character and a Philosophical Language* of 1668. It is the approach these thinkers pursued that interests us particularly. In the *Nouveaux Essais,* which Leibnitz wrote in 1704, there is a chapter heading, "De l'imperfection des mots." This is the keynote to his lifelong preoccupation with a reformed symbolism of thought. Latin had been used for centuries as a symbolism of thought. Leibnitz mastered this symbolism. He also mastered French and likewise German. But what he was after was a *perfect* symbolic system which would imply a perfect method of thought. Back in 1629 Descartes had expressed this same concern in a letter to Père Mersenne in which he referred to a work on Chinese ideographs and their possible use in a universal system of communication. "J'oserois esperer ensuite," Descartes wrote, "une langue universelle fort aisée à apprendre, à prononcer et à écrire, et, ce qui est le principal, qui ayderoit au jugement, luy representant si distinctement toutes choses, qu'il luy seroit presque impossible de se tromper."

"A language apt to serve our judgment because it can represent things so distinctly that it will become almost impossible for our judgment to err," that is what Leibnitz and his contemporaries wished to achieve, for languages like Latin, French, and German—all the traditional symbolisms of thought—seemed to hinder rather than help clear thinking. The ideal language of the searcher after truth was a system very much like mathematics: it could almost actively produce truth if only its user knew how to manipulate its symbols correctly.

The faith here implied in the absolute existence of a hierarchy of ideas which can think and lead to truth almost without the intervention of man and certainly without the intervention of a non-conceptually and emotionally contaminated language, this faith seems to have a strange fascination. It can be encountered even today. Even now at the rate of two or three a year idealistic crackpots produce outlines (never more than outlines) of universal tables of concepts and corresponding symbols which (when completed) can be manipulated as universal systems of communication with foolproof elimination of all possibilities of error or misunderstanding.

In a less extreme form the Leibnitzian faith in an absolute conceptual system has influenced all serious interlinguistic endeavor throughout the nineteenth century and beyond. The contrasting view, which holds that concepts are forms, given by the psychic entity of speech-thought and used by us to organize our perceptional field, was formulated for the first time in 1808 by Friedrich Immanuel Niethammer in his work *Über Pasigraphie und Ideographik,* apparently in pursuance of a suggestion made by Friedrich August Wolf. It was extremely slow in gaining recognition, but the trend of which it is symptomatic led at least to the predominant acceptance of the principle that an auxiliary language might profitably be built after the pattern of living tongues. This is apparent in Schleyer's construction of Volapük in 1880 and still more in Zamenhof's Esperanto of 1887. But all these projects have in common the endeavor to improve on the "imperfection" of traditional tongues by evolving a systematic hierarchy both of forms and of concepts. And they attempt and claim to be as universal as Leibnitz' *ars combinatoria* hoped to be.

What is completely and radically new in projects of supranational communication of the type of Volapük and Esperanto is their motive. Especially the latter is explicitly designed to eradicate misunderstandings in human intercourse. This has its roots in early experiences of the Polish-Russian-Jewish Zamenhof. As a child in his native town he had been struck by the observation that German, Russian, Polish, and Yiddish-speaking groups often came to blows because they could not understand each other. This local situation, he felt, was a perfect illustration of conditions in the world at large. If people could understand each other, there would be neither beerhouse brawls nor wars. So far it is impossible to disagree with Zamenhof. But, when the argument goes on to claim that people would understand each other if they spoke the same language, one cannot help having some guarded doubts. Language, after all, is as useful for purposes of camouflage and worse as it is for laying bare one's innermost soul.

There is something nostalgically beautiful about the naïveté of the true Esperantist's faith in the power of his language. One may be justified in suspecting that it is this faith in its often fanatical

excesses that explains why Esperanto has progressed as far as it did, and not really the power of the language itself.

The grammatical system of Esperanto is both a simplification and an ingenious extension of the accustomed categories of the European languages. The vocabulary consists of a carefully chosen list of roots which are manipulated with a strictly schematic set of affixes. A dog is *hundo,* a bitch *hundino,* a puppy *hundido,* a she-puppy *hundidino,* a big dog *hundego,* a big bitch *hundinego,* a big puppy *hundidego,* a big she-puppy *hundidinego,* etc. *Preĝi* is to pray, *mi preĝas* I pray, *mi preĝis* I prayed, *mi preĝos* I shall pray, *mi preĝus* I should pray, *preĝu* pray!, *preĝejo* church, and so on.

The concepts covered by the roots are those Zamenhof felt subjectively to be indispensable. The forms were subjectively picked by him with a certain awareness of the fact that those represented in several natural languages were to be preferred. In cases of uncertainty he chose words from languages not as yet sufficiently represented to make the entire project impartially just. So boy came to be represented by *knabo* (after German *Knabe*), ship was *ŝipo* (straight from English).

The mastery of this system gives the adept a feeling of prestidigitatorial pride. It works! It is amazing what you can do with a little skill! But the fun is spoiled when one tries to build up conceptual complexes like automobile, orthography, elephantiasis, excrescence.

Until one runs across conceptual problems of the kind just enumerated, one is tempted to consider Zamenhof's system an answer to Leibnitz' prayer. But Leibnitz wanted a full and exhaustive pyramid of concepts and corresponding symbols, whereas Zamenhof suddenly veers away from the consequences of his logical approach as soon as the results promise to be hermetic. He supplies at this point the famous rule XV, which I quote from his *Fundamenta Krestomatio* (p. 254): "La tiel nomataj vortoj fremdaj, t.e., tiuj, kiujn la plimulto de la lingvoj prenis el unu fonto, estas uzataj en la lingvo Esperanto sen ŝanĝo, ricevante nur la ortografion de tiu ĉi lingvo; sed ĉe diversaj vortoj de unu radiko estas pli bone uzi senŝanĝe nur la vorton fundamentan kaj la ceterajn formi el tiu ĉi lasta laŭ la reguloj de la lingvo Esperanto." Or in English: "The so-called foreign words, that is, those which the majority of languages have taken from one source, are used in the Esperanto language without change, receiving only the orthography of the latter language; but with various words from one root it is better to use unchanged only the fundamental word, and to form the rest from this latter in accordance with the rules of the Esperanto language."

I need not analyze further the strange ambivalence of Esperanto. Suffice it to summarize that in this language the rationalistic ideal of a logical hierarchy of concepts bows to the sweep of modern international words. For this should be noted: the expression "foreign words," which from an English point of view seems rather obscure, is taken directly from the German *Fremdwort* and applies to, among other things, all the modern scientific and technological terms that speakers of all sorts of languages concoct out of Greek and Latin material.

In so far as Esperanto embodies what Zamenhof called the *vortoj fremdaj,* it assumes traits of a common European language and abrogates its own oft-repeated claim to universality, for in practice the *plimulto de la lingvoj,* "the majority of languages," must not be taken literally but allowed to coincide with the majority of the languages of the Western world.

It is not as a universal language but as in part a common European tongue that Esperanto has been able to develop a fairly strong appeal in the Orient, especially in China and still more in Japan, where it has proved its ability to function as a language of science and technology, provided both the author and reader are initiates of its cult. In this we may see a realistic supplement to the basically emotional motivation of Esperanto promotion and dissemination.

Yet this question of motivation, which introduced and concluded the foregoing references to Esperanto, needs to be asked with much more incisive brutality. It is clear what inspired Leibnitz and his contemporaries in their quest for a universal medium of thought. It is likewise clear how languages like medieval Latin, Swahili, pidgin English, and the rest came to be languages of international communication. They were needed to carry, and in turn were carried by, some expansive force that could not be contained within the limits of one nation. They were functionally dependent upon some form of colonial, commercial, or religious imperialism. Latin was first the language of conquering legions, then that of an ever-militant Church. Swahili, which originated as the hybrid tongue of the descendents of Arab-Negro intermarriages on the Somali coast, spread to the region of the Great Lakes in answer to trade needs of Arab caravans and later into the Congo basin through the pressure of European colonial and

commercial influences. On the other hand, a great language like Hebrew never achieved extranational currency because until recently it had survived exclusively as the language of a strangely unexpansive, non-missionary religion.

On this basis the whole question might be summarized in these terms: An international language can subsist only as the speech instrument of an expansive supranational dynamism of which it is both vanguard and adequate expression. It is not likely that an artificially constructed language could ever fulfill these requirements, but this last point may well be left out of consideration, for the real issue is quite directly whether or not a supranational dynamism is at work in a given place and at a given time.

If there is nothing of the sort in our day and age, then all our preoccupation with an international language is a harmless and futureless pastime. If on the other hand a cultural power of international scope does exist, then it is not you or I who can decide what sort of an international language we should like to have for it. That cultural power itself will do the deciding, and all we can do is clarify and accelerate the process.

We speak often and insistently of this world of ours as *one*. We take for granted the powers that made it one in so far as it is so. Our religions, our philosophies, our faiths and ideologies are multifarious as of yore. They make us the individuals, the partisans, the nationals we are. They do not make us citizens of the one world. That is the doing for better or worse of that huge event in the current phase of human history which we call by the somewhat niggardly name of science.

The justification of this assertion, its illustration by way of tracing the history of science and its worldwide drive, is a link in the chain of our argument which we may consider established. It is likewise an established fact that science—whatever its branches, reflections, antecedents—is a growth of post-Renaissance Western European history.

The concepts of science have emerged in Western thought and fit the patterns of the corresponding languages. No one has formulated this observation more strikingly than Benjamin Lee Whorf. "The world view of modern science," Whorf wrote, "arises by higher specialization of the basic grammar of the western Indo-European languages. Science of course was not caused by this grammar; it was simply colored by it. It appeared in this group of languages because of a train of historical events that stimulated commerce, measurement, manufacture, and technical invention in a quarter of the world where these languages were dominant." And further: "Among our modern European languages, with perhaps Latin and Greek thrown in for good measure, there is a unanimity of major pattern which exists only because these tongues are all Indo-European dialects cut to the same basic plan, being historically transmitted from what was long ago one speech community; because the modern dialects have long shared in building up a common culture; and because much of this culture, on the more intellectual side, is derived from the linguistic backgrounds of Latin and Greek. From this condition follows the unanimity of description of the world in the community of modern scientists. But it must be emphasized that 'all modern Indo-European-speaking observers' is not the same thing as 'all observers.' That modern Chinese or Turkish scientists describe the world in the same terms as Western scientists means, of course, only that they have taken over bodily the entire Western system of rationalizations, not that they have corroborated that system from their native posts of observation."

Conceptually the language of science is Western European. And, since it is the language of science which alone in our time is truly an international language, Western European is *the* international language of the twentieth century.

What is conceptually correct is likely to be formally correct. And, indeed, not only the concepts of science but also its formal terminologies are Western European. This does not mean that the technical terms of science are derived from this or that Western European language. The point is precisely that they are not French or English or German or Latin or Greek. The word "statistics" is *not* German, even though a German was the first to use it. "Penicillin" is *not* English; "automobile" is *not* French. Nor are these terms Greek or Latin. They are products of the modern world albeit of traditional stuff.

European Language

The existence of a common European language is a correlate of the common occidental civilization. On the level of anthropological linguistics the keenest observations on this count have again been made by Benjamin Lee Whorf, and it is he to whom we owe the term "Standard Average European." In a somewhat specialized context, Whorf wrote as follows: "A comparison between Hopi and western European languages makes evident that the grammar of Hopi bears a relation to Hopi culture, and the grammar of European tongues to

our 'Western' or 'European' culture. And it appears that the interrelation brings in those large subsummations of experience by language, such as our own terms 'time,' 'space,' 'substance,' and 'matter.' Since with respect to the traits compared there is little difference between English, French, German, or other European languages with the possible exception of Balto-Slavic, I lump these languages into one group called SAE or 'Standard Average European'."

If we propose to view, let us say, Spanish and Portuguese as dialects of one and the same standard language which happens not to have been codified, no one will object or find the idea outlandish with the exception of a half dozen exceptionally stubborn Portuguese linguochauvinists. To link Sardinian and Italian, Provençal and French, or even all the Romance languages as variants of a common norm may likewise be accepted as a matter of course. If it is borne in mind that English has a vocabulary of predominantly Romance origin, the postulation of a homogeneous Anglo-Romance group of languages turns out to be fairly easy too. In this latter step, one may have to give a special accounting of the little sentence builders—words like I, to, over, not, will—which are clearly Anglo-Saxon and are rarely replaceable by more highfalutin doublets of Romance origin. Yet on the whole these words perform functions comparable to those of their opposite numbers in Romance, and, again on the whole, they correspond fairly well in an item-by-item way to Romance words.

In what sense, however, would it be possible to draw a language like German into the orbit of SAE? In an earlier passage we have tried to speak of the strong impact of Latin on German and other languages akin to it. German does have a large number of terms coming directly from Latin, and it also has a tremendous learned vocabulary which appears in the language of specialists under international forms.

Yet the most incisive phase of what we may call the historical Europeanization of German has not yet been touched upon. Medieval ecclesiasticism brought into German lands not just a restricted number of terms but rather a whole world of concepts. These were new concepts which established a basic pattern for German thought and made of it a strand in the tradition of occidental, Western, Mediterranean, or European thought. The number of common European concepts thus represented in German is not a whit smaller than the number of such concepts found in English, or French, or Spanish for that matter. Yet, since there was no native population descended from Imperial Roman settlers and no equivalent of the Norman conquest, the introduction of concepts like "conscience," "purgatory," "Saviour," or "cancer," "prejudice," "minority," was not managed by straight terminological adoption. The terms were analyzed in their meaning as derivatives, compounds, or similes, and corresponding German words were built of corresponding raw materials. In this way it came to pass that the concept of "conscience" is represented in German by a term that really says "con-scientia," that is, a collective knowing (*Gewissen*); "purgatory" is really a place where one gets purged or swept (*Fegefeuer*); "influence" is really flowing in (*Einfluss*); something "superfluous" is flowing over (*überflüssig*); "suicide" is self-murder (*Selbstmord*); "equator" equaler (*Gleicher*); "cancer" a crab (*Krebs*); "milliped" a creature with a thousand feet (*Tausendfüssler*); "to possess" to besit (*besitzen*); and so forth.

The number of these terms is tremendous. Yet it is not their number alone that makes them important. What matters particularly is the fact that these terms are derivationally clear and concrete. To illustrate: If the Romans had used a term like *influentia* in the extended sense of our "influence," they would instinctively have felt that the notion was one of flowing in. They could have played on the word, rejecting, for instance, the statement that somebody was under the influence of somebody else by suggesting that the situation was rather the other way round, that it was not *influentia* but *effluentia*. A German can do that with *Einfluss-Ausfluss* exactly after the Latin pattern, while those languages which have not only the Roman notion but also the Roman form could follow suit at best in a very artificial and cumbersome procedure. Only a sophisticated college graduate can understand me if I insist that there was no influence but rather a flowing out. And not even he will laugh.

Indeed, the man who first claimed that German was the most Romance of all Romance languages must have done so tongue in cheek, but he knew what he was talking about.

The Language of Science

The "language of science," as we have amply demonstrated, is common European. But is it really correct to speak of the vast stock of internationally current technical terms as a language? It is perhaps a highly characteristic point that science has

carried to all parts of the globe its system of concepts and its corresponding terminologies but not its complete and coherent language. The reason is that it actually has nothing of the sort, a fact which may seem to many a tempting invitation to find here symptom, symbol, or proof of non-existence at this time of a full and coherent world view of science.

We cling to clearer evidence by insisting at this point that the international terminologies of science, which admittedly have not coagulated in one complete and coherent international language, do form an integral part of SAE. In this setting they do represent a complete and coherent language. And, if in their international functions they are to develop or be developed into a full-fledged language, there is no other system but SAE in which they can thrive and be naturally effective.

The idea that common European is destined to serve as *the* international language of modern times is not new. It was vaguely in Niethammer's mind when he wrote his above-mentioned *Pasigraphie*. It must have been sensed by Schleyer and Zamenhof, who took the bulk of their raw material from occidental sources. But the first to present it in fairly tangible terms was the German-Chilean surgeon Liptay, who more than half a century ago stated, in a preliminary sketch of what he was then planning to elaborate into a complete auxiliary language, that the only originality of his plan was that it was devoid of originality, that the production of an international language was not required since one existed—latent in the common elements of the various national tongues—and that it had merely to be discovered, not invented.

Since Liptay's time, the idea has had a slow and arduous growth. The most pronounced clarification was contributed by Edgar de Wahl in his auxiliary language, Occidental, of 1922. But full emancipation from the last vestiges of conceptual absolutism, of full internationality achieved through logic rather than in the wake of facts and trends in history, has not been attained before the advent of Interlingua in its present form.

Under the sponsorship of Science Service, the linguistic system Interlingua is making a strong bid to serve as the medium of international communication in science, technology, and other fields in which both the initiative and the objectives are such that a restriction of their audience to the limited range of individual national tongues is paradoxical and hence inefficient. Interlingua proposes to succeed where Volapük, Esperanto, and a host of other schemes have failed. Its claims, therefore, need to be examined critically in the widest context of implications.

The vocabulary of Interlingua embodies all the word material that the languages of the Western world have typically in common. It manipulates this material according to a system of word compounding and derivation obtained from the same sources. It operates with a minimum set of grammatical features all of which its source languages are unanimous in requiring as indispensable devices in daily intercourse.

The research underlying Interlingua was a matter of practical observation, never of theoretical experimentation. None of its elements reflects merely what its compilers, in subjective rationale, considered "handy" or "nice to have." Strictly speaking, the language is not *constructed* but *extracted*. It aims to be simultaneously French, English, Spanish, Italian, and so on, each one of these languages streamlined by the elimination of its idiosyncratically distinctive features in such a way that the result coincides with the corresponding streamlining product of the others.

Interlingua looks thus: *"Sub le gerentia de* Science Service, *le systema linguistic interlingua presenta con urgentia su candidatura a servir como medio de communication international in le scientias, le technologia, e altere campos in que tanto le initiativa como etiam le objectivos es tal que un restriction de lor auditores al confinios del linguas national individual es paradoxe e consequentemente inefficiente. Interlingua promitte succeder ubi volapük, esperanto, e un hoste de altere schemas ha fallite. Pro iste ration su pretensiones debe esser examinate criticamente in le plus large contexto de implicationes."*

The major theorems with which Interlingua stands or falls are the following:

1. The languages of the world are not reducible to one universal pattern. There is no "absolute" linguistic pattern, and every language—natural or constructed—falls of necessity into one of many possible specialized molds.

2. The global internationalism of the twentieth century is propelled by ideological forces of occidental origin. These are conceivable only in occidental thought (and speech) molds; they are so conceived by all participants regardless of what their accidental native language background may be.

3. The languages of the Occident are—by origin or in consequence of historical influences or both—so closely akin to one another that it is both sound

and illuminating to conceive of them as variants of one type language which has fittingly been called Standard Average European (SAE).

4. The natural and only possible supranational language of science—which, in the most comprehensive sense, coincides with the forces of contemporary internationalism—is Standard Average European. Of this Interlingua is to date the most satisfactory formulation. Eventual improvements of it, such as are characteristic of all living languages, will come in the form of organic developments through usage.

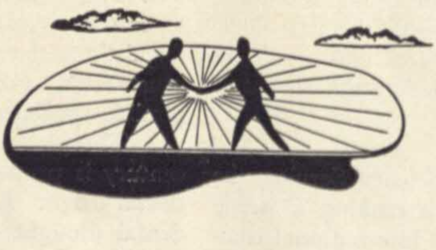
The methodology used in the elaboration of Interlingua was consciously designed to yield a codification of the existing common European language. The result is a living language, not a dead artifact. Hence Interlingua can change and grow. It can be developed further through usage. It can even be reformed if someone proves that this or that feature of its underlying methodology did not yield as perfect an approximation of the common Western type as it could and should have. But change and growth and development and reform cannot replace Interlingua by something strikingly different as long as science with its occidental "grammar" controls the common progress of mankind.

And a final word: Let us respect the non-scientific cultures by recognizing that their "grammars" may differ from that of science and by insisting that the expansive dynamism of Interlingua as an adjunct to that of science shall not violate their prerogatives in their natural habitat.

Whorf wrote: "I believe that those who envision a future world speaking only one tongue, whether English, German, Russian, or any other, hold a misguided ideal and would do the evolution of the human mind the greatest disservice. Western culture has made, through language, a provisional analysis of reality and, without correctives, holds resolutely to that analysis as final. The only correctives lie in all those other tongues which by aeons of independent evolution have arrived at different, but equally logical, provisional analyses."

Interlingua corresponds to the provisional analysis achieved in Western thought. It unifies the dialects of the Western world, not the languages of the world. Its internationality is as vast and as limited as that of science, in which the provisional analysis achieved by Western thought is given a provisional justification.

Note: The reference to Chamisso is *Werke*, Bong, V, 57. All the Whorf quotations are from *Four Articles on Metalinguistics*, Foreign Service Institute, Department of State, Washington, D. C., 1950. The basic Interlingua manuals are *Interlingua-English, a Dictionary of the International Language*, prepared by the Research Staff of the International Auxiliary Language Association under the Direction of Alexander Gode, 480 pages; and *Interlingua, a Grammar of the International Language*, prepared by Alexander Gode and Hugh E. Blair of the Research Staff of the International Auxiliary Language Association, 128 pages, both Storm Publishers, New York, 1951. Both antecedents and methodology of Interlingua are accounted for in considerable detail in the prefatory apparatus of *IED* (*Interlingua-English Dictionary*).



Southern College and University Faculty Research Resources in Physical Science and Engineering Fields

HEATH K. RIGGS

The author was born in Richmond, Vermont, and was educated at the University of Vermont where he taught mathematics and later became Director of Admissions. He holds a Ph.D. degree in higher education from the University of Chicago and since 1951 has been Chief of Educational Research, Southern Regional Education Board, Atlanta, Georgia.

LAST October* John I. Mattill reported to the readers of THE SCIENTIFIC MONTHLY on a national inventory of college and university research resources completed in 1951 by the Engineering College Research Council. The data of this inventory has now been further analyzed by the Southern Regional Education Board to find out how Southern colleges and universities stand in terms of faculty research potential in physical science and engineering fields, to what extent this potential is active in research, and how much of this is active in defense research. The basic purpose was to locate by fields and by states (although the relative state findings are not reported here) the qualified but unused research potential of Southern faculty. The findings of the Mattill article, combined with the findings of this study, afford an opportunity to make some interesting regional and national comparisons. These findings take on added significance, since according to the report, "substantially all" the national potential for research in colleges and universities in the physical sciences and engineering was reported.

It is hoped that the general method employed in this study of drawing from national data and findings, findings of value to particular regions, will encourage and assist other regions to make similar analyses.

Of the 24,881 faculty members of the three levels of professorial rank and full-time research personnel in the nation in all physical science and engineering fields, 5,121 (21 per cent) were in the colleges and universities of the Southern Compact region.† Of the 21,037 in the nation considered qualified by their institutions to do research, 4,155 (20 per cent) were on Southern faculties. Of the 12,866 engaged in research in the nation, 2,321 (18 per cent) were in the South. These are equivalent to 6,745 full-time research workers in the nation and 1,183 (17.5 per cent) in the South. Finally, of the 3,291 in the nation doing research for national defense in February, 1951, 437 (13.3 per cent) were on Southern faculties.

From these findings it may be concluded that Southern colleges and universities were doing in February, 1951, (in terms of time spent) between one-fifth and one-sixth of the college and university

* John I. Mattill, "College and University Research Resources," THE SCIENTIFIC MONTHLY, VOL. LXXV, October 1952.

† The Southern Compact region is composed of the following fourteen states joined together by an interstate compact for the improvement of graduate and professional education: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and Virginia.

research of the nation in these fields but only between one-seventh and one-eighth of the research for national defense. As of February, 1951, Southern colleges and universities were not receiving a proportion of defense research equal to their proportion of the nation's faculty and full-time research personnel in physical sciences and engineering fields, or equal to their proportion of the nation's faculty members considered qualified to do research, or equal to their proportion of the nation's active research men in colleges and universities, or equal to their proportion of the nation's total faculty time spent on research.

Furthermore, about seventeen hundred Southern faculty members considered qualified by their institutions to do research in nineteen physical science and engineering fields remained without research projects. They represent considerable human resources available to meet research manpower needs. But it is even more valuable to know the fields in which this qualified but unused research potential is located. In Table 1 such fields are identified. Those fields with the greatest relative research reserve are listed first. Less than half of the qualified Southern faculty and full-time research personnel in astronomy, marine engineering, mechanics, industrial engineering, mechanical engineering, metallurgical engineering, mathematics, electrical engineering, civil and sanitary engineering, and mining engineering were active in research in February, 1951. On the other hand, more than two-thirds of the qualified faculty of the South had research projects in ceramics and glass, chemical engineering, earth sciences, chemistry, and food technology fields.

TABLE 1

RELATIVE USE OF RESEARCH RESOURCES IN THE SOUTH

Field	Percentage of qualified faculty active in research
Astronomy	32
Marine engineering	36
Mechanics	37
Industrial engineering	38
Mechanical engineering	41
Metallurgical engineering	42
Mathematics	45
Electrical engineering	45
Civil and sanitary engineering	46
Mining engineering	47
Electronics	55
Petroleum and fuels engineering	55
Aeronautical engineering	60
Physics	63
Food technology	69
Chemistry	70
Earth sciences	71
Chemical engineering	77
Ceramics and glass	84

TABLE 2

RELATIVE DEFENSE USE OF RESEARCH RESOURCES

Field	Percentage of total research time on defense research		
	South	U.S.	Difference
Civil and sanitary engineering	20	76	56
Mechanical engineering	21	48	27
Mining engineering	0	25	25
Mathematics	14	37	23
Electronics	64	81	17
Petroleum and fuels engineering	0	17	17
Industrial engineering	1	18	17
Earth sciences	23	38	15
Astronomy	24	37	13
Ceramics and glass	27	39	12
Mechanics	50	56	6
Chemistry	32	38	6
Physics	65	70	5
Aeronautical engineering	78	82	4
Food technology	19	19	0
Chemical engineering	38	36	−2
Electrical engineering	82	76	−6
Metallurgical engineering	82	70	−12
Marine engineering	86	71	−15

It may also be of some assistance to federal agencies who make research grants and contracts with colleges and universities to list the fields in which Southern faculties spent a smaller proportion of their total research time on defense research in the field than the national average. Table 2 gives such information. It appears that in the first fourteen fields listed in Table 2 Southern colleges and universities were doing less defense research in proportion to their total research in the field than the national average. Combining the findings of the two tables, it may be noted that from one-third to two-thirds of the qualified faculty of the South are without research projects in eleven of these same fourteen fields. On the other hand, Southern faculties are doing more defense research in proportion to their total research in marine engineering, metallurgical engineering, electrical engineering, and chemical engineering than the national average for these fields.

In two other respects the national findings as reported in Mattill's article are also true for the Southern Compact region. The Research Council Committee found "that colleges and universities are maintaining emphasis on the fundamental sciences." More than 51 per cent in the South and 53.5 per cent in the nation of all faculty research time in physical sciences and engineering fields was devoted to the basic sciences of chemistry, physics, mathematics, earth sciences, and astronomy. Similarly, 49.4 per cent in the South and 50.2 per cent in the nation of all research time spent on defense projects was in the same basic sciences.

Finally, about the same concentration of defense research holds for three universities in the South as holds for fifteen universities in the nation. Three Southern colleges and universities account for 47 per cent of the total faculty time spent on defense research in all Southern colleges and universities, whereas these same institutions account for only 13.5 per cent of the total faculty and senior research staff members. In these three schools, 72.5 per cent of the faculty are active in research, and 71.5 per cent of their research time is devoted to defense research. Similarly, the Mattill article reported:

> The survey figures show that fifteen colleges and universities account for one-half of the total faculty time spent on defense research throughout the nation, whereas these same institutions account for only 20.5 per cent of the total faculty and senior research staff members. In these fifteen "favored" schools, 75 per cent of the faculty are active in research and 70 per cent of their research time is devoted to defense research.‡

Other comparisons made in the Mattill article for the fifteen institutions in the nation and for the three in the South, and likewise for the 498 other educational institutions in the nation and for approximately 127 in the South responsible for the other half of all defense research time, follow similarly.

These findings on the distribution of defense research provide a possible explanation as to why educators today take such opposing views as to the effect of sponsored research on the educational programs of the institutions. Educational leaders have reacted differently to sponsored research because such research has affected their institutions differently. At some institutions the volume of sponsored research has been so great that educational leaders of these institutions have justly been concerned that teaching programs may have been crowded into the background and that the institutions themselves may have become dependent upon federal support to a considerable extent. At the same time, other institutions in which little sponsored research has taken place, have, to a greater degree, tended to recognize the advantages of sponsored research in expanding the total volume of research on their campuses, in enabling them to retain their graduate faculty while enrollment was relatively light, and in contributing to the advanced education of students by providing research experience and financial support. These differing views of the effect of sponsored research are perhaps merely recognizing the more basic fact that there has been a rather uneven distribution of defense research in colleges and universities in relation to the existing distribution of qualified human research resources.

‡ The Scientific Monthly, Vol. LXXV, p. 236, October 1952.

Both the Mattill study and this one indicate that the critical shortage of technically trained research manpower is to some extent an artificial shortage brought about by the uneven distribution of research funds in relation to the distribution of qualified human research resources. If the nation is to get a maximum benefit from available qualified human research resources, and if the nation is to meet the critical shortage of research talent that federal defense agencies claim, without endangering the supply of new science graduates, additional methods will have to be developed by colleges and universities, and also by federal and state agencies and industries, which will encourage and insure expanded and broadened research opportunities in all sections of the nation for qualified personnel.

What kinds of methods might be explored? Some of the following possibilities might merit consideration.

1) A systematic assessment of the research potential and research use of the nation's colleges and universities in all fields of knowledge (not simply the physical science and engineering fields).

2) The development of more effective ways of keeping institutions informed of the research needs and interests of state and federal agencies, and agencies of the qualifications and interests of faculty research personnel and of institutions to do specific types of research.

3) Studies to develop a practical guide as to the amount and kinds of research a college or university can safely assume to maximize the advantages and minimize the dangers of sponsored research.

4) The development of methods to stimulate qualified faculty members to assume more initiative in developing research ideas and in presenting these in the form of proposals to agencies, foundations, and industries for research support.

5) Self-evaluation by colleges and universities to determine the kinds of basic research the institutions are qualified to do, taking into account personnel qualifications and interests, facilities for research, and unique environmental conditions.

6) Interinstitutional cooperation to work out ways by which the administrative "know how" of institutions with considerable sponsored research experience may be made available to other institutions with little research experience and to consider subcontracting large projects among groups of institutions.

7) A review of the research that federal agencies

are doing in their own laboratories to determine whether some of the basic research might more appropriately be conducted through grants or contracts with colleges and universities, where students may be trained at the same time.

8) The development of administrative methods in federal agencies which will actually produce a decentralization of research contracts and grants.

9) Studies of the kinds of basic research industry would be willing to support through grants.

10) Explorations between state universities and the various agencies or departments of state government as to the types of research appropriate to universities, which the state needs for continued development.

As colleges and universities, federal and state agencies and departments, and industries learn to work together developing additional methods, of which these are only illustrations, perhaps the nation can approach the goal of maximum benefit from all its qualified human research resources and eliminate such artificial shortages as may exist.

POSITION IS EVERYTHING

At the beginning of the Time-Scale men and apes are hardly distinguishable. The Erect-Ape-Men appear to have neither articulate speech, nor traditions, nor accumulated knowledge. They have few means of defense or aggression except the physical force of their bodies and the instinctive aptitudes provided by their biological inheritance. To us they appear to be associating and contending with the apes on fairly equal terms. But if we turn to the end of the Time-Scale (April 23, 1935), we can see at once that something has happened: nothing to the apes, but something to man. The apes look and behave at the end of the Time-Scale very much as they did at the beginning: during 506,000 years they have repeated their activities instead of extending them. But man at the end of the Time-Scale is not what he was at the beginning. He no longer contends and associates with his cousins the apes. He puts them in the Zoo. And if, some fine morning, he should encounter his ancestor *Pithecanthropus* on University Avenue, he would no doubt, failing to recognize the old man, put him in the Zoo also. Imagine then some descendant of *Pithecanthropus* standing in the Zoo, looking up at his remote ancestor in the cage. In what essential respects do they appear to differ? As biological specimens they appear to us not too unlike; and if the man finds the antics of *Pithecanthropus* amusing, it is chiefly because they parody his own on the less formal occasions of life. If the man should be suddenly whisked to the beginning of the Time-Scale and dropped, naked and without appliances, among his ancestors of Java, the amusement, if any, would not be his. All the biological process of 506,000 years would lead him swiftly to the final, good or bad, end of extinction. Fortunately for the man, he is not at the beginning but at the end of the Time-Scale, in the Zoo, looking up at *Pithecanthropus* with amusement. The reason he can afford to be amused is a simple one: he is on the Outside of the cage, *Pithecanthropus* is on the inside.*—*Progress and Power*, Carl Becker

* Carl Becker, *Progress and Power*. Copyright 1935 by Alfred A. Knopf, New York, and reproduced with the publisher's permission.

Reconnaissance Studies of the Green River*

H. E. THOMAS†

The author is Staff Geologist for the Ground Water Branch of the U.S. Geological Survey and has held this office since July, 1952. He received his Ph.D. in geology from the University of Chicago and taught geology at Ohio State University before joining the Survey in 1929. He was district geologist in charge of ground-water investigations of the Salt Lake City, Utah, district from 1939–49, except for a period of service as Lieutenant-Colonel, Corps of Engineers, during World War II. He is the author of The Conservation of Ground Water *and is an outstanding authority in this field.*

CIVILIZATION has long since engulfed the West, and most of the hardships and romantic adventure that characterized the "Wild West" of the nineteenth century now exist only in movies, television, western fiction, and the nostalgic reminiscences of old timers. But there has been very little change in the canyons through which the Colorado River and its principal tributary, the Green River, traverse numerous mountain ranges and plateaus. In Utah these rivers are accessible in very few places to the man with a car. Their tortuous courses can be followed only by boat, and the trip is about as hazardous as when Major John W. Powell first made it in 1869. An outboard motor will now take a boat rapidly through the numerous stretches of smooth water, but the boat arrives sooner at the next swirling, rock-studded, and treacherous rapid, where manual dexterity alone wards off disaster. A trip down these rivers is an outstanding adventure, and one that has been experienced by very few, because numerous tragedies over the years have proved the folly of attempting it without the help of experienced rivermen.

Several expeditions through the canyons have been made by engineers and geologists of the U. S. Geological Survey. From these expeditions have come plans and profiles of the Green River, notes on the geology[1] and studies of utilization and flood control of the river.[2] In recent years Survey engineers, notably LaPhene Harris and the late Norman Nevills, have been among the best navigators of white water in the West (Fig. 1).

* Publication authorized by the Director, U. S. Geological Survey.

† Staff Geologist, Ground Water Branch, Water Resources Division, U. S. Geological Survey, Salt Lake City, Utah.

Purpose of the Boat Reconnaissances

The waters of the Colorado River basin are of major concern to seven Western states, and echoes of the arguments as to who shall benefit from the development of those waters have doubtless been heard in all parts of the nation. Our knowledge of the water available for development in the basin is based almost entirely upon records obtained at a network of gaging stations, each of which provides continuous records as to the quantity of water passing the station, and some of which provide records also of the suspended sediment, the dissolved materials carried by the water, or both (Fig. 2). Comparison of records from successive gaging stations along a stream shows the net effect of all gains to or losses from the stream in the intervening reach: gains by tributary inflow, ground-water inflow, and precipitation upon the water surface; and losses by diversions, ground-water outflow, evaporation from the water surface, and transpiration of riparian vegetation. If the hydrology of the region had been studied in sufficient detail, it would be possible to discriminate these various gains or losses in stream flow.

Unfortunately, not enough is known about the Colorado River basin to explain the vagaries of discharge of the river or its principal tributaries. This is particularly true of the Upper Basin, which is the 110,000-square-mile drainage area above Lee Ferry, Arizona. Detailed studies to show the occurrence of ground water and its relation to

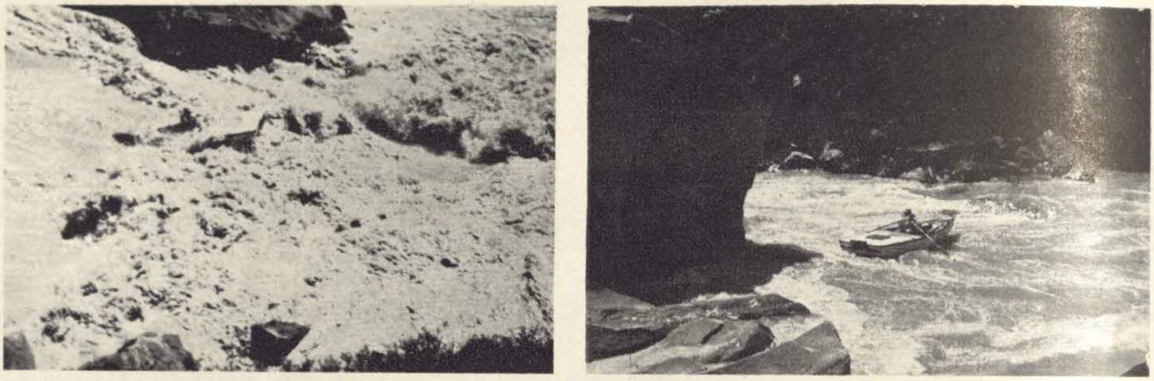


FIG. 1. Ashley Falls, in Red Canyon of the Green River. *Left,* June 1947; LaPhene Harris, navigator. *Right,* September 1947; Harold Chase, navigator.

stream flow, the natural discharge of water by evapotranspiration, the water-bearing properties of the rocks of the basin, and the relation of the rock strata to the dissolved minerals and suspended sediment in the streams, have not been made for any part of the Upper Basin.

Far more basic hydrologic data than are now available will be required for administration of the Upper Colorado River Basin Compact of October 11, 1948. The basis for apportionment of water is contained in Article III of the compact, and Article VI states:

> The Upper Colorado River Commission shall determine the quantity of the consumptive use of water, which use is apportioned by Article III hereof, for the Upper Basin and for each State of the Upper Basin by the inflow-outflow method in terms of man-made depletions of the virgin flow at Lee Ferry, unless the Commission, by unanimous action, shall adopt a different method of determination.

It is evident that proper apportionment of the water in accordance with the compact will require information as complete as possible concerning the quantities of water, both surface and subsurface, that cross state boundaries, and the relation of those quantities to the flow in the Colorado River at Lee Ferry. With respect to each project for development of the water resources in the Upper Basin, the compact requires a determination of the "quantity of consumptive use of water . . . in terms of man-made depletions of the virgin flow at Lee Ferry," a determination that requires knowledge of the quantities diverted for the project and the quantities returned to the stream, as well as the difference between natural losses before and after the project begins operation. Inasmuch as the consumptive use of water by each state is calculated in relation to its depletion of the virgin flow at Lee Ferry, it becomes essential to know the extent of the natural losses from the river, before these virgin-flow conditions are changed by the development of projects.

In so large an area as the Upper Colorado River basin, complete information regarding sources and movements of water, measurement of water crossing state boundaries, and determination of natural losses from the river system will require large expenditures and a large corps of hydrologists. It is likely, however, that investigation of the main stems of the Colorado and its principal tributaries will show that some parts of the drainage basin make negligible contributions to the stream flow, and that study of those areas can be deferred. In Utah those main stems are in deep canyons throughout most of their courses, and not easily accessible for study. The best method of coverage of the entire course of the river is by boat. Some of the information obtained during boat trips—for instance, the data concerning the relation of stream flow to the regional geology and ground-water hydrology—is considered to be pertinent to the hydrology of the basin at all seasons. Measurements of stream discharge and of the mineral constituents in selected water samples, however, provide only fleeting glimpses of the continually changing conditions in the basin. Important clues to the hydrologic relationships of the basin may be derived by analysis of these reconnaissance data in relation to the continuing records from established gaging stations along the Colorado River and its tributaries.

The Green River in Utah and Colorado

The Green River (Fig. 3) barely crosses the state line from Wyoming into Utah before it plunges into a succession of canyons, including the Flaming Gorge, Horseshoe, and Kingfisher Canyons, and then turns eastward through Red Can-

yon to skirt the highest part of the towering Uinta Mountains. In Colorado it flows lazily through the relatively open Browns Park, then turns southward through Lodore Canyon to Echo Park, and then back into Utah through Whirlpool Canyon. After another short calm stretch in Island Park, it bounces down through the sharp arch of Split Mountain before emerging in the broad Uinta Basin near Jensen, Utah. There for several miles are ranches here and there along its banks, which can be reached by road in several places. Then the river traverses Desolation Canyon and Gray Canyon before emerging briefly in the broad Gunnison Valley around Greenriver, Utah. The lower 95 miles of the river's course is in Labyrinth Canyon and Stillwater Canyon. Its confluence with the Colorado River is practically inaccessible except by boat.

Four reconnaissance trips have been made by boat down these canyons of the Green River for the purpose of measuring all tributary inflow and determining the discharge of the river at numerous sections not included in the gaging station network. These boat trips were made by the Water Resources Division of the Geological Survey, under the direction of M. T. Wilson, district engineer in Salt Lake City. Three were made in September and October of 1946, 1947, and 1948, when the discharges of the river and tributaries were at or near the minimum for the year. One trip was made in June, 1947, during the period of maximum discharge from melting snow.

These reconnaissance trips provided valuable data as to the tributary inflow and other hydrologic factors. The data collected in the 1948 reconnaissance provided a better opportunity to evaluate the effect of evapotranspiration losses and ground-water gains upon the inflow of the stream, for the following reasons: gaging stations recently established on the Green River near Jensen and near Ouray in the Uinta Basin permitted analysis of the discharge at those points throughout the period of the reconnaissance; the discharge of the river in September, 1948, was lower than at any time since 1940, and the gains and losses by ground water were accordingly a larger proportion of the flow; and because of favorable meteorological conditions there was very little storm runoff during the 1948 reconnaissance, in contrast particularly to the condition during the 1946 trip, when storms created a flood wave with peak discharge more than double the minimum flow of the preceding week.

The 1948 reconnaissance was made during the period September 14 to 29. The program for this reconnaissance included, as for previous trips, the measurement of the main-stem discharge at numerous sections and the determination of all tributary inflow. In addition, observations were made as to ground-water inflow, losses by evaporation and transpiration, and the geologic conditions that might be expected to affect the stream flow. The geology and ground-water hydrology of the Green River along the 439-mile channel from the Wyoming state line to the confluence with the Colorado River have been summarized elsewhere.[3]

Stream Gains and Losses During Boat Trip 1948

In 1948 the minimum discharge of the Green River was recorded at Linwood, Utah (near the Wyoming state line) on September 17, and at Greenriver, Utah, about a week later. Thus the boat trip of September 14–29 coincided approxi-

Fig. 2. Gaging station on the Green River at Jensen, Utah. During low stages, stream velocities and cross-sectional areas are determined by wading measurements using the Price current meter. Cable car is used for determining concentration of suspended sediment, using D-43 sampler, and for measuring discharge at higher stages. Continuous record of stage is maintained by recording gage, in shelter on bank.

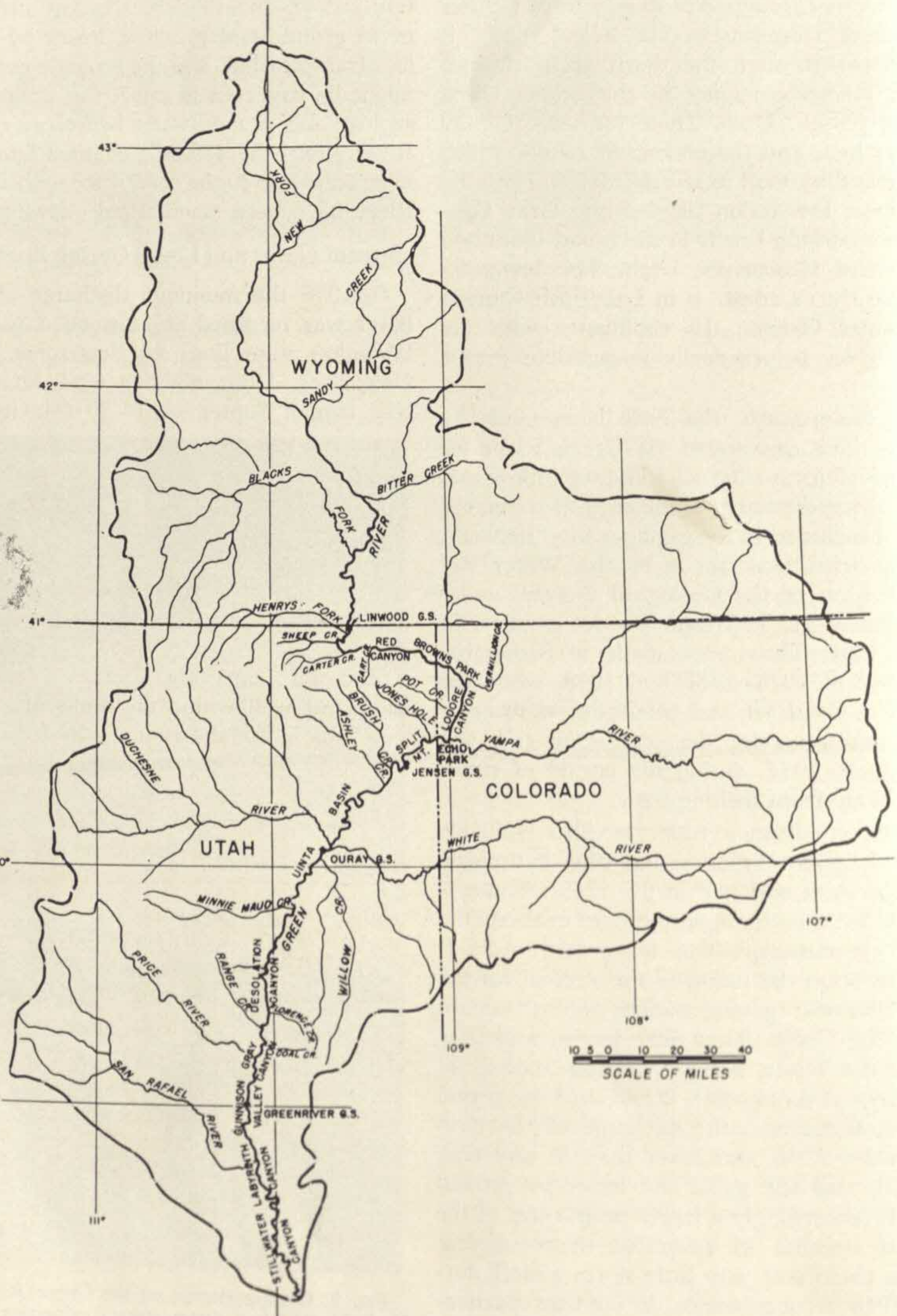


Fig. 3. Map of Green River basin.

mately with the period of minimum flow for the year. Quantitative estimates of the gains or losses due to ground water or evapotranspiration can be made for this period of minimum flow, provided that channel and bank storage at the beginning and end of the designated period are approximately equivalent. The period selected for analysis is therefore one in which the river stage at the beginning and at the end is approximately the same and is not subject to rapid change. The selected period progresses downstream with the approximate velocity of the river flow and is thus about a week later at Greenriver than at Linwood.

The average discharge of the river during the selected 21-day base period was computed from the records at the four gaging stations to be 515 cubic feet per second (cfs) at Linwood, 678 cfs at Jensen, 1,027 cfs at Ouray, and 1,074 cfs at Greenriver. On the basis of measurements during the boat trips and comparison with the gaging station records, the discharge of the Green River at its mouth was estimated to average about 974 cfs during the base period.

Inflow from Tributaries. The principal tributary inflow to the Green River during the 1948 boat trip was from the White River (340 cfs), Yampa River (94 cfs), Jones Hole Creek (30 cfs), Duchesne River (22 cfs), Carter Creek (20 cfs), and Minnie Maud Creek (12 cfs). Gaging stations on the White, Yampa, and Duchesne Rivers several miles above their mouths provided records which could be used in estimating the average flow in the 21-day period, and adjustments were made also for some of the smaller tributaries. The total tributary inflow to the Green River in Utah and Colorado in the 21-day base period was computed to be 560 cfs, of which about 350 cfs came from the White and Duchesne rivers, and about 175 cfs from Yampa River and minor tributaries, such as Gorge Creek, between Linwood and Jensen (Fig. 4).

Gain from Precipitation. During the 21-day base period rain was recorded at every Weather Bureau station in the Green River basin. During the storm of September 16–19, 1948, it exceeded 0.25 inch in the Uinta Range and headwater areas of the Green and Yampa Rivers, and reached a maximum of 0.94 inch near the northern tip of the basin. The storm of September 25–30 covered only the southern half of the basin; the recorded rain was less than 0.25 inch except in the headwaters of the Yampa, where it reached a maximum of 1.61 inches.

These storms had a readily detectable effect

FIG. 4. Measuring the flow of Gorge Creek into the Green River in Red Canyon, September 1947.

upon runoff. The precipitation in the northern part of the basin during the first storm caused an increase in flow at Linwood three days after the rain had ceased, and at Jensen two days later. The high areas in northeastern Utah also received enough precipitation during this period to cause storm runoff at Jensen and Ouray as early as September 19. The storm of September 25–30, which missed the northern part of the basin, caused no storm runoff at Linwood and Jensen but resulted in slightly increased flow at Ouray and later at Greenriver.

The total storm flow during the base period is small, and well within the limits of error of the stream-discharge determinations. It is equivalent to an average during the base period of 8 cfs at Linwood, 12 cfs at Jensen, 25 cfs at Ouray, and 35 cfs at Greenriver. Thus, the inferred storm runoff was about 1.5 per cent of the total flow at Linwood and increased to 3 per cent of the total flow at Greenriver.

Loss by Evaporation. The area of water surface is shown on topographic maps at scale 1: 31,680 prepared during river surveys in 1904, 1914, and 1922. In September 1948 the river discharge was less than in corresponding months in those years, and the river level was undoubtedly lower than at the time of the surveys. The width of the river is arbitrarily assumed to have been 10 per cent less in canyons and 20 per cent less in other reaches than shown on the topographic maps. Thus computed, the water surface of the Green River in Utah and Colorado in September 1948 was about 19,000 acres, including more than 13,000 acres of smooth shallow water in Browns Park, Uinta Basin, and the meandering reaches of the river below Greenriver.

The rates of evaporation from water surfaces have been estimated from records of U. S. Weather

Bureau evaporation stations at Fort Duchesne and Vernal, Utah. Several investigators[4,5] have reported that the evaporation from a Weather Bureau class A land pan is about 50 per cent greater than that from a reservoir surface, and Follansbee[6] accordingly has multiplied the land-pan evaporation by a factor of 0.69 to derive the evaporation from reservoirs. Sleight found also that evaporation from slowly flowing water was about 7 per cent greater than that from still water, but he did not determine quantitatively the relation of stream velocity to evaporation. In the absence of experimental data to show the rate of evaporation from turbulent flow such as is encountered in the rapids of the Green River, it is assumed that evaporation from smoothly flowing water is equivalent to 85 per cent of the land-pan evaporation, and that evaporation from turbulent water is equal to the evaporation as recorded at land pans. These factors are approximately equivalent to those used by the Bureau of Reclamation, in unpublished notes, to compute the evaporation from the river surface. No allowance has been made for the moderate to strong upstream winds which are characteristic of the canyons, and which would increase the evaporation opportunity above that of the Weather Bureau evaporation stations, where average wind velocities may be less.

The evaporation from the river surface during the base period in 1948 is computed to have been at an average rate of 150 cfs, of which about 10 cfs was lost in Browns Park, 45 cfs in Uinta Basin, and 70 cfs in the Gunnison Valley and lower reaches of the river. According to these computations, evaporation was at an average monthly rate of about 5¾ inches from the water-surface area of the Green River in Utah and Colorado.

Loss by Diversion. Small quantities of water are diverted from the Green River in Utah and Colorado for irrigation of adjacent flood plains. The largest of these diversions is in Gunnison Valley near the town of Greenriver, where diversions may be as much as 80 cfs during the irrigation season. The quantities of water diverted have not been measured, nor have the irrigated areas been determined. Instead, the irrigated areas have been included in the total area of evapotranspiration losses, and the diversions are thus included among the natural losses from the river.

Loss by Evapotranspiration. The river doubtless loses some water by evaporation from moist banks and by transpiration of vegetation, especially willows and saltcedars that line the banks in many places. Except in the canyons the river is bordered by flood plains, which are covered in most places by water-loving vegetation such as willow, saltcedar, cottonwood, tule, marsh grass, and salt grass. These plants draw ground water from under the flood plain. It is likely that some of the water used by these plants in September 1948 was not replaced until the river reached a higher stage—perhaps not until the 1949 freshet. However, water was observed seeping into the river from bank storage at many places during the 1948 boat trip; this ground-water seepage to the river might well have been greater but for the transpiration of flood-plain vegetation.

Gaging station records show diurnal fluctuations in river discharge during September 1948 amounting to as much as 12 cfs at Linwood and 18 cfs at Ouray in the Uinta Basin. These diurnal fluctuations afford a minimum measure of the total evapotranspiration draft from river surface and flood plains. A large proportion of the water lost by evapotranspiration may be derived from tributaries and from ground water moving in the alluvium toward the river. Particularly in the Uinta Basin, it is likely that the water so lost comes chiefly from the basin area rather than from the main stem of the river. Nevertheless, the total draft of evapotranspiration in the channel and flood plain of the river represents a depletion from the river of water that would otherwise continue downstream.

The total area of flood plain bordering the water surface of the Green River in Utah and Colorado was about 41,000 acres in September 1948; this total included 20,700 acres in the Uinta Basin, 6,700 in Browns Park, and 6,400 in Gunnison Valley.

The rates at which water is used by the flood-plain vegetation cannot be computed accurately from available data. Studies throughout the West[7] have indicated that willow, cottonwood, and salt grass use water at rates of 60 to 85 per cent of the evaporation from a standard Weather Bureau class A land pan; saltcedar, tules, and cattails use considerably more, but greasewood uses less. The amount and density of cover by individual types of plants have not been mapped, however, and the depth to the water table, which is an important factor in the rates of transpiration by these water-loving plants, is not known in any part of the flood plain.

In order to arrive at an estimate of the total flood-plain evapotranspiration, it is assumed that the average rate was 80 per cent of the recorded evaporation from the nearest Weather Bureau

land pan. By this assumption, the evapotranspiration during September 1948 from the 41,000 acres of flood plain was at an average rate of 3 1/3 inches, which resulted in a water loss at the rate of 280 cfs. This rate is somewhat greater than that computed on the basis of Thornthwaite's[8] "potential evapotranspiration," and somewhat less than that computed on the basis of Blaney and Criddle's[9] "water requirements" for medium to dense native vegetation.

Computed Inflow of Ground Water. The observed ground-water inflow to the Green River totalled 12 cfs during the 1948 boat trip, of which about half came from warm springs in Split Mountain Canyon. In many other places the geologic formations and structure appeared to be favorable for contributions of ground water to the river channel, even though such contributions could not be observed.

The total ground-water inflow during the 21-day base period in 1948 may be computed as the difference between the sum of the gains by inflow from Wyoming and from tributaries, and the losses by discharge into the Colorado River, water-surface evaporation, and flood-plain evapotranspiration.

The boat reconnaissance also provided data as to the relative amounts of ground-water inflow in various segments of the river. In at least two areas, analysis of these data and of the stream discharge as recorded at numerous gaging stations shows the desirability for additional hydrologic research.

Uinta Basin. About 166,000 acres of land in the Uinta Basin are irrigated by diversions from tributaries of the Green River, including especially the Duchesne River. The inflow to the basin is measured at gaging stations above these diversions, but the gaging stations near the mouths of the tributaries and below the irrigated areas measure only a part of the outflow: in September 1948 the combined discharge of the Duchesne River, Ashley Creek, and Brush Creek was only 25 cfs, but the computed ground-water flow to the Green River flood plain within the Uinta Basin was about 240 cfs. This ground-water inflow could not be discriminated in the records of discharge at Greenriver (below the Uinta Basin), for that discharge in September 1948 was only about 30 cfs greater than the discharge at Jensen (where the river enters the Basin) plus the inflow from the White and Duchesne Rivers and minor tributaries. Thus it is evident that the evapotranspiration losses between the gaging stations were nearly as great as the computed ground-water inflow.

Gains and Losses in Green River, September 1948

Gains, cfs		Losses, cfs	
Inflow from Wyoming	515	Discharge to Colorado River	974
Tributary inflow	560	Water-surface evaporation	150
		Floor-plain evapotranspiration	280
	1,075		1,404
Computed ground-water inflow	330		
	1,405		

Of the computed ground-water inflow of 330 cfs, 65 cfs was contributed to the river in the 123-mile segment between Linwood and Jensen; 243 cfs in the 133-mile segment within the Uinta Basin; and only 22 cfs in the lower 180 miles of river channel.

Of course, the unmeasured ground-water inflow and evapotranspiration loss are larger in relation to the stream flow during periods of minimum discharge than at other times. Nevertheless, an accurate evaluation of the consumptive use within the Uinta Basin by the inflow-outflow method "in terms of man-made depletions of the virgin flow at Lee Ferry" (as required by compact) will require more hydrologic data than can be provided by the present network of stream-gaging stations. The Uinta Basin is the principal area of ground-water inflow to the Green River, and may well be the largest contributor of ground water in the entire Colorado River Basin. The quantitative determination of this contribution is possible with adequate hydrologic data, and these quantities can be checked against the gains and losses in stream flow

Conclusions

Information collected during the period of minimum discharge in 1948 indicates that the water in the Green River at the Wyoming state line (515 cfs) would have been reduced about 85 per cent by evapotranspiration losses of 430 cfs in Utah and Colorado, if there had been no inflow. The inflow in those States, however, totalled about 890 cfs, of which 560 cfs was contributed by tributaries and about 330 cfs by ground water, and the flow of the river into the Colorado in this period of minimum discharge was about 974 cfs.

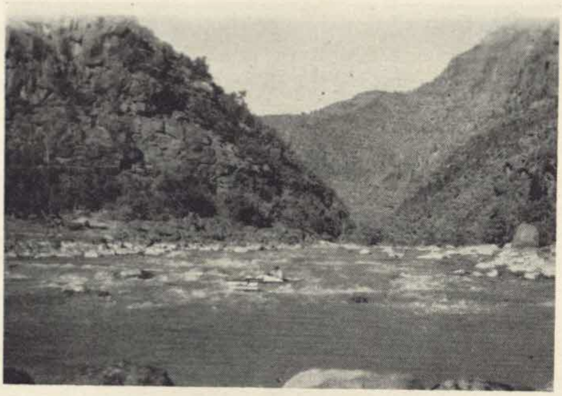


FIG. 5. Triplet Falls in Lodore Canyon, above Echo Park.

FIG. 6. Quiet water in Whirlpool Canyon below Echo Park.

and the beneficial consumptive use and other evapotranspiration loss within the basin.

Proposed Echo Park Reservoir. The proposed Echo Park Reservoir, on the Green River in northwestern Colorado (Figs. 5 and 6), is admirably suited to storage of water with minimum losses by evaporation. However, part of the reservoir area is underlain by cavernous Mississippian limestone, one of the most permeable formations in the region. As shown by records in September 1948, there are no measurable losses into these limestones at low stage. Between the gaging stations at Linwood and Jensen the computed evapotranspiration losses at that time were at an average rate of about 80 cfs, and there must have been about 65 cfs of groundwater inflow.

The Green and Yampa Rivers *do* lose water at flood stage as they cross the Uinta uplift—that is, between the Wyoming state line and Jensen on the Green, and below Cross Mountain on the Yampa.[10] These losses range from 50,000 to 80,000 acre-feet during May, June, and July, and are probably due in part to replacement of water taken by evapotranspiration from some 18,000 acres of flood-plain areas, especially in Browns Park, Island Park, and Lily Park. Gaging stations are not numerous enough to discriminate the losses, if any, by seepage into the limestone. Geologic conditions do not appear conducive to major losses from the reservoir by seepage into the limestone, but no guarantee can yet be made against such losses, because large areas underlain by the Mississippian limestone have not been mapped topographically or geologically.

An adequate accounting for these losses from the Green and Yampa Rivers during flood stages would require detailed studies in areas where large evapotranspiration losses are suspected—notably Browns Park—and studies of the geology and hydrology of the area south of the reservoir site, to determine whether any losses from the proposed reservoir may occur by leakage into the limestone.

References

1. REESIDE, J. B., JR. Notes on the Geology of Green River Valley between Green River, Wyo., and Green River, Utah. U. S. Geol. Survey Prof. Paper 132-C, 35–40 (1923).
2. WOOLLEY, R. R. The Green River and Its Utilization. U. S. Geol. Survey Water-Supply Paper 618, 450 pp. (1930).
3. THOMAS, H. E. Hydrologic Reconnaissance of the Green River in Utah and Colorado. U. S. Geol. Survey Circ. 129, 5–16, pl. 1 (1952).
4. SLEIGHT, R. B. *J. Agr. Research*, **10**, 231 (1917).
5. ROHWER, C. *U. S. Dept. Agr. Tech. Bull. 271*, 44 (1931).
6. FOLLANSBEE, R. *Trans. Am. Soc. Civil Engrs.*, **60**, 705 (1934).
7. YOUNG, A. A., and BLANEY, H. F. *Calif. Dept. Public Works Bull. 50*, 126 (1942).
8. THORNTHWAITE, C. W. *Geog. Rev.*, **38**, 56 (1948).
9. BLANEY, H. F., and CRIDDLE, W. D. Consumptive Use of Water in Irrigated Soil Areas of the Upper Colorado River Basin (provisional): U. S. Dept. Agr., Soil Cons. Service, 49 pp. (1949).
10. THOMAS, H. E. A Hydrogeologic Problem in the Proposed Echo Park Reservoir. U. S. Geol. Survey Mimeo. Rept., 6 pp. (1953).

On Absolute Measurement*

N. ERNEST DORSEY† AND CHURCHILL EISENHART‡

Dr. Dorsey did both his undergraduate and his graduate work at The Johns Hopkins University, receiving his A.B. in 1893 and his Ph.D. in Physics in 1897. After teaching at The Johns Hopkins unitl 1901, he joined the U S. Department of Agriculture. In 1903 he went to the Bureau of Standards as an assistant Physicist, later becoming Physicist until his retirement in 1943. He is a former associate editor of International Critical Tables, *and the author of numerous scientific papers and books. Dr. Eisenhart has served as Chief of the Statistical Engineering Laboratory of the National Bureau of Standards since 1946. He became a member of the faculty of the University of Wisconsin in 1937. During World War II he was engaged in mathematical research on aerial gunnery and rockets. Dr. Eisenhart is the author of more than 50 technical papers and is one of the editors and authors of the book* Selected Techniques of Statistical Analysis.

BY an absolute measurement of a physical quantity, such as the velocity of light, is meant the determination of the value of that quantity in terms of the significant fundamental units of length, mass, time, etc., and of those constant parameters that characterize the accepted system of theoretical equations that connect the several pertinent quantities. (p. 9.)

Theory of Errors

The mean of a family of measurements—of a number of measurements of a given quantity carried out by the same apparatus, procedure, and observer—approaches a definite value as the number of measurements is indefinitely increased. Otherwise, they could not properly be called measurements of a given quantity. In the theory of errors, this limiting mean is frequently called the "true" value, although it bears no necessary relation to the true quaesitum, to the actual value of the quantity that the observer desires to measure. This has often confused the unwary. Let us call it the limiting mean.

Let e denote the amount by which a given member of the family departs from the limiting mean, and let e_q denote that value which in the indefinitely extended family is surpassed by half of the e's; that is, it is an even chance that a given member of such an extended family departs from the limiting mean by as much as e_q.

The quantity e_q, the quartile error, commonly called the probable error of a single observation, will in this study be called the technical probable error of a single member of such a family. (p. 4.)

It should be noticed that the technical probable error either of a single measurement or of the mean of a group of n measurements indicates merely the closeness with which that measurement or mean probably approaches the limiting mean. It tells nothing whatever about the actual quaesitum, and so it is of very minor interest to the experimental physicist engaged in absolute measurements.

To him its main interest is threefold: (a) It tells him when it has become profitless to take additional routine observations; but in most cases other and more important considerations set another limit. (b) It may enable him to state positively that a systematic error affects one or both of two rival families of measurements. (c) It, as applied to a relatively small number of observations, enables him to state positively that systematic errors smaller than a certain amount cannot with certainty be detected experimentally with the apparatus and procedures employed in obtaining those measurements.

The last is, for him, by far the most valuable property of the technical probable error. But in practice he seldom thinks of it in that connection. By what seems to be a kind of intuition, he recognizes rough numerical relations between the mini-

* Excerpts from introductory "Remarks" of N. Ernest Dorsey's "The Velocity of Light" (*Transactions of the American Philosophical Society,* Vol. 34, pp. 1–110, 1944), selected and arranged by Churchill Eisenhart.

† Physicist (retired), National Bureau of Standards. Associate editor, *International Critical Tables,* 1922–1929.

‡ Chief, Statistical Engineering Laboratory, National Bureau of Standards.

mum detectable error and the mean deviation of the several determinations from their mean. And he studies those deviations without thinking about the technical probable error. Actually, the relations he uses are practically those that may be derived in the following manner from the technical probable error.

The argument runs as follows: If the means of two groups of measurements do not differ by at least the sum of their technical probable errors, then the existing difference is not sufficient to justify the assumption that they do not belong to the same statistical family. Consequently, if the only basic difference between the groups were the presence in one of a systematic error that was absent from the other, then the presence of that error could not be certainly established from the difference, unless it amounted to at least the sum of the two technical probable errors. Conversely, it cannot be proved that the measurements are not affected by such an error. (pp. 5 and 6.)

. . . the term "systematic error" is used to cover all those errors which cannot be regarded as fortuitous, as partaking of the nature of chance. They are characteristics of the system involved in the work; they may arise from errors in theory or in standards, from imperfections in the apparatus or in the observer, from false assumptions, etc. To them, the statistical theory of errors does not apply. They are frequently called "constant errors," and very often they are constant throughout a given set of determinations, but such constancy need not obtain. For example, if the value found by a certain measurement depends upon the humidity of the air, which the experimenter fails to record, thinking that it is of no consequence, then the measures will be affected by a systematic error which will, in general, vary throughout the day and especially from day to day. (p. 6.)

Averaging

Any set of numbers may be weighted as desired, and summed and averaged, and the result can be carried out to as many digits as one may wish. The procedures are simple, exact, and not open to any question or criticism. They are purely arithmetical.

But if the numbers represent physical quantities, then questions arise concerning both the validity of averaging and the number of digits that have a physical significance.

1) It is sometimes forgotten that the averaging of a set of values, even of the same kind, may be a physically invalid procedure. That is, that the average may not deserve greater confidence as an estimate of the quaesitum than do the individual values.

For example, consider a series of sets of determinations, each set being affected by a systematic error peculiar to it; that error being constant throughout any given set, but varying from set to set. Superposed on that error are fluctuating errors of various kinds. These last are minimized, set by set, by averaging the determinations composing a set. This averaging is entirely proper. But it leaves one with a series of values that differ, one from another, on account of the presence of systematic errors peculiar to each. In general, the averaging of such a series of values will be quite invalid; in general, the average will not deserve more confidence than do the individual values. The only cases in which it will be justifiable when the values differ by more than can be accounted for by the irregularities inherent in each of the several sets, are three: those in which it is definitely known—or perhaps is very highly probable—that the variation in the systematic error from one value to another either is (a) strictly fortuitous, in which case the fluctuating part of the error is minimized by the averaging, or (b) arises from the error fluctuating between equal and fixed positive and negative values, the number of positive values being essentially equal to the number of negative ones, or (c) arises from the error varying progressively from a positive value to a negative one as certain uncontrolled conditions change, and those conditions are known to vary in such a way that each negative error will in the long run be matched by an equal positive one.

Only when one knows a great deal about the systematic error can one be sure that any of these conditions are satisfied. And when he knows that much, he can often arrange to eliminate, or to evaluate, the error; and he should do so.

The cases that most frequently give trouble are those in which the data give evidence of the presence of a systematic error, but the experimenter does not know its source, and those in which another studying the data finds evidence of a systematic error that was overlooked by the experimenter. In such cases one may not know how the error varies with the conditions. If it makes all the values too great, then the smaller ones will be better than the average. Or the reverse may be true. Or the error may be present in some and absent from others; then averaging will not improve things.

Under such conditions it is quite improper to present the average as being superior to the individual values.

One is never justified in merely guessing that averaging will minimize or eliminate the effect of a systematic error. He must know it, must know that under the actually existing conditions the error is so minimized or eliminated.

In the absence of such knowledge, the proper brief summation of the work would seem to consist in a giving of the extreme values with a statement that at least some of the values seem to be affected by a systematic error of unknown origin. To this might well be added the experimenter's opinion, and if he wishes, the arithmetical average, with a clear statement of its questionable value. To give merely the average tends to mislead the reader, to blind him to the presence of systematic errors. The reader must always be on guard, as it is not very uncommon for a writer to average his results quite invalidly, either because he has not awaked to the fact that averaging may be invalid or because he has failed to recognize the evidence for the existence of systematic error.

2) The number of digits that are of physical significance in the sum and in the average must be carefully considered. (pp. 6 and 7.)

Quaesitum

The quaesitum of the investigation is the actual value of the quantity. The particular value yielded by a given apparatus, procedure, and observer is of no interest in itself, but only in connection with such a study as will enable one to say with some certainty that the value so found does not depart from the quaesitum by more than a certain stated amount. No investigation can establish a unique value for the quaesitum, but merely a range of values centered upon a unique value. The quaesitum may lie anywhere within that range, but the wiser and more careful the experimenter's search for systematic errors, and the more completely he has eliminated them, the less likely is it to lie near the limits of the range. The wider the range, the less becomes the physical significance of the particular value on which the range is centered. (p. 9.)

Definitive Value

The term "definitive value" is used in two distinct, though related, senses. (*a*) In a narrower, particular sense, it denotes the value that is believed to lie as near the quaesitum as any that can be legitimately derived from the observations taken in the course of the work being reported. It is the ultimate or definitive value to which that work itself leads. It is often called the "final" value of the work. (*b*) In a broader, general sense, it denotes the value that is believed to lie as near the quaesitum as any that can be derived from a consideration of all the determinations that have been made, and of all other available pertinent information. Whenever not otherwise indicated by the context or a modifier, it is in this broader sense that the term is to be understood.

Every report of measurements of a physical quantity should state clearly the particular definitive value to which those measurements lead. It may also give the broader definitive value based on everything that is known. But the two should not be confused, as unfortunately they often are. (p. 9.)

Dubiety

The determination of the range is of an importance that is secondary only to that of its center. No absolute measurement has been completed until values have been established for both of those quantities. The determination of the range necessarily involves an element of judgment, and the limits cannot be set with precision. Nevertheless, it is possible to assign a lower limit; and although no fixed upper limit can be assigned, it is possible to say that if suitable care and diligence had been employed, it is not likely that the range exceeds a certain specified value.

In order to distinguish this range from the numerous kinds of "errors" that abound, its half will in this study be called the "dubiety" of the value found. If that value be denoted by V, and the dubiety by D, then the quaesitum will likely lie within the range $(V-D)$ to $(V+D)$. By this, one means that nothing has come to light in the course of the work to indicate that the quaesitum lies outside that range.

The dubiety is made up of three distinct additive terms to which it is convenient to give descriptive names. They are as follows:

Mensural dubiety arises from the uncertainties in the several primary measurements and in the elimination of known systematic errors. It is common practice to take the arithmetical sum of the effects of these individual uncertainties as an upper limit for the mensural dubiety.

Discordance dubiety arises from the fact that the discordance in the individual determinations limits the smallness of a systematic error that can be experimentally detected. The result cannot be less dubious than the size of the largest systematic error that can escape detection. This term of the dubiety is generally the most important by far, and the least understood and least appreciated by those who are not experimentalists.

Deficiency dubiety arises from the determinations

being too few; in particular, finite in number. It is equal to the technical probable error of the result. This term, much honored by those not skilled in experimentation, is always smaller than the discordance dubiety and frequently is negligible in comparison therewith.

Of these three terms, the second alone needs to be especially considered here. In searching for systematic errors, the logical procedure is to make a series of measurements, then to change something and to make another series, and to compare the means of the two groups. This will be repeated as often as may seem necessary. None of the series can be long, for an extended delay offers opportunity for unanticipated changes to occur. If the two means being compared do not differ by more than the sum of their technical probable errors, their difference is of no physical significance—it proves nothing. Hence, the presence of a systematic error that does not exceed the sum of the technical probable errors of the two groups of observations used in the search cannot be established without great difficulty, if at all. That sets a minimum limit for the discordance dubiety. (pp. 9 and 10.)

Obviously, no one should claim a discordance dubiety that is smaller than the smallest systematic error that he might certainly have detected by the tests he made. But there may be reasons that seem to him sound for believing that the actual dubiety is smaller than that. In such case he may, and generally should, state his belief and the reasons therefor; but the statement should never be of such a kind as to lead the reader to confuse the writer's estimate with the minimum discordance dubiety as just defined. (p. 10.)

But on comparing a series of determinations made by different persons with significantly different apparatus and procedures, it may be found that the several members of the series agree more closely than their individual dubieties would lead one to expect. Then if the differences in apparatus and procedure are sufficiently fundamental, one might be justified in thinking it very improbable that the quaesitum lies far outside the range of the means of the several members of the series. And from the whole he might infer a smaller range of possible values than that demanded by the dubieties of the several determinations. (p. 10.)

No one is really interested in how near the quaesitum the definitive value may possibly lie, for he knows that by chance the two may coincide even though the work be very poorly done. But one does keenly desire to know how far the two are likely to differ—how dubious the definitive value may be. And it is the plain duty of the experimenter not merely to show that his definitive value may be that of the quaesitum, but to prove that it is unlikely to depart from the quaesitum by more than a certain stated amount. In order to obtain the information needed to meet that demand, the careful experienced investigator will proceed somewhat as follows. (p. 10.)

Procedure

Before one undertakes an absolute measurement in physics, he will make a careful theoretical study of the problem, including, among other things, methods of attack, sources of errors and how they can be avoided or eliminated, and types of computation. On the basis of that study, the apparatus will be constructed and set up. Only then does the investigation itself begin.

Working standards of the absolute units required must be carefully compared with primary standards. This will ordinarily be done at some standardizing laboratory, which will certify those working standards as being correct under certain specified conditions to within, say, a in 10^n. That value is accepted by the experimenter and sets the top limit to the known accuracy attainable in the work. If, for example, the absolute measurement attempted were simply a length, and the working standard were certified as correct to 3 in 10^5, then the absolute measurement (which determines merely the ratio of the measured length to that of the working standard) could under no condition give the value of the quaesitum to a known accuracy that exceeds 3 in 10^5. No matter how small the technical probable error of the measurements might be, the dubiety of the result cannot be less than 3 in 10^5. Indeed, the dubiety of the value found for the quaesitum will in general be distinctly greater than that, on account of errors inherent in the absolute measurement itself.

The experimenter will measure each of the involved quantities in terms of the appropriate working standard, taking pains to observe as well as may be the conditions laid down by the standardizing laboratory, and to determine carefully whatever is necessary to correct for the actual deviations from those conditions. He will do this repeatedly, and he will also measure them under deliberately different conditions, so as to obtain a check on the accuracy with which he can correct for departures from the specified conditions.

Having found that the apparatus seems to be

working properly, he will change, one by one, and by known amounts, each of the adjustments, and will note how each change affects his observations. If possible, he will carry each maladjustment to a point where it produces an easily measurable change in his observations; and if maladjustments in both directions (positive and negative) are possible, he will similarly study each. Thus he will find how important the several adjustments are, the accuracy with which they must be made, and perhaps how to detect each maladjustment experimentally and to correct for the error that it produces.

Readjusting the apparatus, he will proceed to change, one by one, every condition he can think of that seems by any chance likely to affect his result, and some that do not, in every case pushing the change well beyond any that seems at all likely to occur accidentally.

There still remains the possibility of systematic errors arising from unsuspected causes, from secular variation in laboratory conditions (temperature, humidity, light, vibration, etc.), possibly from solar, lunar, or atmospheric effects, etc. So the observer will take long series of observations, extending over weeks, months, or years, noting carefully everything that seems either pertinent in itself or of assistance in fixing the attendant conditions. These will be worked up, day by day, carefully compared with one another, and probably plotted in such a way as to show clearly any change that might appear. From time to time changes will appear, and will be studied.

Thus the experimenter presently will feel justified in saying that he feels, or believes, or is of the opinion, that his own work indicates that the quaesitum does not depart from his own definitive value by more than so-and-so, meaning thereby, since he makes no claim to omniscience, that he has found no reason for believing that the departure exceeds that amount.

That is exactly what he means. He does not mean as some have suggested, that he is of the opinion that the chances are only one in a hundred, or in a thousand, or in some other number *n,* that the quaesitum's departure from his definitive value exceeds that amount. He, differing from those others, feels that it would be foolish for him to make such a statement, that it could be nothing more than a gambler's guess. For how can one say, without stultifying himself, that the chance is one in *n* that the error produced in his result by an entirely unknown, and possibly non-existent, cause exceeds so-and-so, *n* being a definite specified number? And what can the word "chance" mean in that connection? Quantitative "chance" has significance only in relation to a family of events, and its value for a given event depends upon the characteristics of the family as well as upon that of the event itself. But as regards the uneliminated systematic errors, his observations define no family. He has nothing from which to compute a chance. All he can validly do is to express an opinion; and that opinion can validly relate only to certain theoretical considerations and to the magnitude of the errors that might have escaped his attention, not to any chance that his result might be in error by a given amount.

In every report, such an opinion of the limits within which the quaesitum is believed to lie, based solely on the work being reported, should be given. But in addition to that, previous measurements of the same quantity, when available, will usually be compared with those being reported, for one or more of the following purposes: supporting the author's value; setting other limits for the range within which the author thinks the quaesitum lies; deriving a general definitive value. But even in these cases only the same kind of opinion can be expressed, the number of absolute determinations that have been made of any given physical quantity being far too small to define a statistical family. (pp. 10 and 11.)

The experimenter's opinion must rest on evidence, if it is to have any weight. And the only evidence available comes from theory, the series of observations made in the course of the work, and the diligence with which errors were sought. These, and in particular the discordance of the observations and the diligence of the search, are what must be depended upon. Dependence on theory is weak, for the actual conditions never accord exactly with those assumed in the theoretical work.

He knows that it is impossible to avoid systematic errors, that even when he has done his best, his result is still haunted by the ghosts of such errors. His whole problem has been to seek such errors out, and to eliminate them when found; and he believes that in his long search any existing combination of them that would have produced an effect greater than the limit he sets would have been found. But he would be the first to admit that he may be wrong, that his result might be affected by a much larger error arising in such a way that, in spite of the many changes made in the course of the work, it remained essentially unchanged; but he thinks that contingency is highly unlikely. However, he is not entitled to that opinion unless he has carried out the indicated search, for in no other

way can a foundation be found on which to base an opinion.

In the absence of such a search, the worker can do no more than hope that all is going well. The fact that he sees no reason for suspecting the presence of an unknown systematic error is of no importance at all, no matter who the observer is. The really troublesome errors are exactly those that are not suspected. The suspected ones can usually be to some extent eliminated. (p. 12.)

Report

The work should be fully reported, so that the reader may know what was done, may have the means for forming an independent judgment of the work and for checking possible errors and omissions, and may have the worker's experience to build upon in case he himself should undertake a similar piece of work. The last is certainly a very important function of such a report, and should never be ignored.

The report should, of course, give a clear indication of the care with which search was made for sources of error, and of the thought that was given to it. Otherwise, one has no choice but to conclude either that no search was made, or that the author attached no special importance to it. In either case, the work is of little if any, objective value; its acceptance can rest only on authority, on subjective grounds.

Data should be reported as fully as may be. But in every series of observations some are erratic especially at the start. How should they be treated? Those that occur in the body of the work should certainly be reported as fully as if they were not erratic, and if the cause of the trouble is known, that should be explained.

Those that occur peculiarly at the beginning of the series, arising mainly from maladjustment and inexperience, furnish very valuable information regarding details of adjustment and manipulation that had escaped the foresight of the worker, and that might, therefore, readily escape the attention of the reader and of subsequent workers. In certain cases they give valuable information about unsuspected sources of error. For such reasons, they should never be completely omitted. They need not always be given in full, but they should be given to such an extent and in such detail as will show the reader what they were like and how they were related to the pertinent conditions, and should be accompanied by such explanatory text as will show him how they were regarded by the worker, and how he contrived to remove the disturbing conditions.

In brief, the report should give the reader a perfectly candid account of the work, with such descriptions and explanations as may be necessary to convey the worker's own understanding and interpretation of it. Anything short of that is unfair to the writer as well as to the reader. Every indication that significant information has been omitted reduces the reader's confidence in the work.

It is the unquestioned privilege of the worker to say where the boundary lies between preliminary or trial determinations, made primarily for studying and adjusting the apparatus and procedures, and those that were expected to be correct. But he should give good reasons for placing that boundary where he does; and those preliminary determinations should be reported to the extent already indicated.

Furthermore, it is scarcely fair, to anyone concerned, to describe a series of determinations as "preliminary," thus implying, in accordance with common usage, that they are open to question, that they are merely preparatory for something better, and then, later on, to include that same series in the list of good, acceptable determinations. To do so, both confuses the reader and suggests to him that the use of the adjective "preliminary" may have been merely a face-saving device intended to justify the ignoring of that series in case it should be found to disagree uncomfortably with later ones. (pp. 12 and 13.)

Miscellaneous

To say that an observer's results are influenced by his preconceived opinion does not in the least imply that those results were not obtained and published in entire good faith. It is merely a recognition of the fact that it seems more profitable to seek for error when a result seems to be erroneous, than when it seems to be approximately correct. Thus reasons are found for discarding or modifying results that do violence to the preconceived opinion, while those that accord with it go untested. An observer who thinks that he knows approximately what he should find labors under a severe handicap. His result is almost certain to err in such a direction as to approach the expected value.

The size of this unconsciously introduced error is, obviously, severely limited by the experimenter's data, by the spread of his values. The smaller the spread, the smaller, in general, will be this error. The size will be much affected also by the circumstances of the work, and by the strength of the bias. If the work is strictly exploratory, its primary

purpose being to find whether the procedure followed is at all workable, then only a low accuracy will be expected, and there will be no serious attempt to explain departures from the expected, even though the departures be great. Consequently, this error of bias may be entirely absent from such results. But if the worker is striving for accuracy, then departures from the expected will appear to him serious; and the stronger the bias, the more serious will they seem. He will seek to explain them; and that seeking will tend, in the manner already stated, to introduce an error. An error arising in this way will seldom be negligible, but in no case should one expect it to be great, the work being done in good faith. (p. 2.)

. . . published definitive values, with their accompanying limits of uncertainty, are not experimental data, but merely the authors' inferences from such data. Inferences are always subject to question; they may be criticized, reexamined, and revised at any time. (p. 3.)

. . . it is every author's duty to publish amply sufficient primary data and information to enable a reader to form a just and independent estimate of the confidence that may be placed in the inferences that the author has drawn therefrom. If he does not, he is false to both his reader and himself, and his inferences should carry little weight, no matter how great his reputation may be; . . . .

Indeed, values reported without such satisfactorily supporting evidence have no objective value whatever, no matter how accurate they may happen to be. They rest solely on the authority of the reporter, who is never infallible. (p. 3.)

FOREST IN THESE DAYS

Where the tall branch of maple swings,
A few young Chebecs test their wings
All unaware of what we mean
By atoms split, or radar screen.
With all our thought and human powers,
We cannot make the forest ours,
Though we may walk its dim trails over
And flush the partridge from their cover.
Yet we may feel some vague surprise
That vivid trees still point to skies.

DANIEL SMYTHE

Delanson, New York

SCIENCE ON THE MARCH

PROTEIN STRUCTURE PROJECT

THE empirical manner in which research in the protein field must proceed has long been a thorn in the side of biochemists. Besides requiring untold hours of cut-and-try experimentation, it leaves unanswered the fundamental questions on the nature of proteins.

Without proteins there is no life. Proteins exist only as part of and as products of life. Many agents producing disease owe their destructiveness to the proteins they contain. Yet, the structure of not one single protein is known!

In spite of the immense variety of proteins and their range from beneficial to harmful, they are known to have many common characteristics. For instance, they can all be broken down into a very few chemical substances, all of which are called amino acids; this is evidence that the underlying structure of all protein molecules is similar. There is a great deal of other evidence, all pointing in the same direction. However, the structure—the way atoms are arranged and fastened together—in even one kind of protein molecule is still unknown. If this underlying structure were known in the fundamental molecule of a protein, it would doubtless be possible to explain many of the ways that proteins behave and even to predict new things that proteins could do.

Among the things needing explanations are: What is the difference between a cooked and an uncooked egg? (That is: What happens to the proteins of which an egg is composed when they are heated?) Once an egg is cooked, why can it never be made into an uncooked egg? Why do viruses, which are composed of protein molecules swimming in some sort of liquid, sometimes cause diseases and sometimes not? Why does an antitoxin prevent a particular disease, but not others? What is the difference between cancerous and healthy living tissue? And there are, of course, many other questions one can ask about proteins. All these questions could be attacked with much greater confidence of success if one had a knowledge of the nature of even one protein's structure.

One may ask, "How can the structure of a molecule be found?" Many answers to this question have been discovered, and the most universally successful answer is, "By a study of the way x-rays are scattered from crystals consisting of these molecules." There is a huge background of experience and skill in successfully working out the structures of even quite complicated molecules by the x-ray scattering method. It is true that the structure of a protein molecule is much more complicated than that of any molecule attacked up to now, but the methods, experimental, mathematical, and theoretical, are at present powerful enough to give one confidence that a determined attempt to find the structure of a protein molecule can be successful.

The work involved is difficult and extensive; it is necessary to use the talents of several kinds of specialists and to resort to the most modern and complex computing machinery, as well as the best laboratory equipment; but the probability of success is very great.

With this in mind, there was formed in June, 1950, the Protein Structure Project of the Polytechnic Institute of Brooklyn, under the direction of Dr. David Harker, formerly head of the Crystallography Division of the General Electric Research Laboratory, Schenectady. It is sponsored by the Dean Langmuir Foundation, the Rockefeller Foundation, and the Damon Runyon Fund. The Institute contributes location and overhead; the International Business Machines Corporation provides calculating facilities.

This is a straight-line ten-year project with one objective—to determine the atomic positions within protein crystals. The principal tool in this program is a General Electric x-ray diffraction unit adapted to the extremely delicate type of analysis that must be employed on protein molecules. The molecular weight of a protein is 5000 or greater, which poses problems of unusual magnitude in crystallographic studies.

The central requirement of this diffraction apparatus is to permit setting crystal orientation to hundredths of a degree. Dr. Harker and his associates have added to the usual spectrogoniometer a unit tentatively termed an Eulerian cradle, which makes it possible not only to orient the protein crystal with the required accuracy, but also to minimize the exposure of the crystal to x-radiation and thus reduce the change produced in the sample itself by x-radiation.

The x-ray beam collimator is now adapted specifically for single crystal work, with background

reduced to an absolute minimum so as to provide as little interference as possible with the weak protein crystal-diffracted x-rays. The intensity of the diffracted radiation obtainable from protein crystals is only a thousandth of that from single crystals of calcite or quartz. A maximum count would be in that neighborhood of 500 x-ray quanta per second. With the new collimator, background has been reduced from 3 to 10 counts per second, at present, and may go lower as the apparatus is improved.

In the protein structure project, studies are made of samples which diffract at very small angles, compared with the materials encountered in ordinary analytical work. Diffraction angles will be of the order of 1 degree up to 60 degrees at the most, whereas the smallest angle at which iron crystals diffract is about 45 degrees. These requirements call for much higher resolution and control than are customary in other single crystal analysis.

A combination of crystallographic techniques is employed by the project. Photographic recording of diffracted x-ray intensities is used in certain phases, but for others, data are obtained with the Geiger counter spectrometer by counting directly the diffracted intensities. The aim is to get intensities accurate to 5 per cent, and in a shorter length of time than has been possible hitherto—shorter, that is, by from 5 to 10 times.

The effects of x-radiation on the sample, it is planned, will be eliminated by replacing the crystal as soon as such an effect can be detected.

Proteins are high polymers produced only by living matter. The simplest viruses are regarded as nothing more than very large protein molecules, with weights running into the millions. Each molecule is alive, for it eats and reproduces itself.

However, in this project, work has begun, of course, with the simplest structures, molecules measuring only around 30 to 50 angstrom units in diameter. The work now in progress consists of three main programs: (*a*) the preparation of protein crystals, (*b*) the obtaining of x-ray diffraction data, and (*c*) the conversion of these data into knowledge of protein structure.

These programs are now in the following stages: (*a*) Crystals of several allotropic forms of the enzyme ribonuclease have been grown large enough to be useful for x-ray diffraction experiments, and their diffraction patterns are being obtained.

(*b*) The data from which a description of protein molecular structure will be derived are the intensities and directions of the x-ray beams diffracted by protein crystals. These data can be recorded either photographically or by means of a Geiger-Müller counter. Devices for recording photographically the single-crystal diffraction data have been purchased and are now being used. Another device in which a counter collects the data has been assembled from some purchased parts and some parts constructed by the project's instrument maker. This device has been used to obtain data from some ribonuclease crystals given to the project by Professor I. Fankuchen and will soon be used on other crystals. In addition to these sets of data, lists of intensities have been borrowed from two other laboratories: on insulin from Dr. Dorothy Hodgkin of Oxford University and on hemoglobin from Dr. F. Perutz of Cambridge University. At the same time as the collection of data just mentioned, a device is being designed and constructed by means of which all the data from a crystal will be collected automatically and recorded on IBM punched cards. This device will not be in operation for at least a year, but should justify the effort being put on it in a very short time after it is first put into use.

(*c*) The data to which reference has been made so often are related to the crystal structure in a rather complicated, but definite mathematical way, so that the knowledge of the data is, in principle, a knowledge of the crystal's structure. At one point in the computation there is a difficulty, however, which is equivalent to deciding for each diffracted x-ray beam whether it should be positive or negative. There are ways of deciding this question, but they too involve a large amount of computation.

In consequence, it was felt necessary to obtain the cooperation of the International Business Machines Corporation before attempting to plan the project at all. This cooperation has been forthcoming in that the project personnel may work with the computing devices of the Watson Laboratory of IBM as much as is necessary to the success of the project. Use has been made of this privilege, and methods of computation suitable for crystal structure work on proteins have been devised and tested, using the data on insulin and hemoglobin borrowed from the English universities. These methods are now being tried out on the preliminary ribonuclease data recently obtained.

In addition to accomplishing the main objective, the knowledge of a protein structure, it is certain that many other very valuable pieces of information will be picked up during the course of the work. These may include results in protein chemistry, x-ray physics, mathematics, and equipment use and design. The sponsors of, and participants

in, this project count on a rich reward in scientific achievement, the results of which cannot fail to be of important and lasting value to all people.

Related work is being done at Harvard Medical School, the Massachusetts Institute of Technology, and California Institute of Technology, and at Cambridge University, Oxford University, and University of London, in England.

The permanent staff of the Protein Structure Project at Brooklyn Polytechnic Institute consists, in addition to Dr. Harker, of Dr. Murray Vernon King, chemist, who prepares and selects the protein crystals suitable for analysis; Dr. Thomas C. Furnas, Jr., physicist, in charge of designing apparatus for diffraction data; Dr. Beatrice S. Magdoff, physicist, who converts diffraction data into useful structural information; William G. Weber, precision instrument maker, who constructs and maintains the apparatus and laboratory equipment; and Mrs. Harker, secretary and librarian. In addition to the permanent staff, other scientists will spend periods of time working in the laboratories of the project; Dr. V. Luggati is the first of these.

The laboratories occupy 1200 square feet, divided into an x-ray diffraction laboratory with connecting photographic darkroom, chemical laboratory, three office rooms, and a machine shop.

DAVID GOODMAN

Milwaukee, Wisconsin

BOOK REVIEWS

WHAT MAKES AN INSECT TICK

Insect Physiology. Kenneth D. Roeder, Ed. xiv + 1100 pp. Illus. $15.00. Wiley, New York; Chapman & Hall, London. 1953.

IT is evident why book reviews of this fine text have been slow in appearing, for it is not recommended for light fast reading, even by the specialist. On the other hand, applied perusal quickly underlines the fact that insect physiology has "come of age," if the specializations evidenced in the respective treatments of different phases of the subject by fifteen authorities, two of them Australian and the others American, are adequate criteria. At least it would be as difficult for any one person to marshal the facts with the mastery accomplished sectionally in this technical treatise, as for another one to review it as adequately.

The method of presentation indicates that it is not intended as a teaching textbook. On the other hand, it will unquestionably prove to be an indispensable reference work both for teaching and research. The 107 pages of bibliography attest the thorough documentation of subjects ranging from considerations of the integument and cuticle, circulation, respiration, nutrition, excretion, behavior, and embryology—through 32 chapters to hormones in metamorphosis.

That insects are peculiarly well suited to certain types of physiological experimentation is repeatedly shown, and the familiar *Drosophila* flies again play one of the leading roles. Many comparisons with the more advanced knowledge in equivalent vertebrate fields add to the value of the work. For example, chemoreception is considered as being basically the same in human beings and in insects, which makes the latter ideal experimental subjects. The editor's prefatory remark that the insect physiologist shares "with entomologists an interest in insects per se" is well taken, and emphasizes all the more the major omission of sections on the physiology of the reproductive and light-production systems, of which there is no equivalent lack of literature. One would also have liked to find more reference to physiological advances in the fields of symbiosis, parasitism and parasitic acarina, and relationships to

disease agents, among others. Though there is considerable discussion of optic mechanisms and developments, one looks in vain for any further discussion of the functioning of the previously speculative, so-called "night-eyes" of certain male Diptera, with their upper areas of much enlarged facets. But there are limitations to what can be put into one book, as the reviewer can attest (this volume already weighs just four pounds)—not to mention the technical dictionary often needed for a not inconsiderable amount of terminology unfamiliar in my own limited background. These are all evidence of an advancing discipline and not a criticism.

The book abounds in interesting facts, such as the almost incredible record of the alternation of flight muscle movement in flies, as high as 1046 per second—far beyond the observed performance of stimulated frog muscle and the humming bird among higher vertebrates—and the ingenious use of surgical devices in demonstrating ontogenetic tissue growth and differentiation by which the subsidiary science of insect endocrinology was launched. It was startling to learn that in some insects the compound eyes are developed by invagination of the epidermis of the larval head capsule, whereas in others they develop from embryonic pharyngeal tissue. Parts of chapters on insect behavior and social patterns, while fascinating, would seem more appropriate in some ecological treatise. On the other hand, for other specialized entomological groups such as insect toxicologists, this book will quickly become one of their most important reference books. The text is remarkably free of typographical errors. A useful innovation is the page-use citation for each of the bibliographic references. Furthermore, some chapters have helpful summaries or conclusions.

This reviewer, as an "outsider," feels well repaid for having read through this comprehensive treatise with a view to future reference. Any shortcomings it has will undoubtedly be rectified when revised, or will "point the finger" to the many important lacunae still to be investigated—and these indeed have often been called to attention in the present text. The book is a worthy testimonial to the maturation of insect physiology, although in this early development of the science it still is far from answering all the complicated questions on what makes an insect tick.

CORNELIUS B. PHILIP
National Microbiological Institute
Rocky Mountain Laboratory
Hamilton, Montana

CELLULOSE AND INDUSTRY

Cellulose, the Chemical That Grows. Williams Haynes. 386 pp. Illus. $4.00. Doubleday, Garden City, New York. 1953.

CELLULOSE, in both bulk and value, is a major raw material of the American chemical industry. This book describes the historical development and present condition of each of the important chemical industries that are based on cellulose, including the explosives, lacquer, film, plastics, and synthetic textile industries. The author is personally acquainted with many of the pioneers in the development of these industries and this fact gives the book the authenticity and intimate detail of an eyewitness account. The historical style that mingles in a dramatic fashion important laboratory discoveries with patent law suits, missed opportunities with astonishing commercial successes, makes fascinating reading for a nontechnical audience. The book also includes a chronology of cellulose technology, a technical glossary, and appendices of statistical data. The 45 illustrations are beautifully reproduced and informative. The only faults are lack of bibliography and a rather weak index. The book is heartily recommended to everyone from high school students to salesmen and financiers who may be interested in the development of an important segment of American industry. It is seldom that one finds a more carefully written, informative, and thoroughly interesting book.

MARK H. ADAMS
Department of Microbiology
New York University Bellevue Medical Center

BACTERIOLOGY FOR NON-SCIENTISTS

Microbiology and Human Progress. Madeleine Parker Grant. xvii + 718 pp. Illus. $6.75. Reinhart, New York. 1953.

THIS college textbook will serve primarily the undergraduate or graduate student whose major interests lie outside science. Many such students must take one course in a natural or physical science and are antagonized by rather than attracted to microbiology when forced to compete with science majors and to use a technical textbook that presupposes background in biology and chemistry. Dr. Grant's recognition of the need for a textbook for non-scientists has resulted in frequent use of descriptive material relating microbiological observations with phenomena associated with higher forms of life, interesting historical backgrounds to many microbiological discoveries, excellent illustrations, simple appropriate diagrams, and several medical case histories. The book has a good conversational tone and includes much of the philosophy of the author.

The book is *not* recommended for the use of students majoring in chemistry, biology, microbiology, bacteriology, or the various medical fields. Although the organization of all the material is fairly good (especially the regional medical grouping of diseases), the treatment of many subjects is nontechnical and often unbalanced. Science students should be given much more material on cell physiology, for example, than can be included in a textbook of this type. Also, of the 363 pages devoted to applied microbiological fields, only 14% deal with "microbic friends of man" whereas 86% are concerned with "microbic foes of man." Despite the inclusion of the word "microbiology" in

the title, the great majority of the general and applied sections deal specifically with bacteria, rickettsia, and viruses. Algae, yeasts and molds, and protozoa are discussed in only 18 pages of the general section; in the medical section, the pathogenic yeasts, molds, and Mastigophora are omitted completely. The space devoted to amebic dysentery in the medical section is only one-sixteenth the space devoted to typhoid fever although the author states that in the United States in 1949 twice as many cases of amebic dysentery as typhoid fever were reported. Important present-day bacterial infectious diseases that are not discussed include plague and cholera; others of lesser importance that are omitted include tularemia, psittacosis, spirochetal diseases other than syphilis, and leprosy. Infectious diseases of plants are not mentioned.

The chapter on chemotherapeutic agents was apparently written in 1947 (based on the contents and references) but nevertheless contains a considerable amount of useful information. Of all the chapters, approximately half contain some references to work published between 1949 and 1952. The index comprises only 15 pages which is unusually brief for a lengthy textbook. Perhaps the brevity of the index reflects the fact that the size of the book has been inflated by the large number of illustrations and by the large amount of nontechnical material of a philosophical, historical, or social nature. Minor criticisms include the use of the adjective "exotic" to describe rickettsia and viruses and to describe the inorganic nutrilites required by autotrophic bacteria; also, in the general section on molds the statement is made that sprue is a fungal infection.

None of the above criticisms will annoy the non-science student and instructors of courses in bacteriology designed for such students should, when selecting a text, seriously consider this book.

EUGENE D. WEINBERG

Department of Bacteriology
Indiana University

RADIOCHEMISTRY

Radioisotopes in Industry. John R. Bradford, Ed. vii + 309 pp. Illus. $8.00. Reinhold, New York. 1953.

THIS book consists of a compilation of papers presented at a symposium under the title, *Radioisotopes in Industry,* presented in cooperation with the Atomic Energy Commission at Case Institute of Technology in April, 1951. A wealth of valuable and interesting information is made available within the covers of this one book.

It contains fourteen chapters, each written by a different author. The chapter headings indicate clearly the scope of information presented: Radioisotopes, A New Industry; Radioisotopes for Industry; Fundamentals of Radiochemistry; Radiation Protection; Radioisotopes in Physical and Chemical Research; Applications of Radioisotope Techniques; Radiochemical Laboratories, Design of Laboratories for Safe Use of Radioisotopes; Production and Separation of Radioisotopes; Tracer Experiments; Instrumentation in the Radiochemical Laboratory; Decontamination and the Disposal of Radioactive Wastes; Industrial Uses of Radioactive Fission Products; Distribution of Radioisotopes by the Atomic Energy Commission.

The style of writing varies from chapter to chapter, as should be expected in a book of this type. All authors are authorities in the field and the presentations are all clear, informative, and interesting. Readers who are not acquainted with the field will find the book very readable because no assumptions have been made concerning prior knowledge of radioisotopes. Also, the expert in radiochemistry as well as those whose interests are primarily in industry will read this book with considerable interest and profit.

Each chapter is written as a complete unit. This means that there is some repetition from chapter to chapter, yet it also makes it possible for the reader to select only those chapters in which he is especially interested if he so desires. The headings throughout the chapters are well arranged. Each chapter concludes with a list of general references for supplementary readings.

The reviewer noted throughout a very proper emphasis on the facts that the industrial uses of radioisotopes have already become very important and yet that the surface of tremendous possibilities has hardly been scratched. This book is to be highly recommended, both to experts in the field and to those members of the general public who are interested in knowing more about the industrial age in which we live.

ROBERT D. FISCHER

Department of Chemistry
Indiana University

MEN OF NERVES

The Founders of Neurology. Webb Haymaker, Ed. 479 pp. Illus. Thomas, Springfield, Ill. 1953.

THIS collection of biographical vignettes, describing the life and work of 133 founders of neurology, is a new departure in the writing of scientific history. The volume brings together brief but intimate accounts of outstanding investigators in the neurological field, including anatomists, physiologists, pathologists, clinicians, and surgeons. The biographies are written by eighty-four authors, mainly Americans, who have usually been students, friends, or colleagues. The book was conceived by a group of members of the American Neurological Association at its meeting at Atlantic City in June, 1948. It was to have been presented to the Fourth International Neurological Congress at Paris in 1949, but has only now come from press. The volume is notable in bringing together photographs of all of the workers discussed, an achievement that adds much to the sense of intimate insight into their lives.

Skillful editing has produced a style of presentation that runs consistently through the whole volume. In

each biography the main scientific contributions are outlined. Relationships and associations between workers are indicated so that a three-dimensional picture of neurological activity, in space and time, builds up in the reader's mind. Of the men studied all but sixteen were alive in 1880, and the account covers mainly the century from 1830 to 1930. The biographies contain many stories and anecdotes, illuminating the scientific philosophy, or personal traits, of members of the group. The origins of terms and phrases, now widely used in the field, are frequently made clear. The history of classical controversies and discoveries is outlined.

This book will be cherished by all those who now investigate or teach in the field. It will be an inspiration and challenge to younger men hoping to enter it.

WILLIAM R. AMBERSON

Department of Physiology
University of Maryland Medical School
Baltimore, Maryland

Books Reviewed In SCIENCE

July 10

Advances in Cancer Research. Vol. I. Jesse P. Greenstein and Alexander Haddow, Eds. New York: Academic Press, 1953. 590 pp. Illus. $12.00.
Reviewed by Michael B. Shimkin.

Science and Humanism: Physics in Our Time. Erwin Schrödinger. New York: Cambridge Univ. Press, 1952. 68 pp. $1.75.
Reviewed by Karl Lark-Horovitz.

Record of the Rocks: The Geological Story of Eastern North America. Horace G. Richards. New York: Ronald, 1953. 413 pp. Illus. $6.00.
Reviewed by George W. White.

July 17

Plant Anatomy. Katherine Esau. New York: Wiley; London: Chapman & Hall, 1953. 735 pp. Illus. + plates, $9.00.
Reviewed by Richard H. Goodwin.

Clinical Allergy. French K. Hansel. St. Louis, Mo.: Mosby, 1953. 1005 pp. Illus. $17.50
Reviewed by W. C. Spain.

Zoogeography of the Sea. Sven Ekman; trans. from Swedish by Elizabeth Palmer. London: Sidgwick and Jackson, 1953. (Distributed by Macmillan, New York.) 417 pp. Illus. $6.50.
Reviewed by Gordon A. Riley.

The Permeability of Natural Membranes. Reissue. Hugh Davison and J. F. Danielli. New York: Cambridge Univ. Press, 1952. 365 pp. Illus. $6.00.
Reviewed by L. J. Mullins.

July 24

Methods and Principles of Systematic Zoology. Ernst Mayr, E. Gorton Linsley, and Robert L. Usinger. New York–London: McGraw-Hill, 1953. 328 pp. Illus. $6.00.
Reviewed by G. G. Simpson.

Fatigue and Fracture of Metals. William M. Murray, Ed. Cambridge, Mass.: Technology Press of the Massachusetts Institute of Technology; New York: Wiley, 1952. 313 pp. Illus. $6.00.
Reviewed by A. G. Guy.

The Theory of Homogeneous Turbulence. G. K. Batchelor. New York: Cambridge Univ. Press, 1953. 197 pp. Illus. $5.00.
Reviewed by R. C. Binder.

July 31

Flying Saucers. Donald H. Menzel. Cambridge, Mass.: Harvard Univ. Press, 1953. 319 pp. Illus. $4.75.
Reviewed by C. C. Wylie.

Physiological Foundations of Neurology and Psychiatry. Ernst Gellhorn. Minneapolis: Univ. Minnesota Press, 1953. 556 pp. Illus. $8.50.
Reviewed by Robert G. Grenell.

ASSOCIATION AFFAIRS

THE BOSTON MEETINGS OF THE ASSOCIATION: A BIT OF BACKGROUND

THE 120th Meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, the annual meeting for the year 1953, is also, officially, the Seventh Boston Meeting. The AAAS was conceived in Boston 106 years ago and, in some respects, this can be considered the ninth meeting in Boston and the tenth on the banks of the Charles —since the precursor of the Association met twice in that city and the young AAAS held its second meeting in Cambridge, in 1849. This year's gathering of scientists, industrial leaders, administrators, educators, engineers, and other science-minded professional people from all over the continent will come together for a common purpose suggested by the theme: "Scientific Resources for Freedom." In the final week of the year, it will be time once more to take stock both of current scientific research and of the problems that confront all scientists. Particular attention will be given to the nation's resources of scientific men, materials, and methods.

In meeting in Boston again, the Association is returning to the city where its founding was planned and authorized, on September 24, 1847. It was at the eighth and terminal meeting of the Association of American Geologists and Naturalists on this date, more than a century ago, that the decision was made to reorganize the society as an enlarged American Association for the Promotion of Science. The chairman of the society at that time was William Barton Rogers (1804–1882), professor of geology and natural history in the University of Virginia, who, later, was to select Boston in which to found the Massachusetts Institute of Technology and to serve as its first president. When the new organization, renamed the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, met in Philadelphia, September 20, 1848, a special resolution was passed that Professor Rogers, last president of the AAGN, henceforth should be recognized as the first president of the AAAS and, in fact, he presided until his elected successor, William C. Redfield of New York, took office.

The AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE has other links with Boston and the Commonwealth of Massachusetts, from which, in 1874, it received its charter of incorporation. In 1837, John Collins Warren of Boston read a paper before the British Association for the Advancement of Science and was so impressed with the value of one large meeting devoted to all the sciences that, upon his return the following year, he began the active promotion of a parallel organization in America. He, too, was present at the September 1847 meeting when the AAGN was reorganized. Since the British Association for the Advancement of Science, founded in 1831, is the prototype of the AAAS and of all similar associations for the advancement of science subsequently established throughout the world, it is particularly appropriate that a recent past president of the British Association, Dr. A. V. Hill, will deliver an address at this Seventh Boston Meeting.

The official First Boston Meeting of the AAAS, held in August, 1880, at Massachusetts Institute of Technology, then located between Copley Square and the Public Gardens, was an occasion, scientific and social, long to be remembered. Lewis H. Morgan, the renowned anthropologist, was president of the Association, which then had 1,555 members. The address of the retiring president, George F. Barker, an outstanding chemist of the period, was on "Some Modern Aspects of the Life-Question." There were 979 registrants from more than 30 states, Canada, England, and Cuba and 276 papers were read. As first past president of the Association and also as first president and founder of M.I.T., the host institution, it was eminently fitting that William Barton Rogers, though now 76 years in age, should serve as General Chairman and deliver an address of welcome. The local "Committee at Large" included Charles Francis Adams, Charles W. Eliot, Ralph Waldo Emerson, Asa Gray, Oliver Wendell Holmes, Henry W. Longfellow, Francis Parkman, and Josiah Quincy—to name but a few. Samuel H. Scudder and Edward Burgess were secretaries.

In this more leisurely, less complicated period, M.I.T. served complimentary lunches daily. The President and Fellows of Harvard University entertained the entire attendance at dinner in Memorial Hall. There were receptions, notably those by President and Mrs. Rogers, Mr. and Mrs. Alexander Graham Bell, and Mr. and Mrs. S. Endicott Peabody, and many open houses—including those sponsored by the Athenaeum, the Boston Society of Natural History, the Massachusetts Historical Society, and the Massachusetts Horticultural Society—and the City of Boston provided an excursion boat trip down the harbor complete with a collation. To facilitate reaching the sessions from the downtown hotels, "those cars passing by the Institute [were] designated by a white flag, with

the letters A.A.A.S. . . . " The Western Union Telegraph Company and the American Bell Telephone Company transmitted the messages of the delegates gratis, the Post Office arranged to be open on Sunday morning, and the railroads not only had special rates for general convention travel but operated free trains to the White Mountains.

The Fiftieth Anniversary of the Association was celebrated at the Second Boston Meeting of August, 1897, with another distinguished anthropologist, Frederic W. Putnam, who had served the AAAS as permanent secretary for 25 years, now the president. M.I.T. again was host institution. The Copley Square Hotel was AAAS headquarters —with single rooms at $1.00 to $2.50. The address of Wolcott Gibbs, retiring president, and one of five surviving founders of the Association, was "On Some Points in Theoretical Chemistry." The Honorary President, Governor Roger Wolcott, took a personal interest in this meeting and delivered an excellent address. The papers read totaled 443 and the registration was 903.

By the time of the Third Boston Meeting, in 1909, once more on the former campus of M.I.T., the Association had changed its time of meeting from summer to the last week of December (primarily, because of the development of summer sessions on campuses), and the pattern of participation by a large number of scientific societies was well established. David Starr Jordan, eminent zoologist and university president, was president of the Association; the retiring presidential address, "A Geologic Forecast of the Future Opportunities of Our Race," was given by Thomas C. Chamberlin. Harry W. Tyler was General Chairman. There were 1,140 registrants, making this the largest AAAS meeting up to that time. Among the 404 papers read was "The Chemist's Place in Industry" by Arthur D. Little, founder of the firm which bears his name today. A national Bureau of Mines was recommended by the AAAS.

The Fourth Boston Meeting of December, 1922, with the celebrated Canadian anatomist, J. Playfair McMurrich, as president, was held principally on the new campus of M.I.T. though, as on previous occasions, there were events at Harvard University in Cambridge. The address of retiring president Eliakim H. Moore was "What Is a Number System?" Professor Samuel C. Prescott of M.I.T. was General Chairman. The Somerset was AAAS headquarters hotel. The exhibits, arranged for by a committee headed by Professor Robert P. Bigelow, for the first time included a number installed by commercial exhibitors. It is gratifying to note that some of these pioneer exhibitors not only are still in business but will participate in this year's Exposition. The first of the annual addresses of the Society of the Sigma Xi at AAAS meetings was given by President Livingston Farrand of Cornell University on "The Nation and Its Health." The papers read totaled 1,019 and the registration was 2,339.

All local institutions of higher learning were hosts of the Fifth Boston Meeting of December, 1933. Sessions were held, principally, at Harvard, M.I.T., and at the Hotel Statler, AAAS headquarters. The exhibits, now the responsibility of a staff member, were in Harvard's Memorial Hall and, in number, exceeded those of all previous Expositions. It was a large and successful meeting despite the extremely low temperatures experienced by the entire East during this exceptional winter. The famous astronomer, Henry Norris Russell, was president of the Association, and presided at the address of retiring president, John Jacob Abel, eminent pharmacologist, on "Poisons and Disease." Again, Samuel C. Prescott served as General Chairman; A. Lawrence Lowell was Honorary Chairman. A much appreciated event was a complimentary testimonial concert given by the Boston Symphony Orchestra with Dr. Serge Koussevitsky conducting. The eleventh winner of the AAAS Thousand Dollar Prize was Reuben L. Kahn for the paper, "Tissue Reactions in Immunity," in the program of Section N. About 1,500 papers were read and there were 2,351 registrants, as usual from nearly every state and Canadian province.

Though, again, all local institutions were hosts of the Association, the Sixth Boston Meeting of December, 1946, was characterized by a much more intensive use of downtown hotels for session rooms. The Annual Science Exposition was located in the Cadet Armory near the Hotel Statler, AAAS headquarters. President of the Association was James B. Conant; the retiring president, Charles F. Kettering, gave his address, "A Look at the Future of Science," in Symphony Hall. David M. Little of Harvard University was General Chairman. The twentieth winner of the AAAS Thousand Dollar Prize was shared equally by T. M. Sonneborn, Ruth V. Dippell, and Winifred Jacobson for several papers on the mechanism of heredity in *Paramecium,* read before the American Society of Zoologists; and by Quentin M. Geiman and Ralph W. McKee for "Cultural Studies on the Nutrition of Malarial Parasites," read before the American Society of Parasitologists. A total of 2,736 persons registered and 1,332 papers were read. The first

AAAS-George Westinghouse Science Writing Award was won by James Graham Chesnutt of the San Francisco *Call-Bulletin* for a story on a bubonic plague preventive.

In summary, the records of all previous meetings in Boston do not fail to mention the warm spirit of hospitality and interest in the Association and its work shown by the people of this cultural center. The group of cities and suburban communities which comprise the Boston Metropolitan Area—now with a population of two and one-half millions—has one of the country's greatest concentrations of institutions of higher learning, and of libraries, museums, and scientific laboratories. New England is compact and New York is nearby, so that local and regional attendance added to the several thousand persons who will come from all parts of the continent to attend the programs of the Association's 18 sections and subsections, and the national meetings of the zoologists, geneticists, science teachers, meteorologists, the History of Science Society, and others, may make the Seventh Boston Meeting the second largest in the annals of the Association. In all, in national and regional meetings and cosponsored sessions, some 57 organizations will participate. With sessions for contributed papers, symposia, distinguished evening addresses, and a growing number of conferences, the Seventh Boston Meeting will be one of the most significant annual conventions in the long history of the Association. Of the 15 past presidents of the Association now living, five are residents of New England. It is hoped that they—Karl T. Compton, James B. Conant, Harlow Shapley, Edmund W. Sinnott, and Kirtley F. Mather*—and the others will be able to attend this year's meeting.

With its many historical landmarks, Boston itself is worth a visit at any time. Indeed, the "Points of Interest" are too numerous to describe in this year's General Program-Directory. Instead, each registrant will receive a complimentary printed handbook at the Main Registration–Information Center in the Mechanics Building. Founded in 1630, since colonial times, Boston has been a seaport, the banking and commercial metropolis of New England, and a great industrial center. In recent years, this city has become noted as the site of new and important developments in chemistry, electronics, and nuclear physics. Many of these new "scientific resources for freedom" will be on display in the 160-booth Annual Exposition of Science and Industry in Mechanics Building. It is particularly fitting that the General Chairman of this year's 120th AAAS meeting is Earl P. Stevenson, president of Arthur D. Little, Inc. Not only is he the leader of a company that has pioneered in the organized applications of science, but he is active in a number of national scientific organizations. His appointments—the many persons who are working to make the Seventh Boston Meeting an unqualified success—will be listed later. The fruits of their contributions of time and thought will be apparent to those who attend this year's meeting.

The American Association for the Advancement of Science continues to grow, both in personal membership and in affiliated professional societies and academies of science. In just the seven years since the Sixth Boston Meeting, the AAAS, now with a membership of approximately 50,000, has experienced a net gain of more than 21,000 members. In 1946 there were 200 affiliates and associates; at this time, the number of affiliated and associated organizations is nearly 250. The Association's capacity for service to science, to scientists, and to society has been correspondingly enhanced. Fundamentally, the Association *is* its membership. Those who attend the Seventh Boston Meeting will do much to help chart its future course.

RAYMOND L. TAYLOR
Associate Administrative Secretary, AAAS

* The other ten living past presidents are Liberty Hyde Bailey, Robert A. Millikan, Henry Norris Russell, Albert F. Blakeslee, Irving Langmuir, Arthur H. Compton, Anton J. Carlson, Charles F. Kettering, Elvin C. Stakman, and Roger Adams.

THE SCIENTIFIC MONTHLY

SEPTEMBER 1953

The Diary of William Fellowes Morgan

TEMPLE R. HOLLCROFT

Dr. Hollcroft has been Professor of Mathematics and Department Chairman at Wells College for thirty-four years, and was recently appointed Wells College Historian. His five degrees were conferred by Hanover College (B.S., A.B., and Sc.D.), University of Kentucky (A.M.), and Cornell University (Ph.D.). In World War I he was a 2nd Lieut. in the Field Artillery. He spent two years studying and traveling in Europe. He is a member of four mathematical societies, four honorary societies, including Phi Beta Kappa and Sigma Xi, and fellow of the AAAS. Dr. Hollcroft was Associate Secretary of the American Mathematical Society for fourteen years. He has published 38 research papers in mathematics, and many articles on local history.

WILLIAM FELLOWES MORGAN kept a diary of a portion of an expedition to Colorado and New Mexico with his great-uncle, Lewis Henry Morgan, in the summer of 1878. The chief purpose of this expedition was to study the prehistoric ruins in that region and the buildings and customs of the Pueblo Indians. The diary also contains an account of the 27th meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE in St. Louis, which they attended.

Mrs. Cleveland E. Dodge of New York City, a daughter of William Fellowes Morgan and Vice Chairman of the Wells College Board of Trustees, presented this diary to the Wells College Library on March 21, 1953.

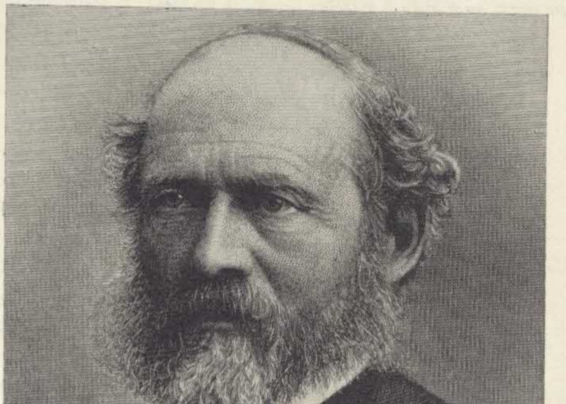
A short note under the caption "Has he a rival?" was published in SCIENCE (117, 234 [1953]). In this note it was stated that William Fellowes Morgan, who became a member of the AAAS at the age of 17, may have been the youngest member ever elected. Now it is revealed in the diary that he presented a paper to the AAAS on the day following his election to membership and that his paper received high commendation from the scientists who heard it. This was a remarkable accomplishment for a youth of 17, who had just completed his second year at college.

The emigrant ancestor of both Lewis Henry Morgan (1818–1881) and William Fellowes Morgan (1860–1943) was James Morgan (1607–1685), a native of Wales, who came to Boston, Massachusetts, in 1636. One of his sixth-generation descendants, Jedediah Morgan (1774–1826), moved in 1792 with his parents to a farm near Aurora, New York, and later became a New York state senator. Two of Jedediah Morgan's sons were Lewis Henry Morgan, "the father of American anthropology" and President of the AAAS in 1880, and Amos Morgan, the father of David Pierce Morgan, an eminent banker of New York

City, who married Catherine Fellowes, daughter of William Fellowes. Their oldest son, William Fellowes Morgan, the author of the diary, was born September 24, 1860.

Lewis Henry Morgan was born November 21, 1818, on his father's farm (where his grandfather had settled in 1792), three miles south of Aurora, New York. His father moved to Aurora when Lewis was four years old. Lewis attended Cayuga Academy in Aurora until 1838 when he entered Union College as a junior, graduating in 1840. Christened "Lewis Morgan," he added "Henry" to his name at about the time he started to college. In 1840 he returned to Aurora and studied the classics in Cayuga Academy and law in the office of David Wright. It was about this time that he organized "The Grand Order of the Iroquois" among the students of Cayuga Academy. Beginning at Aurora, this society soon established "tribes" in several other localities in central New York. The G.O.I. was both an expression of Morgan's growing interest in Indian life and customs and an incentive to the development of that interest. Many of his researches were begun in order to model the G.O.I. after the structure of the Iroquois social and political systems. His first book, "The League of the Iroquois," published in 1851, consists largely of a collection of addresses written for the G.O.I. and delivered at its meetings.

Lewis Henry Morgan moved to Rochester in 1845 and soon began writing the articles and books, chiefly on anthropology, that made him famous. His business ventures were more successful than his law practice, and he became moderately wealthy. He served in both the Assembly and the Senate of the New York Legislature during the sixties. Interested in the education of women, he bequeathed his estate to the University of Rochester to be used in establishing a college for women at that university, of which he was a trustee. He was also the first elected trustee of Wells College. He organized the Anthropology Subsection D of Section B of the AAAS, was its chairman for some years, and was President of the AAAS in 1880. He presided at the 1880 Boston meeting, but was too ill to deliver an address as retiring president at the 1881 Cincinnati meeting. His health failed rapidly, but he was able to complete his last book, *Houses and House Life of the American Aborigines* (Department of the Interior, Washington, 1881; referred to hereafter as *House Life*). A biography of Morgan, written by F. W. Putnam, Permanent Secretary of the AAAS for 25 years and President in 1898 (*Proceedings of the American Association for Arts and Sciences,* Vol. 117 [1882]) contains the concluding sentence: "The great principles which his researches have brought out . . . will ever stand . . . as foundations upon which to build in the further study of America in archeology and ethnology."



Lewis H. Morgan, anthropologist, the uncle of W. F. Morgan. (Dept. of History, Bureau of American Ethnology.)

William Fellowes Morgan graduated at Columbia University in 1880 and received the additional degree of Mining Engineer there in 1884. He married Emma Leavitt in 1885. As a scientist, he was among the first to foresee the possibilities of preservation by refrigeration. He entered the cold storage business in 1887 and was soon made president of the company. He was a director in several other companies and, in 1905 and 1907, a member of the New Jersey Legislature. He held offices in many educational, religious, and philanthropic institutions, including President of the Y.M.C.A. Association of New York City, alumni trustee of Columbia University for six years, trustee of Wells College for 26 years, for 13 of which he was President of its Board of Trustees.

Although his business activities, which he pursued with marked success, diverted him from a life of science, William Fellowes Morgan was always interested in that field and continued his membership in scientific organizations, including the AAAS and the New York Academy of Sciences. When he died in 1943, he had been a member of the AAAS for 65 years. He and five others were eulogized in the article, "Six Patriarchs of the Association," published in the January 1945, *AAAS Bulletin*. He was elected a Fellow in 1932 and an Emeritus Life Member in 1933. These honors, together with


William Fellows Morgan, about the time he wrote the diary.

the fact that he became a member and presented a paper at the age of 17, constitute a unique record in the annals of the AAAS.

Lewis Henry Morgan, with two of his grand-nephews, William Fellowes Morgan and his younger brother, David Percy Morgan, went on the expedition to the Southwest in the summer of 1878. They were accompanied by two other young men called "Donald" and "Henry" in the diary. They visited the Pueblo Indians in their villages and made a survey of many ruins of prehistoric dwellings in southwestern Colorado and northern New Mexico.

The party first went by train to Cañon City, at that time the western terminus of the Denver & Rio Grande Railroad. For about six weeks thereafter, they traveled on ponies and in wagons several hundred miles through an extremely rough and largely unsettled country. Lewis Henry Morgan published the findings of this expedition in *House Life*. On page 172, he states that they visited pueblo ruins near the Animas River on July 22, 1878 (one of the very few dates given). Probably a few days previously, they had studied a newly discovered cliff-house on the Mancos River. This cliff-house was the one described by William Fellowes Morgan in his AAAS paper. After viewing other ruins of prehistoric dwellings, including some on the San Juan River, the party arrived at Conejos, Colorado, on August 5.

The diary of William Fellowes Morgan begins with their arrival at Conejos. Here they obtained a team and a "Kansas Wagon" in which they traveled to Taos Pueblo and back, a distance of about 175 miles, and then from Conejos to Alamosa. At that time, Alamosa was the southern terminus of the Denver & Rio Grande Railroad. They continued the journey by rail via Denver to St. Louis to attend the AAAS meeting. The following portion of the diary ends with their departure from St. Louis.

Costilla [New Mexico], August 6, 1878.

When we arrived at Guadalupe, or Conejos[1] as it is usually called, we stabled our horses in a barn belonging to J. M. Archeluta, a Spanish merchant, and took our supper at a hotel kept by Joe Coleman. After supper, as we felt too lazy to put up the tent, we agreed to spread our blankets in the hay in the barn. We spent a pretty comfortable night and were waked early by the chickens, for we were surrounded by them. After breakfast, we busied ourselves trying to get a team to take us to Taos. We at last made arrangements to go to Taos and leave Henry. We made some purchases and saw Henry off. We had engaged a team from Joe Coleman, the landlord. It consisted of a Kansas Wagon and two sorrel horses and a Mexican driver.

We started about 9½ o'clock. We drove 18 miles to the Rio Grande, forded at Mayer's Crossing and stopped for lunch. Our drive during the morning had been over a lava field and was very level, being through the San Luis Valley. It was a very pretty ride. On all sides we had magnificent mountain views of the Sangre de Cristo range and Mt. Blanca, also of the Costilla, Conejos, and other smaller ranges.

The Rio Grande at this time was quite low, so that few people availed themselves of the ferry at this point. After stopping a few minutes at the river to feed the horses and ourselves, we drove on to Costilla, 12 miles further. Costilla is a moderately large plaza, 30 miles nearly east of Conejos. To get to it you have to cross from one side of the San Luis Valley to the other. It is situated at the base of the Costilla range. We camped near the hotel kept by Mr. George Blackmore. Uncle Lewis,[2] Donald and I went to the hotel. We prepared for tea by taking a swim in a small bathing pool near the hotel. We had a very good supper and then went to our tent. We were much amused by some Mexican boys who came to our tent and seemed much surprised at our mode of living.

Taos [N. M.], August 9, 1878.

We got up early Wednesday, August 7. After breakfasting at the hotel, we left about 6:45 and drove down the San Luis Valley over a level road for 20 miles or so. We camped for lunch on the

North Town, Taos, N. M., from the southwest. (Courtesy Bureau of American Ethnology.)

Rio Colorao, or Red River. We took lunch with some drummers[3] from St. Louis, one named Howison. After lunch we had a pretty hard pull, our road leading over quite a number of steep hills before coming to the plaza of San Cristobal. We went past this plaza up onto the mesa and had a level road for some distance. After driving about two miles to Arroyo Hondo, we camped in the street in the middle of the Plaza.

We got up early and started for Taos before taking breakfast, as we were only 12 miles from it. We started at 5:20, our road lay over several mesas so that we were obliged to walk up the hills from one mesa to another. We arrived at Taos at about 8 A.M. Taos is a very old Mexican town and has been the home of Kit Carson, Gov. Bent[4] and other prominent men. The main business portion is situated on a large square. The houses are all one story and have but few windows except on the inner or court side. This custom is a remnant of old times when the Indians attacked the towns, but now when there is no danger the old custom still remains. We drove through several narrow but pretty clean streets, for Taos has its streets swept regularly. We came to the main square and drove to the hotel kept by H. Dibble. Here we got a good breakfast together with the St. Louis drummers who had come down from Costilla at the same time we had. After breakfast, Mr. Dibble gave me an Apache jug and at the same time I bought from him a handsome San Juan vase. We then engaged a Mr. Scherrick as interpreter and got Mr. Müller to go with us and introduce us to the pueblo Indians. We started for the pueblo village 3 miles from town. This pueblo village, consisting of two large pueblo buildings, one on each side of the river called "casa grande," and numerous smaller buildings, is situated on the Taos River 3 miles from the Mexican town of Fernandez de Taos. We drove up to the pueblo on the south side of the river to the dwelling of the "cacique" or head of the tribe. This man, of the Concha family, is blind and is always led around by his son who is the head war chief of the tribe. We stopped by his house and went up the ladder to the roof of the house. We were then introduced to Jose Maria, the "alcalde" or governor. This man is a very smart Indian and has a good face. After waiting a little while, we were introduced to the cacique who had come up. We had laid in a supply of tobacco at Taos which now became useful. We were taken to an open place above the pueblos on the north side of the river. Uncle Lewis now busied himself learning all he could about the customs, etc. of the Indians.[5]

In the meanwhile, I got out a photograph of the pueblo which amused the Indians considerably. We next put up our tent and got lunch, after which we began to buy things from the Indians. This kept us busy all the afternoon and evening. In the

evening we were much interested listening to the Indians singing their dance songs. It is great fun sitting in the tent surrounded by Indians, giving them pipes to smoke and crackers to eat.

After getting rid of them all, which the alcalde helped us to do, we put our purchases in order and went to bed. We got up early the next morning, Friday, August 9, and began our trading again, for the Indians were ready before we got up. After a breakfast of milk and crackers, we went out to visit some of the rooms in the pueblo. After I got back to the tent, the cacique and all of the chiefs of the tribe came in from their work in the fields. They sat down in the tent and we boys entertained them with cigarettes, etc. and with crackers spread with apple butter. As Uncle Lewis did not come back from visiting the pueblo, the chiefs went to the room of the alcalde and waited for him there. I went also and smoked a "cherook" with them. We had quite a pow-wow and then took leave of them all. As our small change had given out, we arranged with the Indians to come to town and get their pay. We then packed up our purchases and started off. On our way to town we picked up Juanita, a nice clean squaw, and Juan Jose Cuasso, an Indian to whom we owed some money.

We arrived in town and settled with the Indians. We then entrusted our purchases to Mr. Burckert to have them packed and sent to New York. After dinner at the hotel we started for home. Our drive was over the same ground we had been over coming down. We went past our old camp at Arroyo Hondo and went on to San Cristobal where we camped in a pouring rain. Our visit to the Indians had been very satisfactory. We had got an insight into their way of living. For the last three years their crops have been badly damaged by grasshoppers which had made them so poor that they were willing to sell their pottery, etc. for anything. We treated them well and so they were very kind and accommodating to us.

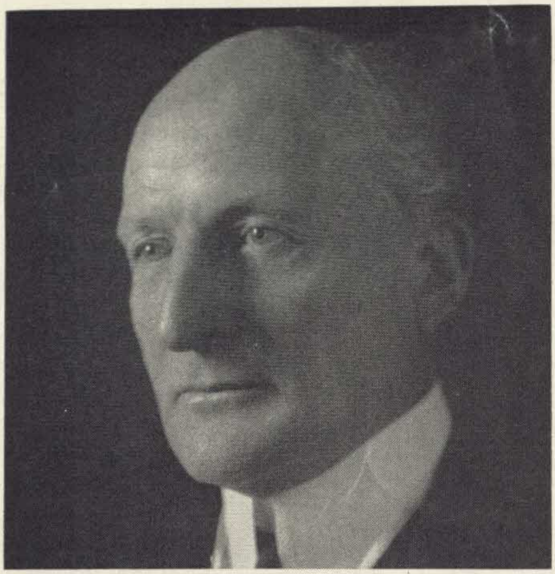
Conejos [*Colorado*], *August 11, 1878.*

We left our camp on the San Cristobal early Saturday, August 10. Soon after we had started it began to rain and it continued nearly all day. We boys walked most of the day so as not to catch cold. We had pretty heavy pulling up steep hills in the mud, for the adobe of this country makes a more sticky mud than any I have ever seen. It was all we could do to move. We camped this side of Rio Colorao and procured some "tortillas." These are merely unleavened cakes and are as tough as leather. After lunch we drove on through mud and gravel until at last we came back to Costilla. Here we stopped for the night with Mr. Blackmore. We left Costilla about 8 o'clock Sunday morning, August 11. We had a bright sunny day which made a nice contrast with the day before. Just before we came to the Rio Grande we saw an antelope. After crossing the river, we camped. We boys amused ourselves washing our clothes and wading across to a little island in the river. On this island I found a very pretty piece of rock which Uncle Lewis told me was "obsidian." After staying some time at the river, we went over the lava field to Conejos. We arrived here about 5 o'clock. We spread our beds in a large room of the hotel. We did not have a good supper. In the evening Uncle Lewis had a long conservation with Major Head,[6] ex-Lieut. Gov. of Colorado, who lives here.

Cañon City [*Colorado*], *August 13, 1878.*

We left Conejos the next morning, Monday, August 12, with the same team. We now drove northward to Alamosa, gradually approaching the mountains. We drove about 12 miles when we met a large train of bullwhackers.[7] Next we saw a coyote. We had already seen two of them two days before. This one did not seem at all frightened at our approach. Donald fired a shot at him and he ran off. After driving a little further we camped for lunch by Alamosa Creek. We then walked on ahead of the wagon and met Gen. Heffernan[8] going back home to Animas City. We got on very well until we came to a very sandy part of the road where we had a hard pull. We got into Alamosa about 4 P.M. We first found Mr. Simpson, the baggage agent. We next got our traps which Henry had left. These we packed and directed for home. We then went to the Perry House and engaged rooms for the night. In the evening we met Judge Ira B. Felton who told us how he had met Will Sloan and had loaned him some money. Alamosa is the terminus of the D.&R.G.R.R. It is a mushroom town as it was begun only a month or so ago and now is well advanced, many stores being completed and many others nearly so. It is the old town of Garland City[9] taken to pieces and put up here. The sound of the hammer can be heard from morning till night. Everyone is busy getting ready for winter.

We were called at 5 A.M. and went to breakfast. Our appearance was very ludicrous, our hats were large freighters' hats, very dirty, our pants were overhauls very much spotted with axle grease, ink, etc. My shoes were all worn out, so much so that when I asked the Porter after he had blacked them how much he would charge me, he said, "The



William Fellows Morgan in later life.

general price is 25 cts., but I will let you have it for 10 cts." We got our seats on the train which starts at 6:20. I now met a friend named Joy from Columbia School of Mines. He is out here in the lumber business at Placer. The fireman on this train is from N. Y. He asked us to tell his father, Charles Freeman, who lives near the McCombs Dam Bridge, that he is well. After the train had been out a little while I saw 4 antelopes a moderate distance from the railroad. We passed Fort Garland and then the remains of Garland City.[9] After going a few more hours we came to the summit, 9339 ft. high. The descent of the Veta Pass was very beautiful. We had just left Blanca and Baldy peaks in all their glory and now we came into full view of the Spanish Peaks, or as the Indians call them, [Huajatolla],[10] which means "Twin Breasts" and it is a good name for they have that appearance very distinctly. After going down some distance we came to Dump Mountain. The railroad goes nearly around it, so some people get out and walk across and meet the train in the Mule-Shoe Curve. As we go around the end of Dump Mountain, we get a very pretty view of the valley. After getting down from the mountains, the railroad skirts along the foothills. We arrived in Pueblo about 1 P.M. and found the firemen's tournament going on. As our train to Cañon City did not leave until 8 P.M., we thought we would look at a game of baseball between Pueblo and Trinidad. We spent our time loafing and writing until at last it became time to leave. During the afternoon I decided to go with Uncle Lewis to St. Louis to the meeting of the American Association for the Advancement of Science. We arrived at Cañon, walked up to the hotel, got our rooms and went to bed.

St. Louis [Mo.], August 21, 1878.

We got up late the next morning, Wednesday, August 14, and changed our clothes for civilian's dress, during which performance Henry appeared. After buying a new pair of shoes, I went to breakfast, after which Donald, Percy, Henry and I got horses and started for Marble Cave. We had a nice ride to the cave, tied our horses to some trees and set to work picking up specimens of marble, etc. We next went into the cave. We had remembered to bring some candles. This cave is very deep and worth seeing. The marble is a species of red marble which when polished is very handsome. After staying for some time at the cave we arranged our specimens and started for home. On our way we saw a good many owls sitting on prairie dog holes. We got back just in time for tea, after which we packed our trunks, as we were to leave at 4:20 in the morning.

We were waked before light the next morning, Thursday, August 15. We went down to the train, got on board and started off. On the way we ran over the fore legs of a cow and so had to shoot her. We took breakfast at Pueblo and then started to Colorado Springs. The scenery on the way was about the same as from Alamosa to Pueblo. We arrived at Colorado Springs at 8:35 and took the stage for Manitou, 7 miles distant. On the way we passed Colorado City, once capital of the territory, and saw the old capitol, an old wooden barn now nearly falling to pieces. We arrived at Manitou about 10 A.M. and took rooms at the Beebee house. We boys made arrangements to walk to the top of Pike's Peak, for we had left Uncle Lewis in the train, he not caring to go to Pike's Peak. We started out about 10:30 A.M., taking a drink at one spring on the way up. We followed Fontaine Creek for six miles or more and then we came to the Lake House 4¾ (miles) from the summit. As it was cloudy and I did not see the trail, I made a mistake and went down the old trail to Colorado Springs. I did not find out my mistake until I had walked four miles. I then had to turn around and foot it back. In the meanwhile, Donald and Percy had walked on towards the summit. I caught up with them about two miles from the summit. We sat down and ate some lunch. It then began to blow very cold and to rain, and as it was very cloudy, we decided to go back. We got back to the Lake House at 6 P.M. From here to Manitou, 8 miles, it took 85 minutes. We got back to the hotel at

7:30, tired out, hot, and dirty. We got some supper, cleaned up and visited the springs, after which we went to bed. The hotel is a very nice one although the prices are very high. The table is very good, the best we found.

We were waked at 6 A.M., Friday, August 16. We took our breakfast and then seats on the top of the stage. When we were seated, Percy pointed out to me Rose Keep, and sure enough, there she was in deep mourning. At the same time we saw her, she saw us as she was going down in the stage. She came up on top and we had a long talk on different subjects. On the way down, we could see a little of Cathedral Rock in the Garden of the Gods. We took our seats in the train, arriving at Denver at about 12. As Uncle Lewis had gone to the Wentworth House, we boys stopped there, while Mrs. Marshall Field,[11] Rose Keep, Miss Scott and the children went to another house. After dinner I paid a visit to Bishop Spaulding, but found that he was in Europe. I had a very nice conversation, however, with Mrs. Spaulding and Mr. Thomas, an assistant of his. We boys next went to see Mr. C. M. Henry, Supt. of the Colorado Central, from whom we got passes to Georgetown.

We took the omnibus to the station and got seats in the train. The road runs up the cañon of Clear Creek. At Golden we changed cars, taking the narrow gauge division. After getting some way above Golden, the cañon narrows very considerably and gives some beautiful scenery. I had a long talk with Rose Keep, going to Georgetown.

When we got there, we drove in an omnibus to the Barton House. I registered for the whole party. After supper, I enquired where Mr. F. M. Taylor lived, and then went to his house. I went into the Concentration and Excavation works[12] and saw them in operation. After I had seen the works, Mr. Taylor came in and I introduced myself. I then went back to the hotel and found it very late.

The next morning at Georgetown, August 17, I took leave of the boys, who were to stay over and meet me at Chicago. I took the train at 7:25. I saw the large rock near Forks Crk. called "Mother Grundy." On the way down the front wheels of the car I was in went off the track, but they soon arranged it.

When I got back to Denver I walked to the hotel and saw Uncle Lewis. After dinner I went down to the station and kept seats for Uncle Lewis and myself. Our train started at 3:15. After we had been gone a little while, Uncle Lewis introduced me to Judge Cobb, ex-Chief Justice of Kansas.

During the afternoon I bought some views and a few Indian traps, made by the Comanches of New Mexico. In the evening, Uncle Lewis went to the drawing room car, but I made myself comfortable in the common car. The scenery during the day was the same as we had seen on the A.T.&S.F.R.R. We went along the Smoky Hill River for miles and miles. At the beginning it did not have much water, but it gradually increased in size.

The next morning, Sunday, August 18, I busied myself preparing my paper for the meeting of the American Association for the Advancement of Science. Judge Cobb amused us part of the day telling us stories and anecdotes. At Junction City we crossed the Republican River and saw it join the Smoky Hill River thus making the Kansas River. This part of Kansas is wonderful in appearance. The corn has grown to a height of 9–12 feet. The road runs along the north bank of the river. We arrived at Kansas City about 5 P.M. and immediately took our things into the Missouri Pacific train which left a few minutes after we got there. It soon got dark, so I arranged myself comfortably for a night's rest.

The next morning, Monday, August 19, we arrived in St. Louis and went to the Lindell Hotel. After breakfast we set to work on our papers. This kept us busy all the morning. In the afternoon, Mr. Bandelier[13] arrived from Highland. We had a long talk and then went down to parlor 22, reserved for the Association, and Uncle Lewis introduced me to Prof. F. W. Putnam,[14] Permanent Secretary of the Association. We then went out for a walk and Mr. Bandelier took a package to the train. After supper we went up to the room and prepared our papers. St. Louis is a very hot place, so much so that the perspiration would roll off you even when quiet.

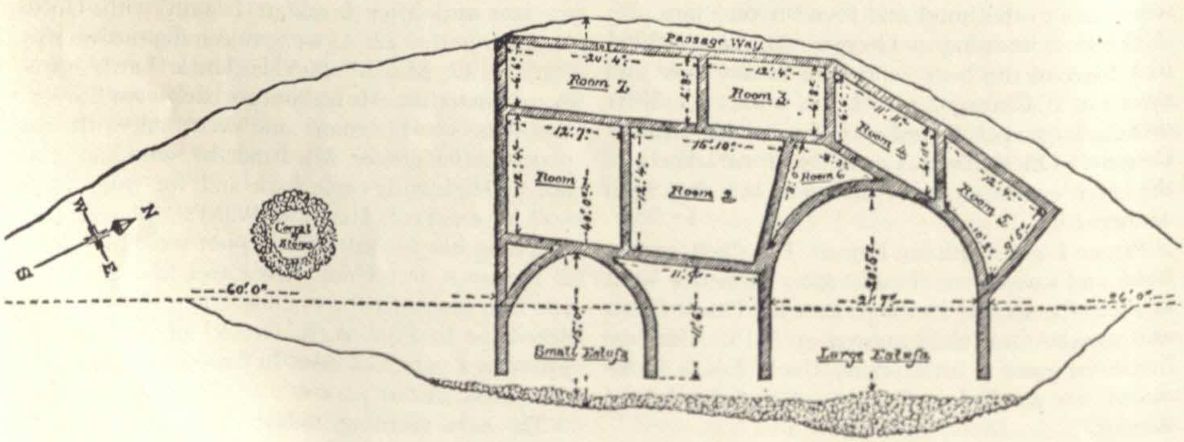
The next morning, Tuesday, August 20, I woke up late and after breakfast I went with Uncle Lewis to parlor 22. As we were coming out we met Prof. O. C. Marsh[15] of Yale. Uncle Lewis introduced me to him. He had just got back from Europe. We then went upstairs and went on with our papers. After dinner, Mr. Bandelier, who had gone out to Highland, came back and we went for a walk to see Prof. Henry A. Ward's[16] plaster casts which he has for sale. We did not see Prof. Ward at the store, but Uncle Lewis and Mr. Bandelier saw him later. In the evening Uncle Lewis introduced me to Prof. A. R. Grote[17] of Buffalo who I think is a very nice man. In the evening we wrote some more of our papers.

The next morning, today, we woke up earlier. After breakfast I went out and bought some draw-

ing paper, etc. I then went upstairs and continued writing until nearly 10 o'clock, when we went to the opening meeting. Uncle Lewis introduced me to Dr. Dalrymple of Baltimore. After the general session was over, the meeting adjourned to meet in sections. After that meeting was over, I waited for Uncle Lewis who, being elected to the Sectional Committee,[18] had business after the meeting. I was now a member of the Association, as I was elected in the morning. After dinner I went on with my work for a while until Prof. Grote came up and asked us for our papers to write an abstract for the *New York Tribune.* I then went down to parlor 22 and while he was at work I drew my ground plan and made an abstract of my paper. After tea I worked a little and then we all went to Washington University to hear the addresses of the two vice presidents, Professors Grote and Thurston.[19] After the proceedings were over, as there was a general invitation to go to Schnaider's Garden and the omnibuses were at the door, we all got in. Our party was Uncle Lewis, Prof. Ward, Mr. Bandelier, and myself. We walked around a little while and then sat down at a table and ordered some beer. This garden gave us a good insight into beer gardens in Germany, women, even, drinking beer. We enjoyed it very much.

We got up pretty early the next morning, Thursday, August 22, and I finished my ground plan. We then went down to breakfast and sat at Prof. Marsh's table. After breakfast we went to our room and finished our papers. Mr. Bandelier then came up and made an abstract of Uncle Lewis's paper which I wrote. We then went downstairs and hired a carriage in which Uncle Lewis, Prof. Grote, Mr. Bandelier, and myself drove to the Washington University. The first thing I did was to procure my member's ticket, after which I went up to the meeting of the Association in the chapel. After some more general business the Association met in sections as on the day before.

The first paper read [in Section B] was by Mr. Henderson of Springfield, Illinois. It was on some excavations in mounds near Naples, Ill. He had with him some beautiful specimens of pipes, copper axes, etc. used by the mound builders. His paper was very interesting and called forth a good deal of comment. Uncle Lewis raised a point that the mound builders were not ancestors of the Indians, in which he was supported by Prof. Marsh. After this paper there were several down on the programme which were omitted. Then came a paper by Prof. Riley[20] of Washington. It was an entomological paper and so I did not understand it. Besides, as my paper came soon, I was quite frightened and did not listen much. Next came a short paper and after it a recess of ½ hour. During this recess I had a chance to put up my diagram and collect my thoughts, for I was still frightened. At the end of the recess, the chairman came in, called the section to order and announced my paper, "Description of a Cliff-House in the Cañon of the Mancos River, with a Ground Plan of the Structure."[21] I was now very frightened, for this was my entrance into the arena of a scientific life. You can imagine my condition from the fact that I forgot to say "Mr. Chairman" at the beginning of my speech. I took my position by the platform and began in a loud voice, for I had not lost it. I soon regained confidence and read my paper as if I had done nothing else all my life. I referred every now and then to the plan. After reading 15 or 20 minutes, I finished and went back to my seat. My audience, which was quite large, applauded me, which made me feel much better. Prof. Putnam, Major Powell,[22] Mr. Mason,[23] and Mr.



Ground plan of cliff-house, drawn by W. F. Morgan for his paper presented at AAAS meeting in 1878.

Henderson made remarks on my paper, and then Mr. Bandelier made some remarks, after which Vice President Grote, Chairman of the Section, said, "We have all listened with pleasure to the paper of Mr. Morgan, and compliment him on it. We also wish to congratulate ourselves on the advent among us of another anthropologist who may follow the footsteps of his illustrious uncle." These remarks called forth some applause. Thus ended my first appearance on the scientific stage, a half hour which I shall never forget; I feel so proud of it.

Next came two papers by Uncle Lewis[24] which were afterward commented on, and then came Mr. Bandelier's paper.[25] It was a very interesting paper, but owing to want of time, he could not finish it. It is a paper of great importance and when in print will be of much value to ethnology. After the meeting I went with Uncle Lewis to the hotel and took dinner at Prof. Marsh's table. After that I went to my room and packed, for Uncle Lewis had decided to leave that same evening. We went downstairs and met Major Powell, who congratulated me anew. Uncle Lewis then had a long talk with Major Powell, who then introduced us to Capt. Dutton.[26] We then went to a beer saloon. Our party was Uncle Lewis, Major Powell, Capt. Dutton, Mr. Bandelier, Marcus Benjamin,[27] and myself. We staid at the beer garden a little while and then walked back to the hotel. Major Powell tried to induce Uncle Lewis to stay, but in vain. Prof. Marsh then came downstairs and he tried too, but with no better success.[28] We had made an engagement to go to Dr. Engelmann's[29] house at 5:30, but this we had to break. So, after taking tea, we had another talk with Major Powell. Prof. Marsh then wanted us to go up to his room and see some pottery from Chirique, Central America, but it was too late. Prof. Ward had engaged to meet us at the Depot, so we found him and Mr. Howell, his brother-in-law, at the station. We took the C&A train at about 8 o'clock. We left Prof. Ward at Main St. Mr. Howell staid with us. During the night I had a long talk with him.

We traveled all night and arrived (at Chicago) Friday, August 23. As it had been agreed that I should stop at Chicago to wait for Donald and Percy while Uncle Lewis went on to Rochester, I told Uncle Lewis goodbye and took the Omnibus to the Grand Pacific Hotel. During the morning I busied myself writing my dairy, etc. In the afternoon I went to the Chicago Exposition Building where Prof. Ward has placed his "Mastodon." I went upstairs and took a good look at it and then came back to the hotel.[30]

NOTES

1. Conejos is the oldest existing village in Colorado. The original settlement in this vicinity was Guadalupe, on the opposite side of the river from Conejos. According to local tradition, a pack mule of a Spanish traveler balked there. After ordinary means had failed, the owner vowed to build a church on that spot dedicated to the Virgin of Guadalupe if the mule would move on. It did. The vow was fulfilled and the village of Guadalupe grew up about the church. Because of recurrent floods, however, the inhabitants moved to higher ground across the river. The name of this village became Conejos, but its church is still the Church of Our Lady of Guadalupe.

2. Lewis Henry Morgan is called "Uncle Lewis" in the diary.

3. Drummers: traveling salesmen.

4. Charles Bent (1799–1847) was an early western pioneer. In 1835, at Fernandez de Taos, he married Maria Jaramillo whose younger sister later married Kit Carson. General Kearny, after his bloodless conquest of New Mexico, appointed Charles Bent civil governor on September 22, 1846. The following January, Governor Bent and several others were assassinated in an uprising at Taos. Major Price led an expedition against the Mexicans and Pueblo Indians involved, and quickly subdued them.

5. See *House Life,* pp. 144–153, 182–183.

6. Major Lafayette Head fortified and successfully defended Guadalupe (see Note 1) from the Utes in 1855. When Colorado became a state in 1876, he was elected its first lieutenant governor.

7. Bullwhackers: drivers of oxen.

8. General Heffernan, see *House Life*, p. 188.

9. A short time before the arrival of the Morgan party, the houses, stores, and churches of Garland City had been moved on flat cars to Alamosa. They had been set up and occupied in a few days, but work on them was still going on.

10. When he was writing his diary the next day, evidently William Fellowes Morgan could not remember the Indian name of the Spanish Peaks, so it is omitted in the diary. The name Huajatolla has been inserted by the editor. This Spanish-Indian name means "Breasts of the World" rather than "Twin Breasts." The twin peaks rise to heights of 12,683 and 13,623 feet, respectively, and are quite a distance from the Culebra Range of which they are a part. They served as landmarks to guide the early explorers. The Utes believed that the peaks were inhabited by evil spirits.

11. Nannie Scott Field, the first wife of Marshall Field. In the latter part of the diary, Mr. Morgan tells about his visit to their home in Chicago.

12. The settlement of Georgetown, Colorado, began with the discovery of gold there in 1859. The placer mines soon gave out and a period of stagnation followed. Shortly afterward, however, Georgetown became the most important silver mining camp in Colorado and remained so until the great Leadville silver

strike occurred in 1878. Naturally, W. F. Morgan was interested—he later specialized in mining.

13. Adolph F. Bandelier (1840–1914), archeologist and specialist in Spanish-American history. A biographer says of him, "No American archeologist has depended as did Bandelier on historical sources and no American historian has checked his historical work so carefully by a study of archeological materials." See *House Life,* p. 84.

14. Frederic W. Putnam (1839–1915); assistant of Louis Agassiz, 1856–64; archeologist; Curator of Peabody Museum; Permanent Secretary of AAAS, 1873–1898; President of AAAS, 1898.

15. Othniel C. Marsh (1831–1899) head of expedition in 1870 to study cretaceous and tertiary fauna in the west, presented his great collection to Yale, January 31, 1878; President of AAAS in 1878.

16. Henry A. Ward (1834–1906), student of Agassiz, best known for his geological collections and for the exact replicas he made of rare fossils and animals. The "Ward Cabinets," usually containing both natural specimens and replicas, were very popular, selling at prices up to $100,000.

17. Augustus R. Grote (1841–1903); entomologist and Curator of the Buffalo Society of Natural Science; Vice President and Chairman of Section B of the AAAS in 1878.

18. At that time the AAAS contained only two sections: Section A—mathematics, astronomy, physics, chemistry, mineralogy; Section B—geology, zoology, botany, anthropology. There were however, three subsections C—chemistry, D—anthropology, E—microscopy. Lewis Henry Morgan was chairman of Subsection D in 1875–76. At the 1878 meeting, although one-third of the papers of Section B were in anthropology, it was decided not to subdivide Section B, so Subsection D did not organize.

19. Robert H. Thurston, Stevens Institute of Technology, Vice President and Chairman of Section A of the AAAS in 1878.

20. C. V. Riley, U. S. Entomologist.

21. William Fellowes Morgan's paper is published in full (with the diagram of the ground plan of the cliff-house) in the *1878 Proceedings of the AAAS* (1879), pp. 300–306.

22. Major John Wesley Powell (1834–1902); Director of the U. S. Geological Survey. He led the first expedition through the Grand Canyon of the Colorado in 1869.

23. Otis T. Mason, Columbian University (now George Washington University). He presented two papers on Indian language and antiquities.

24. The two papers presented by Lewis Henry Morgan were: "Remarks on the Ruins of a Stone Pueblo on the Animas River, New Mexico, with a Ground Plan" and "Observations on the San Juan River District as an Important Ancient Seat of Village Indian Life." The contents of both of these papers are included in *House Life,* on pp. 172–188 and 192–197, respectively.

25. A. F. Bandelier's paper, "On the Sources for Aboriginal History of Spanish America," is published in full in the *1878 Proceedings of the AAAS* (1879), pp. 315–337.

26. Clarence E. Dutton (1841–1912); geologist; member of U. S. Geological Survey of the Rocky Mountains; published important findings on the nature and speed of earthquake vibrations .

27. Marcus Benjamin; graduated Columbia School of Mines, 1878; became editor of the publications of the U. S. National Museum.

28. Had President Marsh known of the telegram he was to receive the next morning, he might have had "better success." At the general session on Friday morning, August 23, occurred the high point of the meeting. After Thomas A. Edison (elected Fellow at this meeting) had been introduced, President Marsh announced to the members of the AAAS and *to Edison* that he had received by telegram that morning the welcome news that the Grand Prize of the Paris Exposition had just been awarded to Thomas A. Edison "for one of the most wonderful inventions of the age." Edison's newly invented phonograph was exhibited for the first time at the 1878 Paris Exposition.

29. George J. Engelmann of St. Louis, "venerable" botanist and one of the charter members of the AAAS, presented a paper and was elected a Fellow at this meeting.

30. William Fellowes Morgan remained in Chicago six days, meeting the boys there and leaving with them on August 28. The diary ends the next day.

Industrial Research as a Tool of Industry*

S. L. HOYT

The author has been engaged in industrial research for more than thirty years, the past fourteen of them at Battelle Institute, where he has specialized in problems of physical metallurgy. He is an authority on the selection of metals and alloys for engineering purposes. Dr. Hoyt has made numerous contributions to technical journals and is the author of several books and monographs, including Metals Data. *He received his B.S. in mining engineering from the University of Minnesota and did graduate work at Charlottenburg (Germany) Technical School and at Columbia University, where he received his doctorate.*

I FIRST MET Dr. G. K. Burgess on July 22, 1912, in Washington, D. C., and came to know him as a very warmhearted and wholesome person. As Chief of the Division of Metallurgy, he had the high ideals of the true scientist, and the ready acceptance of contributions from his laboratory showed that he lived up to those ideals. Later, in his capacity as Director of the National Bureau of Standards, he applied his talents to a broader field, and the nation suffered a great loss with his death in July, 1932.

As I now recall it, a large share of his work was done in order to give reliable answers to basic questions of metallurgy. In this way, he provided others with a firm factual basis for handling problems that arose in their work. He felt that by advancing and perfecting basic metallurgical knowledge he could contribute to the improvement of the technology of the metal arts. I also recall that he had a fine appreciation of practical research of the almost purely industrial type.

As I have just said, Dr. Burgess believed in what I am calling Industrial Research and participated in it. This particular applied type of research has become a great national asset and is now the almost universal method for improving existing technology and for putting to use the findings of science. There should be no need to point out what technology means to industry and hence to the economy and prosperity of the nation.

Not everyone agrees with this viewpoint, however, and before proceeding to a positive appraisal of my subject, I should like to dispose of the negative view. A man who for years has been in the upper circles of our government has stated it as his belief that the country would be very well off if technology were frozen, with no further research and development, and no additional applications of science in a practical way. This is an amazing statement, coming from an intelligent man; although it is also a challenge to the validity of the system that has advanced our material civilization so remarkably. What is the rebuttal?

Upon thinking it over, I could see no reason why the statement applied specifically and uniquely to the time that it was made; it could have been made just as appropriately the preceding year, or ten years earlier, or one hundred years earlier. At the end of my regression, I found that I was back to Adam and Eve. You may recall that they lived under the simplest possible conditions, with apples for food, fig leaves for clothing, and nothing to worry about except a serpent. Possibly this is an oversimplification, yet I suppose there are many who think that no further technological development is desirable. If a highly educated and intelligent man could feel that way about material progress, I felt that I had to justify my own belief in it. Intuitively I felt that the gentlemen was wrong and that what man has accomplished technologically to improve his working and living conditions, his health and leisure, and his cultural and recreational pursuits, is good in the most devout sense of that term. The fact that we first desire and then enjoy these things is, to me, a powerful argument in their favor. However, I chose not to go back to Adam and Eve to justify my belief in technological development; I settled for a century, and it is interesting to contrast the situation as it then was with what it is today.

Not many more than one hundred years ago the first crude railroads were being built. The inhabi-

* Burgess Memorial Lecture, American Society for Metals, Washington, D. C., February 9, 1953.

tants had hardly become used to the wonders of the new canal system when steam power revolutionized transportation on both land and water and greatly extended the factory system of manufacturing. That, it will be recalled, was the period of the rise of the Industrial Revolution. Presumably, this learned gentleman would have frozen technology at that time, too. If so, he would deprive the worker of the benefits of mechanical and electrical power and would leave the family unit largely dependent upon its own resources with its activities limited essentially to the sphere of the backwoodsman.

It is interesting to note what another man had to say of that same system; yet this was said in the year 1828. This man was likewise learned, but he was also enlightened. I refer to the German poet, Goethe. In one of his novels, *Travel Years of Wilhelm Meister,* he considers the impact of the Industrial Revolution on society. At that time there was great agitation against the new order; the cry was to the effect that the rich will get richer while the poor will become slaves or be thrown out of work. In this book, Goethe takes time out from telling about Wilhelm Meister (himself) to describe life in a new, though fictitious, factory town. The inhabitants were weavers, but, instead of doing handwork at home, they operated power looms in the factory. Here came the vision. The product of those looms was of good quality and relatively inexpensive; hence the owner readily located markets among others who were likewise enjoying better remuneration for their services. Out of this exchange there grew trade and commerce which gave many people better employment than they had previously known. They all benefited because they could then enjoy a standard of living that was formerly beyond their means. In this way, Goethe presented a picture of the benefits of industrialization in terms of an improved standard of living. Personally, I am willing to let Goethe provide the rebuttal; we can profit today from the thoughts of this great man on social and management problems.

This is perhaps a good time to define a little more closely what is meant here by industrial research. First, it is research, an investigational discipline that is pursued in order to uncover new facts or principles. Research is usually classified as basic and applied. Industrial research is mostly of the applied type because there is an immediate and practical use in mind which someone is more or less patiently waiting to apply. Basic research is commonly done merely for information's sake and there is seldom anyone waiting to use the findings, patiently or otherwise. Industrial research may also avail itself of basic research, however; a good example is the current work on titanium-alloy diagrams. I think we may say that basic research gives us the seeds which industrial research then plants and nourishes, whereas industry produces the fruit. Obviously, without the seed no fruit will be forthcoming; but, aside from paying my respects to basic research, I shall concentrate on the applied type.

Dr. G. K. Burgess

I wish to emphasize that industrial research is something new. Whereas science and scientific research grew up together, that was not true of technology and industrial research. For many centuries, technology advanced by clever inventions and by the lessons of experience. The history of cathedral construction illustrates this point very clearly. Thus, building one house taught how to build another one better. From the memoirs of an elderly engineer, I recently read that his firm erected its shops in 1880 by just such rule of thumb methods. They used the committee procedure of taking mutual counsel. Furthermore, there was not an engineer on the staff; today, however, a firm in that type of business would have both engineers and laboratories.

To get along in history to the start of industrial research, we come to relatively recent times. I do not know whether the Schenectady laboratory was

the first full-time industrial research division in this country, but it was surely a bold pioneering step a bare fifty years ago. For, although the Industrial Revolution was then far advanced, management in general was not yet ready to adopt the experimental method in a business enterprise. On the other hand, it is also true that scientists failed to visualize industrial research as a career; in fact, it took several years of commuting between Boston and Schenectady before Dr. Willis Whitney could make up his mind about the nature of his life's work. Since this attitude of management toward research is important for an understanding of the historical development of my subject, I should like to quote a pertinent statement of a very learned man, Max Planck, that bears on the factor of the human element. He stated: "An important innovation rarely makes its way by gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul.* What does happen is that its opponents gradually die out, and that the growing generation is familiarized with the ideas from the beginning."

Let us pause a little longer at the turn of this century, the period of the inception of industrial research. The old system of technological development then in current use had already given us railroads, steamships, electric street cars, the electric arc and incandescent lamps, electric power, the telephone and telegraph, the phonograph, gasoline and Diesel engines, automobiles, farm machinery, machine tools, the Bessemer and open hearth processes of steel making, numerous steels and alloys, etc. Compared to a century previous, use of the primitive methods had advanced material civilization tremendously. But let us take note of the scientific discoveries of that period that awaited development and use. Steinmetz was at work on alternating-current theory and equipment and on the long-distance transmission of electric power. Roentgen had discovered his mysterious x-rays and Hertz his curious electromagnetic waves, while Becquerel had discovered natural radioactivity, and the Curies had isolated radium. There also was the baffling Edison effect which was to be harnessed for radio and the electronics industries while, in the field of metals, numerous rare ones had been isolated as laboratory curiosities and metallurgy had just rounded the corner as a budding science.

Beginning at about that time, the industrial use of the products of science was beyond the capabilities of the empirical and intuitional methods. The handwriting was on the wall, and here and there the especially enlightened managers saw the need of using science in applying science. As an example, I would like to cite the case of the electrical industry, in order to illustrate the contribution of the experimental method to the development and growth of industry.

I have mentioned the Schenectady laboratory and Dr. Willis Whitney, who is undoubtedly the dean of industrial research directors in our country. It was E. N. Rice, Jr., president, and Albert G. Davis, in charge of the patent division, who had the inspiration to have the General Electric Company enter into research on a formal scientific basis. It was also Mr. Rice who secured the services of Steinmetz and who, in general, saw that the electrical industry was in its infancy and in need of men of specialized scientific training. This movement was notably assisted by Elihu Thompson, who was brought into the fold by the combination of the Thompson-Houston Company and the Edison Electric Company, to form the General Electric Company. A little later, F. S. Terry and B. G. Tremaine established the National Electric Lamp Association. They were great organizers and managers, but our interest in them here comes from their recognition of industrial research as an essential part of their business. In those early days, that was vision indeed.

For the purpose of housing their home office and research and development facilities, they installed Nela Park in Cleveland, sometimes referred to as the University of Light. A small effort went into basic research on light and radiation, but it was to applied research that the greatest attention was paid. For this, they set up development and testing laboratories and a small or pilot plant factory. I think it is consistent with the vision that they displayed in other activities of their organization that they insisted that these research facilities improve incandescent lamps and the methods for making them in order to produce more light for less money. Your incandescent lamps have been doing that for all of you these many years as a symbol of what industrial research is capable of accomplishing when under good management.

The chemical industry is also a good example because, like the electrical industry, it had to have scientifically trained men in its employ. It has long been a leader in this field and has plowed back a higher percentage of its sales into research than have other industries. Then, during World War I, there came a powerful stimulus for research when the importation of chemicals from Germany

* This refers to the change in Saul who, while on the road to Damascus, gave up his early thinking to adopt the teachings of Jesus; whereupon his name became Paul.

was cut off. Furthermore, it was the chemical industry that introduced the first privately operated institute for industrial research. That was the conception of Robert Kennedy Duncan in 1907 which later became the basis of operations of the Mellon Institute in Pittsburgh.

The automobile industry did not lag far behind, possibly because it had the advantage of certain outstanding personalities, such as Ford and C. F. Kettering. Those men of vision led the way, while the terrific competition for the patronage of the purchasing public resulted in the survival of companies whose managers recognized the advantages of research and development for improving and reducing the cost of the product.

One of the miracles of that age was the development of the airplane by the Wright brothers. I mention it here although its classification is somewhat difficult. I think one would say that the work of the Wrights was invention and development, although they made use of engineering principles to guide them patiently through a maze of difficulties to reach their goal. It was surely experimentation, but not the systematic type of research in which significant variables are studied and their effects evaluated. Another reason for mentioning the work of the Wright brothers is to suggest that we should not let overenthusiasm for formal research blind us to the possibilities of advancing by other techniques.

Turning to the field of metallurgy, it was during the decade which included World War I and the early twenties that a conspicuous expansion occurred in the use of applied or industrial research. As I recall it, this was first noticed in the producing industries, particularly in their aluminum, zinc, and nickel branches, with some healthy signs of progress in the field of precious and rare metals. Not long thereafter, similar developments occurred in the copper, magnesium, tin, and steel branches, with cast iron and high alloys coming in shortly before World War II. The situation regarding lead is not clear, although it is known that this branch has supported considerable research.

The consuming industries form a heterogeneous group with great variations in size, etc., of the individual units and in facilities for doing research. Furthermore, in the past, they have been relatively slow in taking advantage of the benefits that come from research. The electrical industry is a consuming industry in metallurgy and it is only fair to say that it showed much less zeal in tackling its consumer problems than it did in developing its own new products. In the past, this has been true in numerous other instances. This is mentioned, not as a note of censure, but in order to arrive at a reason for the tardy adoption of the research method by the consuming industries.

I have repeatedly run across the attitude of the consumer that his supplier will give him the information he needs for fabrication and even for his products when in service. Any company that is large enough to afford research, I believe, makes a grievous error with such a policy. This is not to be interpreted as a reflection on either the ability or the integrity of the supplier, but is intended, rather, to point up certain principles of the use of industrial research. For I believe that the consumer is in a position to know and should know more about the requirements of fabrication and use of his products than is possible for his supplier to know. I also believe that if the consumer acquires this knowledge, which will go well beyond that needed for purchase specifications, he can do a much more intelligent job of purchasing his own raw materials. He will know just what points he must insist on in procurement, both in and out of the specifications, and what points he can concede. The availability of this kind of information makes selling and purchasing a more intelligent process.

A few examples may bring out more clearly what I have in mind regarding the use of research in support of procurement. At one time we had the problem of purchasing a large amount of steel sheet for single-coating enameling. There was an obvious answer to the steel question—the purchase of the non-reboiling enameling quality steel such as is used for household appliances. However, the competition in our market was such that we knew we could not afford such expensive steel. After we had made our studies of steel behavior in fabrication and in enameling, we were able to tell our suppliers just what we needed. With their intelligent cooperation, we found that we could use a cheaper grade of sheet, and there followed a sizable and satisfactory business. We also got a new feeling for the maxim of the engineer that "good enough, is best."

Another experience likewise brings out the value of this use of industrial research, only more pointedly. This was a case of procurement of steel in large tonnage for an application which had strength as a primary requirement. During the preliminary or screening tests a certain steel was selected which carried an extra cost of \$4.00 per ton. The reason for using this type of steel, however, was not its strength but to give our customers the quality needed for satisfactory performance in

service. That extra charge was a target to shoot at and so we went to work. We put into practice the principles that I have mentioned, and after considerable laboratory work and observation of fabrication, and with the close cooperation of the supplier, we learned that with a rather well-defined steelmaking practice we could secure the necessary quality at a cost of only $2.00 per ton extra. With large tonnage purchases, that saving paid for a lot of research. The supplier got the business. The moral here is that by systematically studying the steel variables and correlating them with behavior in manufacture and with tests of the quality of the finished product, we were able to effect these savings legitimately. It meant that we had to learn what the steel mill could supply, what we could do in our own shops, and what our customers had to have for satisfactory use of the product. The steelmaker could not be expected to acquire all that knowledge and, furthermore, we, and not the ultimate user, were his customer.

This use of industrial research as a guide to intelligent procurement is perhaps less commonly given as a function of the research laboratory. The more common uses for developing new products and processes and for selecting materials and their treatments are too well known to require detailed discussion here. However, practically every product and operation is a suitable subject for industrial research sooner or later, because science is forever making new contributions with possibilities for exploration, and technology is in a continuing state of flux.

The Federal Government has been a major factor in advancing technology and hence in promoting industrial research. There are numerous agencies involved and they purchase a wide variety of products, but they have one trait in common: they always want something better. As a naval officer once told me when I plaintively asked him how he thought we were going to make what he wanted, "We just give you the answers; the rest is up to you people." I also recall an experience at a steel foundry when I was doing some work during World War I. On my first trip there I found the superintendent fussing and fuming about having to work to government specifications. "Well," he said, "we've always just poured our castings and let them go. Now these government specifications call for pull tests and we're held down tight on elongation." I told him he would have to normalize his castings to refine the structure. Many steel foundries discovered "elongation" during that period, while during World War II, with the tight alloy situation, they were required to exploit liquid quenching.

Naturally, experiences of this kind could be repeated over and over. What they teach here is the influence which the military services exert on technology by always demanding more and more. With that kind of pressure, management is faced with increasingly difficult problems of devising the best technology to meet the situation. The recent and current problems of gun-barrel erosion, steel cartridge cases, cold extrusion, low-alloy steel armor plate, welding armor, steel quality for ship hulls, the alternate and boron steels, etc., point up the utility of research today. Even more illuminating is the recent work on the development of jet propulsion, the utilization of nuclear energy, and such materials as the super-alloys, titanium, and zirconium. It is inconceivable that such matters could be handled effectively if we could not apply the methods of industrial research. There are other cases which I might have used, some of which were shining examples of the intelligent use of industrial research and some definitely not so shining. Thus, there are the producing and refining branches of the oil industry, the gas-transmission industry, the railroad and transportation industry, the welding industry including welded structures, the pulp and paper industry, the printing trades, and agriculture. In all these cases, industrial research has been used to a greater or lesser extent, depending largely on the vision and progressiveness of management and secondarily on the potential benefits which might come from research.

The broad question of who should do the research has never been objectively analyzed, to my knowledge. On the other hand, the electrical industry waited for no one to do the things that needed to be done for its future growth. The same is true of the chemical and various other industries. On the other hand, and leaving aside industries which have been backward in helping themselves, there is another case of quite a different type. Again, I shall use an example with which I am familiar. For years, the petroleum industry wasted enormous quantities of natural gas by loss into the air, and this at a time when that fuel could have been profitably used in our industrial and residential areas. The question is, should it have initiated a research program to find out how to transport that gas economically to market? It would have paid off handsomely if such a program had been successful. Or, since it was a transportation problem, should the railroads have undertaken it? Of course they did not do so, nor did other agencies

which might have taken up this project out of self-interest. Actually, the solution came from the pipe industry. In the long run, management is wise not to go outside its own sphere. Yet I believe that this aspect of industrial research deserves more attention now than it has received in the past. Where the interest of a consuming company or industry is greater than that of the supplier, the former might well undertake research from which it stands to gain. That is especially true today, because there are university and private research institutes which can supply the facilities needed for practically every branch of technology.

It is now appropriate to look at industrial research against the backdrop of history to appraise its position in the scheme of things. I reiterate that whereas basic research resulted from man's desire to know more about his world and the way in which it functions, industrial research came from his desire to make a better world for himself. To see this interesting development in its proper perspective, one has to philosophize a bit, and I look at it as follows.

The lower animals differ from man in that they live according to a fixed pattern. The coelacanth fish that was recently caught off the coast of Africa, and which has been termed a "living fossil," still lived essentially as its ancestors did 150,000,000 years ago. The squirrels in my yard retain the living habits that squirrels presumably have always had. They desire no research laboratory because, regardless of their mental capacity, they remain satisfied with their mode of living. Just as it may be reasonably postulated that one of the early fishes was altered by fortuitous mutation and thus was able to come up onto the land, so it can be conceived that something comparable occurred in some primitive member of the ape family and produced the first man. That early progenitor had a distinctively human characteristic—more likely a combination of two characteristics—which was something akin to the desire to invent and to make living better and easier for himself. In response to this urge, he invented tools and fire, made clothing, learned how to provide shelter, and the like. It is significant that he has passed that trait on to us of the present generation. In the material world, the greatest chain reaction was not that of the atom bomb but the one that was initiated by that first uniquely human mutation and that has since been transmitted by inheritance and has been improved by selection.

In the field of industrial research, we cannot expect much more of prehistoric man. So let us leave him with his fire, his crude implements, and his wheels, and pass on to recorded history to follow the development of technology and research. Until the introduction of industrial research, there were three periods when man reached new heights of intellectual output. Although the contributions of the great thinkers of those periods profoundly changed all phases of human activity, and in spite of continuing developments in engineering and technology, in no instance did man devise the general experimental method for advancing technology.

Our first period was that of the early prophets of the Old Testament, of the Greek philosophers, and of the craftsmen and engineers of Rome. We are not concerned here with their intellectual triumphs or with their important contributions to religious and moral teachings, but with what they accomplished in technology. This, it appears, was limited to engineering structures such as roads, buildings, temples, ships, and aqueducts, certain engines of warfare, and the products of the crafts. We are, of course, indebted to them for some of the sciences, and the Greek philosophers developed the unique human function of abstract reasoning. It is beyond doubt that they were progressing in most ways, but those fields that concerned their material welfare were left to the engineers and artificers and were handled by the empirical method.

The next period was the late Renaissance, when Europe was swept with a wave of new thought that freed it largely from the frozen dogma of the past. It was then that the physical sciences got their start with the experiments of Galileo in mechanics and the work of Copernicus and Newton on the movements of celestial bodies. Shortly thereafter, Newton formulated his rules of procedure for solving problems of the physical sciences. Thus, at a relatively early period, it appears that science, at least, was off to a sound start in both methodology and philosophic guidance; but technology proceeded in its ancient and empirical groove. The well-known prophecies of Roger Bacon and the inventions of Leonardo da Vinci demonstrate man's capabilities at that stage of his development, but the invention of a method for making inventions still lay beyond him.

The third period began about the middle of the eighteenth century when the Industrial Revolution was ushered in with Watt's steam engine and when civil engineering became an independent profession. This started the first great period of technological development, which may be called the technological age because of the dominant role

of technology in changing material civilization. The continued development of the physical sciences, which spread from physics to chemistry and then to thermodynamics and physical chemistry, mechanics, electricity, and finally to metallurgy, should also be noted. Due to this concatenation of developments in science and technology, it was inevitable that the methods of the former should be used for improving the latter. This occurred late in the nineteenth century. The end of this period would be the time when science assumed the leading role in technology, both in development and in practice. As already indicated, this did not happen abruptly; we are actually dealing with a period of transition during which empiricism gradually faded out and was replaced by the scientific method.

In my example of the electrical industry, the start came about 1900. Curiously enough, in metallurgy, a start can be detected at an earlier date if we accept the use of controlled experimentation by industry as our criterion. As examples, I would cite the work of Hadfield in England and Taylor and White, and Sauveur, in this country, although one could note the work of pioneers in other countries as well.* It was during the 1880's that Hadfield developed his silicon and manganese steels. The soundness of his work, and the reliability of the systematic, experimental method are attested to by the continued use of his compositions and treatments up to the present day. Sauveur correlated laboratory work on structure with steel characteristics and technical practice in another early example of the utility of controlled experimentation in industrial work. It was also by this method that Taylor and White developed the analyses and heat treatments that are still reflected in high-speed steel technology. The end of this transition period may be thought of as occurring about 1940.

This date is difficult to establish because it is not a unique point in history, like a proclamation, but a point of consensus. I mean that it was about this time that industrial research was accepted by the consensus of management. Just as a consensus of public opinion does not usually lend itself to yardstick measurement, except perhaps by polls, it is likewise uncertain just when this date should be set. I am judging largely by two criteria. In 1940, there were war clouds on the horizon and our government had a large procurement program under way. Not only did this call for development work but the government also sponsored research programs on a large scale. This activity, by bringing in new organizations and training new research workers, demonstrated the possibilities of the research method on a sufficiently large scale to have a nation-wide effect.

My second criterion comes from our experience at Battelle which, we think, indicated a swing in that direction at about the same time. In other words, research was in the air in industrial circles. Management seemed to change from the attitude of, "Should we spend company money on such a speculative venture as research?" to the modern approach, "What research program best suits my company's needs?" Industrial research had come of age.

This historical discussion will have brought out, I trust, the long apprenticeship served by technologists in their constant endeavor to improve their arts and crafts. From prehistoric times down to our own lifetime, the evolution was slow and labored; because it was wrought by the empirical method, it was relatively costly and inefficient. Ultimately, and I believe inevitably, the strong hand of management replaced empiricism with the tool of industrial research.

I have not cited statistics nor have I tried to describe the methods and philosophy of industrial research, for its mechanics and its place in industry and in society are well known. However, I do not believe it is beside the point to say that industrial research is a great national resource and one of the most significant factors in the strength of our country. Nor is it out of place to say that, in use, it has brought the prosperity to the nation and the material comforts to the citizens that were in the minds of those who financed it and participated in it. It feeds strength and versatility into the industrial machine and there come out more and better employment for the workers, better living conditions, more time for recreation, and, in brief, a better life for all of us.

The great and dynamic force of technology produces change, and the change has been synonymous with improvement. An object lesson of its beneficence is the daily worker who provides well for self and family by performing even simple tasks in production. In our country, through technology, a large and increasing population has been able, not just to subsist, but actually to improve its lot. Indeed, the great truth of technology is that it has made it possible for prosperity to be spread throughout the land. At no other time in history

* A classic example would be the early work of Chernoff on critical points and the heat treatment of guns.

could this be done, and in no other country has it been done. Because of this, technology should be understood in all walks of life in order to make sure that it remains healthy and in a position to insure our continuing prosperity.

Only when viewed in this light do we see the role of technology and its junior partner, industrial research, in its true perspective. Without technology and industrial research, neither legislation nor financing nor management nor labor could produce the changes and provide the comforts that we now enjoy. Indeed, whereas all the latter are vital components of industry they would be as impotent as our backwoodsman, were their functions not implemented by those two partners. To the scientists who give us facts and principles and to the research workers who translate them into industrial operations, we should be very generous in our recognition of what they give to society.

Dr. Burgess was in the midst of this great movement as contributor and manager. As Director of the National Bureau of Standards, he had a broad field for his activities. In his quiet but effective way, he did much to introduce the present scientific age, especially by means of the great powers that lie in the hands of management.

SCIENTIFIC MANPOWER—1915*

We must undoubtedly hold that if a larger supply of talent exists than is discovered, developed and put to use that, since, as we have seen, it is so valuable when estimated in terms of social progress, we are dealing wastefully with talent. We are allowing great ability to go to waste since we are leaving it lie in its undeveloped form. Therefore one of the problems of the proper conservation of talent consists in finding a method of discovering and releasing this valuable form of social energy. . . .

We shall be wise when we realize the worth of our workable talent and so establish its working conditions that it may secure the full measure of its productiveness. If scientific management for the mass of laborers of a nation is worth while how much more serviceable would it be to extend its fructifying influence to the most able members of the community.——The author (1866–1949) entered the field of sociology from the ministry. He received his Ph.D. at Chicago Theological Seminary and was a rural preacher in the Middle West. He later took his Ph.D. degree in sociology. In 1908 he established a department of sociology at the University of North Dakota and served as its head for forty years.

* From *The Conservation of Talent Through Utilization,* by John M. Gillette, The Scientific Monthly, 1, 151 (1915).

Warm-Bloodedness*

SIMON RODBARD

The author, Assistant Director of the Department of Cardiovascular Research of the Michael Reese Hospital in Chicago, received his Ph.D. in 1941 from the University of Chicago, and in 1951 he received his M.D. from the University of Illinois School of Medicine. During World War II he served as Aviation Physiologist in the Air Corps. While stationed in the mountains of Arizona, he developed an interest in historical geology. At present he is engaged in studies of hardening of the arteries, and in the application of hydrodynamics to the understanding of the structure, function, and diseases of the heart and the blood vessels.

ON a bitter wintry Chicago night, with the thermometer hovering at sixteen degrees below zero, an obscure woman, benumbed with alcohol and with cold, stumbled into an alleyway and fell into a deep sleep. While she slept she became the object of the intense interest of physicians and research workers who recorded her every heart beat and respiration. Each half-hour the radio broadcast reports of her condition, and newspapers all over the country and as far away as Japan headlined her story. When she regained consciousness twenty-four hours later she was already assured of distinction in medical annals, as she had recovered from one of the lowest body temperatures (65° F) ever recorded in a human being.[1,2]

During this period of low body temperature, her heart had slowed to fifteen beats per minute, her respirations were three per minute, and the blood flowed sluggishly through her arteries and veins. Her blood pressure could not be determined. The circulation through her arms, legs, and internal organs had virtually ceased, and the total output of her heart had gone to supply only her heart, lungs, and brain. As her body temperature began to rise under the watchful eyes of a corps of physicians, her heart rate and respirations increased, the circulation of blood accelerated, and the blood pressure climbed back to normal levels.

The keen interest of medical science was due to the fact that the case of the "Frozen Woman" offered lessons on the ability of the human body to function far below the 98.6° F considered to be normal. Military medicine was anxious to learn what it could from the case in order to be able to improve the treatment of freezing and frostbite in the field. The recovery of this subject from severe hypothermia, and the recent intensive experimental studies on induced hypothermia served to accelerate the utilization of "artificial hibernation" in cardiac surgery. Public interest was in the miraculous persistence of life and the recovery from such unbelievably low body temperatures.

Biologists, divorced from the restrictive anthropocentric view, were interested but not astonished. Some of these scientists viewed the case as an atavism revealing the long-forgotten function of hibernation which certain of the more primitive mammals still exercise. The long march of the warm-blooded animals to a higher degree of freedom from the thermal limitations of their environments has occurred as a result of the development of a large variety of physiological mechanisms that have masked the hibernating function The history of this escape from control by the environment is recorded in the temperature-regulating centers and reflexes originating in the central nervous system. The story is elicited in the analysis of the phylogenetic evolution of the vertebrates, and in the study of the poorly developed temperature regulation of the newborn.

In the course of the evolution of the higher animals and probably of the higher plants and the insects as well, there has been a progressive increase in the ability to withstand rapid environmental temperature changes. It is known that fish are relatively sensitive to slight variations in their environmental temperature, and that a rapid change of a few degrees may lead to serious injury or death.[3,4] However, the ability to adjust to rapid temperature change is not essential for the survival of fish living in large bodies of water, since such changes do not occur in their environment (Fig. 1). Their ability to move to the surface or descend to the cooler depths apparently is adequate to meet thermal stresses produced by warming of the surface

* From the Cardiovascular Department, Medical Research Institute, Michael Reese Hospital, Chicago, Ill. This department is supported in part by the Michael Reese Research Foundation.

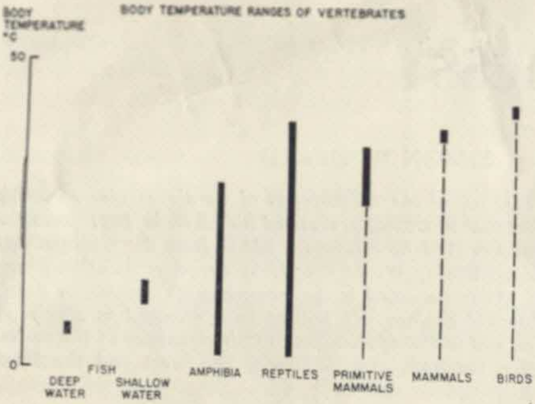


FIG. 1. A diagram to illustrate the progressive increase in temperature tolerance with the evolution of warm-bloodedness. The vertical bars represent the degree of tolerance to rapid temperature change. The dotted lines represent potential tolerance to thermal change that may be masked by development of temperature-regulating mechanisms. From SCIENCE, 108, 413 (1948).

during the day. Shallow-water fish may have to adjust to somewhat wider ranges in the course of a single day and these forms, including the common goldfish, are known to withstand a somewhat greater degree of temperature change without injury.[5,6]

The increasing ability to tolerate rapid environmental temperature changes must have been a factor in the development of land-living forms from the shallow-water crossopterygian fish of the Devonian times of 340 million years ago. When these fish left the water to live on the land as the earliest amphibians, they were exposed to more rapid and wider swings in body temperature than could ever have occurred under the cover of the high specific heat of the protecting water. Within the course of the following 100 million years, some of these amphibians developed the capacity to tolerate even greater temperature ranges; these were able to abandon the water completely, and gave rise to the reptiles.

It is well appreciated that variations in the environmental temperatures occurring during the course of the day in temperate climates play an important role in the cycles of activity of cold-blooded animals living on the land.[7-9] During the day, absorption of warmth from the sun raises the body temperature and increases the metabolic rate (Fig. 2). Increases occur in oxygen consumption, in respiratory and heart rates, in blood pressure,[10] in the velocity of the circulating blood, and in general activity. (A similar sequence of physiological events was seen during the rewarming of the "Frozen Woman.")

Cold-blooded animals may regulate their body temperature at optimal levels by moving from sunshine to shade, or by changing positions in order to expose greater or lesser surface area to the sun. These functions are commonly observed in lizards and turtles basking in the sun.[9] When optimal temperatures are attained, those functions having to do with defense, nutrition, and reproduction are consummated; this results in individual and species survival. With evening, these cold-blooded animals, without ability to retain heat, lose warmth to their environment. This leads to a slowing of metabolism, of circulation, and of general activity: the animal may fall into a sleep-like torpor. Each day this cycle of warming and activity, cooling and torpidity, is repeated (Fig. 2).

The advantages of a body temperature maintained at high levels, independent of changes in the environment, readily become apparent. Animals capable of remaining warm and active obviously can dine at the expense of their cold and torpid neighbors. Natural history records numerous more or less successful attempts to maintain a high body temperature. This phenomenon is seen in the communal hive-warming technique of the bees[11] and in the warming-up preflight process in insects[12] and bats.[13] It is possible that these and other species are even now evolving in the direction of better temperature regulation.

Among the fossils of the Permian period of 220

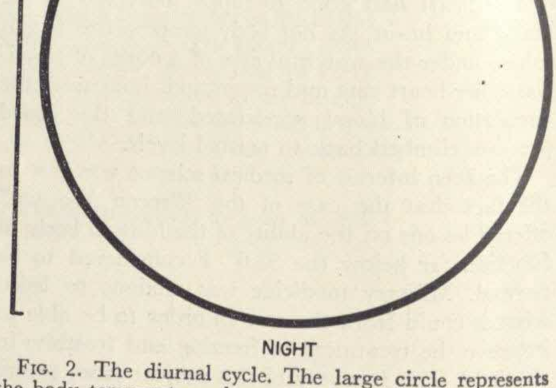


FIG. 2. The diurnal cycle. The large circle represents the body temperature changes during the day and night in cold-blooded animals living on the land. The small circle represents the diurnal cycle of warm-blooded animals. It mirrors the larger cycle but is limited to a narrow range.

FIG. 3. Permian scene, by the late Charles R. Knight. Two species of reptiles with elongated vertebral spines are shown. The carnivore, *Dimetrodon*, is represented by four of the large reptiles shown. *Edaphosaurus*, an unrelated herbivore, has apparently wandered unhappily into the scene at the left foreground. (Courtesy of the Chicago Natural History Museum.)

million years ago, several species of reptiles, closely related to the ancestors of the mammalian line, present evidence for a unique temperature-regulating mechanism. These reptiles had long vertebral spines extending two or three feet above the middle of the back, and forming the skeletal basis for a sail-like structure (Fig. 3). While no definitive proof can be presented for the function of these sails, one interpretation[14] is that the placing of the sail perpendicular to the rays of the sun in the early morning would provide a greater surface area for the absorption of heat, thus warming the animal and bringing him to an optimal working temperature ahead of his sail-less cousins. Evidence in support of this interpretation is seen in the extremely large grooves in the spines that may have contained blood vessels. Such a mechanism would be of particular value in a period of relative frigidity such as the Permian is believed to have been. That these species were dominant in the land for nearly 10 million years speaks eloquently for the excellence of adaptations to their environment that may have included such thermic adjustments. The disappearance of these species in the succeeding warmer Triassic period suggests that the "heat collecting" sails may have finally become a liability.

It is in the Triassic rocks formed 200 million years ago that the paleontological record first shows evidence of the evolution of the early mammals. A kind of passive warm-bloodedness may have already been achieved by the dinosaurs. Their great bulk and relatively small surface area may have operated to keep them warm during the cold night. During this period the theriodont reptiles, which probably already had developed an ability to withstand large diurnal temperature changes, gave rise to the early mammals. Thirty million years later, in the Jurassic, an unrelated group of reptiles gave rise to the ancestors of the modern birds.

The more recent evolution of the birds, with the establishment of a higher body temperature[15] than that of the mammals, has given our feathered friends certain advantages over us. Their higher metabolic rate provides them with a more rapidly responding nervous system than that of their plodding mammalian cousins. This advantage could have led to the displacement of the ruling mammals and the avian mastery of the Earth. However, the birds did not develop a cerebral cortex adequate for this task. Instead, they specialized in the development of the basal ganglia of the brain, structures concerned primarily with complicated locomotion. Thus it appears unlikely that the birds will assume control of the destiny of the Earth, except perhaps by radioactive default.

The evolution of hair or feathers probably played an important role in the ability to retain warmth during the evening and night. Evidence for such a nocturnal role for the early mammals has been given in comparative studies on the eye.[16] The development of other heat-conservation mechanisms, such as the ability to erect the hair and to shift blood from the skin, served to increase the thickness of the layer between the cold external world and the warmer internal structures (Fig. 4). The development of a thick subcutaneous fat pad also helped to retain heat. Mechanisms increasing the production of heat by shivering and perhaps by thyroid stimulation completed the process of making these animals relatively independent of their environmental temperatures.

Because of the high body temperatures we maintain throughout the twenty-four hour cycle, it might be considered that, Joshua-like, we have made the sun stand still so that we might live out our lives at the high noon which our cold-blooded relatives can enjoy for only a few hours each day (Fig. 2). This high and constant body temperature is important not only for ourselves but also for the parasites who have come to live with us. Thus, the malarial[17,18] and filarial[19] protozoans, the pinworms,[17] and the body lice[20] are highly attuned to slight diurnal changes in the temperature of our bodies. This is evident in their responses to these variations.

The increasing efficiency of the heat-conservation and heat-production mechanisms finally brought about a new situation: the danger of rais-

ing the body temperature to critical levels. At this time, mechanisms resulting in a loss of body heat began to have survival value. These included reflexes that shunted the warmer internal blood to the surface; sweating, panting, and such positional changes as might reduce the intake of heat and increase the surface area exposed to the environment. Similar time patterns in the development of heat-conservation and heat-loss mechanisms may be seen in newborn mammals and birds: a recapitulation of the evolutionary process.

Warm-blooded animals long have been known to have extraordinary thermostatic centers in the brain that carefully regulate the body temperature within a fraction of a degree. Recent studies have shown that these temperature-regulating mechanisms of warm-blooded animals reside in a part of the brain known as the hypothalamus.[21] This minuscule portion of the brain has been shown to be a basic center for the regulation of a large number of apparently unrelated functions. The hypothalamus has been considered to be the central organ for the control of the entire autonomic nervous system, which in turn controls many of the unconscious functions of the body such as heart

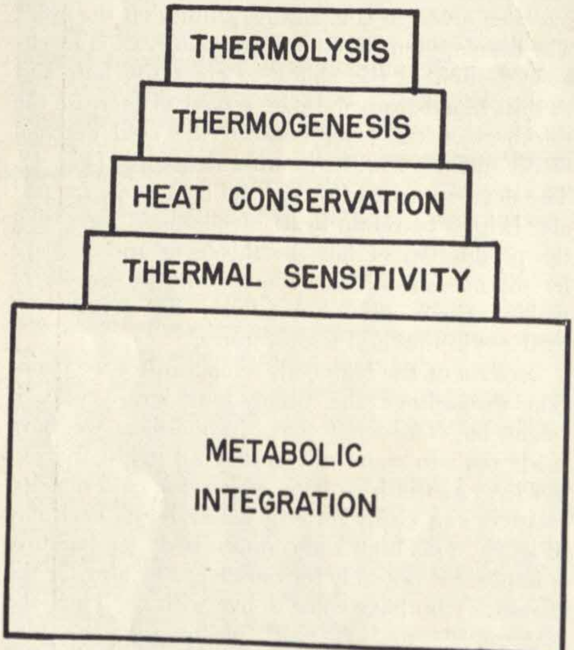


FIG. 4. Schema to illustrate the development of warm-bloodedness. The apparatus for Metabolic Integration of the organism provides the foundation for the development of Thermal Sensitivity. Utilizing this function, Heat Conservation apparatus develops. The later development of heat-producing apparatus (Thermogenesis) and heat-loss mechanisms (Thermolysis) completes the machinery for maintaining the normal body temperature of warm-blooded animals.

rate, digestive action, and glandular secretion. This part of the brain has also been implicated in the regulation of the blood pressure, respiration, blood sugar, fat, and water, and also related to the control of appetite and the diurnal rhythm of sleep and wakefulness.[21] Through control of the master gland, the pituitary, the hypothalamus is believed by some workers to control the sexual cycle.

The recent discovery that cold-blooded animals also have thermosensitive brain centers that affect the blood pressure and other functions[22,23] has provided a basis for a better understanding of the evolution of temperature regulation, and of the bodily changes that are brought about under the stimulus of alterations in body temperature.[24] In these experiments the brain of the turtle was subjected to thermal stimulation. A wire connected to a water reservoir was inserted in a selected site in the brain. When warm water was placed in the reservoir, the brain surrounding the wire was heated and the blood pressure was seen to rise within a minute or so. When cold water was used and the brain was cooled, the blood pressure fell within the same time period. Attempts to localize this temperature-sensitive site have shown the most sensitive region to be in the same general area of the brain that is concerned with the regulation of body temperature in the warm-blooded animals.[23] Prior to this, it had been generally accepted that, unlike the warm-blooded animals, cold-blooded animals are completely at the mercy of their environmental temperatures because of the lack of adjustment mechanisms. However, the evidence that cold-blooded animals may make certain adjustments in their internal functions in response to stresses placed by the environmental temperature, shows that the beginnings of thermal independence are already present in these more primitive animals.

On the basis of the evolutionary approach discussed above, the apparently variegated and unrelated functions of the hypothalamus may be considered as parts of an integrative mechanism playing a role in the regulation of the internal environment of the body in response to changes in body temperature. It is apparent that as the amphibian leaves the water and is exposed to large changes in body temperature, enormous variations probably occur in the metabolic rates of the various tissue and organ systems. Unless these changes are properly integrated, they might easily produce maladjustments in the internal economy of the animal. An internal environment suitable for temperatures near freezing might be wholly unsuitable

for body temperatures approaching 100° F. This phenomenon is emphasized in the marked differences in response to drugs at various body temperatures.[25] Since temperatures of such magnitude might be impressed within the course of a few hours on land-living cold-blooded animals, the development of a coordination center would provide survival value for its possessor. In cold-blooded animals, such a center, without capacity to regulate the body temperature, apparently operates to adjust some elements of the internal environment in coordination with the changes induced by the external environment. A rapidly reacting center of this kind appears to have developed in the hypothalamus of the cold-blooded animals, and the temperature-regulating apparatus was later laid down in close proximity to it.

The development of heat-conservation, heat-production, and heat-loss mechanisms introduced a new factor—the relative constancy of the body temperature. The actual body temperature levels of a given animal group appear to be related to body size (Fig. 5).[15] These relatively constant conditions served to mask the basic integrative equipment, and the internal adjustments become minimal during the diurnal cycle.

A number of patterns of behavior of warm-blooded animals suggest the retention of some of the elements of the daily temperature cycle of the cold-blooded animals. These include the diurnal variations in body temperature, blood pressure, heart rate, fluid and blood shifts, and the period of torpor (sleep).[26]

The phenomenon of hibernation in warm-blooded animals may be considered a remnant of the response to seasonal temperature changes, and in this way similar to hibernation in cold-blooded animals.[24] Several functions such as the reproductive cycle, which previously depended in part on the daily variations in temperature and their annual recurrence, as a result of the constant body temperature have escaped from the solar cycle and established a reproductive pattern independent of the sun. For example, in the female the estrus cycle is closely tied to body temperature variations which reflect the annual cycle. Prior to ovulation the body temperature is low; it then rises and remains high during the proliferative and/or the pregnant period.[27, 28] Curiously, certain species of plants will not flower unless the plant experiences a rapid drop in temperature.[29]

A most interesting problem of the constant high body temperatures of the warm-blooded animals is that of the peculiar association of the sexual cycle with body temperature and the thermal vul-

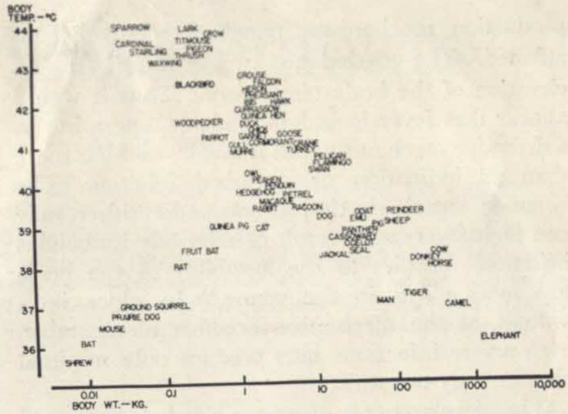


FIG. 5. The resting body temperatures of mammals and birds plotted against an average body weight. From SCIENCE, **111**, 465 (1950), with correction.

nerability of the spermatozoa. In many animals with high constant body temperatures the testicles are protected by their expulsion from the body proper into the air-cooled scrotum.[30] In birds, Cowles has suggested that the migration of the testicle upward, to a close approximation with the relatively cool air sacs of the lungs, represents a similar mechanism.[31] The diurnal cycle operates to permit spermatogenesis in some birds by means of the nocturnal drop in body temperature.[30]

Fever has commonly been interpreted as a mechanism aiding the body in overcoming some infective agent. However, only a few microbes such as the spirochete and the gonococcus are known to be injured during fevers. On the other hand, fevers occur in many conditions in which they confer no known advantage. It is well known that a rise in body temperature of only a few degrees may seriously disturb the normal function of the body[32] and thus weaken the ability to resist infection. In fact, an uncontrolled rise in body temperature may in itself lead to exhaustion and to death.

The great neurologist, Hughlings Jackson, long ago pointed out that the more recently developed a function of the nervous system may be, the more susceptible it is to injury.[33] For example, the slightest toxicity, such as that which results from the lack of oxygen, or from small quantities of alcohol, may seriously disturb one of our most recently developed functions, that of judgment. By the same token, if the heat-loss mechanism is of recent development (Fig. 4), it would be expected that it would be most susceptible to injury. Invasion of toxic materials into the blood stream would thus disturb the new and sensitive heat-loss mechanisms, while the older heat-conservation and heat-

production mechanisms remained relatively unaffected. The dysbalance would result in an elevation of the body temperature. Thus it would appear that fever is seldom laudable, operating as a defensive mechanism, but is usually nothing more than an indication of disturbed function. The common experience that infants and children may run high fevers as a result of relatively innocuous infections, testifies to the instability of the heat-loss mechanisms in the young.[34] In older individuals, as the mechanism becomes more stable, even severe infections may produce only minimal rises in body temperature.

The development of warm-bloodedness could not have taken place without the provision of an adequate supply of fuel and oxygen to maintain the fires of the body. The logistic problem was solved in the course of the long and involved evolution of the heart from the relatively simple pump of the fish, capable of developing only low pressures, to the complicated "double" heart capable of generating the much higher blood pressures required by the warm-blooded animals.[35] With these higher pressures, greater rates of blood flow can be obtained to supply the rapidly metabolizing tissues. These higher pressures also made it possible for animals to raise their heads skyward for the first time in history, since the pressure was now high enough to pump the blood upward against gravity. However, it also became possible for the pressure to be raised to such high levels that it might have deleterious effects on the blood vessels and even upon the heart itself.

The ancients tell us that Zeus was infuriated by the theft of the heavenly fire by Prometheus who gave it to man. To vent his spleen, Zeus fashioned the all-endowed Pandora, gave her a jar filled with evils, and sent her as a gift to Prometheus' brother, Epimetheus. She was accepted despite the dire warnings of Prometheus about gifts from the Greek gods. The opening of the jar and the escape of its contents was the inevitable outcome.[36] We may look in similar vein upon the mechanisms for adjusting to the Promethean fire of warm-bloodedness which has brought on us a Pandora's vessel containing high blood pressure, hardening of the arteries, and heart failure, but also Hope.

References

1. Reincke, J. J. *Deut. Arch. klin. Med.*, **16**, 12 (1875).
2. Laufman, H. *J. Am. Med. Assoc.*, **147**, 1201 (1951).
3. Britton, S. W. *Am. J. Physiol.*, **67**, 411 (1924).
4. Roule, L. *Fishes, Their Journeys and Migrations.* New York: Norton (1933).
5. Eddy, S., and Surber, T. *Northern Fishes, With Special Reference to the Upper Mississippi Valley.* Minneapolis: University of Minnesota Press, 2nd ed. (1947).
6. Pincher, C. *A. Study of Fish.* New York: Duell, Sloan and Pearce (1948).
7. Ditmars, R. L. *Reptiles of the World.* New York: Macmillan (1936).
8. Cowles, R. B., and Bogert, C. M. *Bull. Am. Museum Nat. Hist.*, **83**, 265 (1944).
9. Smith, H. M. *Handbook of Lizards of the United States and Canada.* Ithaca, New York: Comstock (1946).
10. Rodbard, S., and Feldman, D. *Proc. Soc. Exptl. Biol. Med.*, **63**, 43 (1946).
11. Park, O. W. In R. A. Grout, *The Hive and the Honeybee.* Hamilton, Ill.: Dadant and Sons (1946).
12. Krogh, A., and Zeuthen, E. *J. Exptl. Biol.*, **18**, 1 (1941).
13. Allen, G. M. *Bats.* Cambridge, Mass.: Harvard University Press (1939).
14. Rodbard, S. *Copeia*, **3**, 224 (1949).
15. ———. *Science*, **111**, 465 (1950).
16. Walls, G. L. *The Vertebrate Eye and Its Adaptive Radiation.* Bloomfield Hills, Michigan: Cranbrook Institute of Science (1942).
17. Manson-Bahr, P. H. *Manson's Tropical Diseases.* Baltimore: Williams and Wilkins (1950).
18. Gradwohl, R. B. H., Soto, L. B., and Felsenfeld, O. *Clinical Tropical Medicine.* St. Louis: Mosby (1951).
19. Napier, L. E. *Medicine*, **23**, 149 (1944).
20. Buxton, P. A. *The Louse.* London: E. Arnold (1947).
21. Zimmerman, H. M. The Hypothalamus. *Res. Publ. Assoc. Nerv. Ment. Dis.*, **20**, 824 (1940).
22. Rodbard, S., *et al. Am. J. Physiol.*, **158**, 135 (1949).
23. Rodbard, S., Samson, F., and Ferguson, D. *Ibid.* **160**, 402 (1950).
24. Rodbard, S. *Science*, **108**, 413 (1948).
25. Fuhrman, F. A. *Physiol. Rev.*, **26**, 247 (1946).
26. Kleitman, N. *Sleep and Wakefulness.* Chicago: University of Chicago Press (1939).
27. Bergman, P. *Acta Obstet. Gynecol. Scand.*, **29**, Sup. 4 (1950).
28. Bullough, W. S. *Vertebrate Sexual Cycles.* New York: Wiley (1951).
29. Daubenmire, R. F. *Plants and Environment.* New York: Wiley (1947).
30. Moore, C. R. In: *Sex and Internal Secretions.* Chap. VII. Baltimore: Williams and Wilkins (1939).
31. Cowles, R. B., and Nordstrom, A. *Science*, **104**, 586 (1946).
32. Neymann, C. A. *Artificial Fever.* Baltimore: Thomas (1938).
33. Taylor, J. *Selected Writings of John Hughlings Jackson.* London: Hodder and Stonghton, 2 Vols. (1931).
34. Nelson, W. E. *Textbook of Pediatrics.* Philadelphia: Saunders (1945).
35. Rodbard, S., Brown, F., and Katz, L. N. *Am. Heart J.*, **38**, 863 (1949).
36. Hesiod. *Days and Works; Theogony.* Transl. by H. G. Evelyn-White. Cambridge: Harvard University Press (1936).

Sets of Three Measurements

W. J. YOUDEN

The author received his Chemical Engineering degree from the University of Rochester, his Ph.D. in Chemistry from Columbia University, and studied at Galton Laboratory, University College, London. For twenty-four years he was a chemist at Boyce Thompson Institute for Plant Research. After three years service overseas as operations analyst with the Army Air Forces, he received the Medal of Freedom. Now a consultant at the National Bureau of Standards, his recent work has been in the design and interpretation of experiments.

SINGLE measurements have meaning only when they are made against a background of familiarity with the measurement procedure and the object that is measured. Without this background the numerical value obtained by making a single measurement is inherently misleading. The numerals set down to represent the measurement convey a sense of exactness to the unwary. Unless there exists some prior information about the agreement that might be expected between this measurement and a second measurement, the zone of uncertainty is so large that the first measurement, by itself, can hardly be said to convey any quantitative information. Certainly, no experienced person would be willing to hazard a guess as to how close such a single measurement represents the phenomenon or class of measurements of which this measurement is a single representative.

A second, truly independent, measurement does a good deal more than fortify the first measurement by making it possible to use the average of both measurements. The *difference* between the two measurements provides the information that is indispensable if limits about the average are to be designated that may, with some stated degree of certainty, include the unknown value for the class. The average of the two measurements is an estimate of the class value. Notice that the difference between the two measurements is a single estimate of the average difference. This estimate of the difference could be improved if more measurements were available for examination. The limits that are laid off above and below the average are very sensitive to the number of measurements available for estimating the errors in the measurements. Working at the 95 per cent level of certainty, the limits, if five measurements are available, are only 14 per cent as wide as those that must be set if only a single pair is taken. The measurements are assumed to be normally distributed.

The largest part of this reduction in the spacing of the limits comes, not through the greater stability of an average of five over an average of two, but by virtue of the much better information about the dispersion or errors in the measurements. The 95 per cent limits set around the average of two measurements when the dispersion is known exactly are only 15 per cent as wide as those that must be set if the single difference between the two measurements constitutes the sole information on the dispersion. In other words, an average based on two measurements, when the dispersion is known, is about as useful as an average of five measurements when these five measurements must also supply the information on the dispersion. To anticipate the objection that the dispersion is never known exactly, it is sufficient to remark that the above percentage is slightly increased to 16 if the "known" dispersion is based on as many as three score measurements.

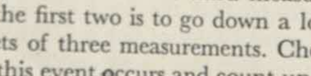
The preceding paragraphs stress the importance of knowledge regarding the errors in measurements. An experimental worker is naturally interested chiefly in the average, because this is the magnitude sought in connection with the scientific problem under study. The errors in the measurements are inevitably regarded as a nuisance. Every experimenter makes strenuous efforts to reduce experimental errors so that the zone of uncertainty is small. The point being made here is that the zone of uncertainty may also be reduced if attention is given to obtaining a good estimate of the errors.

Because experimenters have for so long been successful in reducing the errors in their measurements to the point that they could almost be ignored, they have tended to give very little study to the behavior of the errors of measurements. Extraordinary as it may seem, experimenters taking measurements every day are unaware of relatively simple properties of the sets of data they enter in their notebooks. Perhaps it is unnecessary to know much about the behavior of measurements in order to interpret the experimental results. More likely,

by reason of long experience, workers develop some sort of unstated working rules that are used more or less automatically in forming judgments. Nevertheless, the properties of measurements are themselves quite interesting.

Many believe that it is necessary to have rather large sets of measurements in order to make use of statistical techniques. This is not at all the case, as was shown in the opening remarks regarding pairs of measurements. Given a sequence of pairs, or triads of measurements, a great deal can be learned about the behavior of measurements by simple empirical procedures. It is not necessary that all the pairs or triads refer to the same object. The objects measured may be different. The only requirement is that the measurement procedure have the same errors of measurement over all of the sets. It is not uncommon to have as many as three measurements on the same object. Any one set tells very little; but if many sets are available, certain patterns of behavior are easy to establish by examination. The patterns may also be predicted by mathematical procedures. The study of these patterns is a major activity in mathematical statistics.

A simple question will illustrate what is meant by the phrase "the behavior of measurements." Consider a set of three measurements. It is hardly likely that these can be obtained simultaneously. They are, at least, put down in some time sequence. After the first two are entered, the experimenter may be curious about how often the third measurement falls between the two already entered. The question does not apply to any one set. The question is: In general, over all sets of three measurements, how often is the third measurement intermediate between the first and second? This is a good question because no assumption needs to be made that the measurements are normally distributed. One empirical way to find the proportion of triads in which the third measurement lies between the first two is to go down a long list, say 100, of sets of three measurements. Check the sets in which this event occurs and count up the checks. This is an answer, but certainly not an exact answer. Another set of 100 triads would very likely give a different count. The correct proportion can be approached as closely as desired by sufficiently increasing the number of sets examined. It is easier, this time, to use a simple bit of logic to go directly to the exact proportion. Indicate on a scale the positions of three measurements A, B, and C.



Obviously, B does lie between A and C. Now, these three measurements might have been obtained in any order. There are only six orders in which the measurements can be arranged.

ORDER	1	2	3
	A	B	C*
	A	C	B
	B	A	C
	B	C	A
	C	A	B
	C	B	A*

The asterisks show the two, out of the six equally likely orders, that bring B between A and C. And so the answer is that one-third of the sets of measurements will be such that the third measurements falls between those already in hand. Two times out of three, on the average, the third measurement will be smaller than both or larger than both the first pair. This proportion is a property of data; probably not a very useful property. It cannot be changed by anything the experimenter can do. But an easy generalization may be of interest. Suppose nine measurements have been made. Inevitably these are scattered over a range of values. Most experimenters would rather expect a tenth measurement, if made, to fall somewhere in the range of values already encountered. There is a formula to find how often this will occur. If $(n-1)$ measurements are followed by an nth measurement, the chance that this measurement falls between the smallest and largest of the $(n-1)$ measurements is $(n-2)/n$. Thus, once out of five times, the tenth measurement will be either smaller or larger than the other nine. Many might suspect a tenth measurement that fell outside the range of nine measurements. Once in five times is hardly a rare event and it would be unwise to suspect the tenth measurement on this ground.

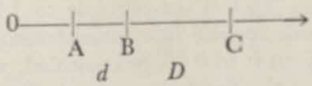
It is customary in school courses in quantitative chemical analysis to require students to turn in duplicate analytical results. The grade for the analytical work depends in part on how well the average of the two determinations agrees with the value ascribed to the material, and in part on how well the two determinations agree with each other. It is rumored that students have discovered that it takes practically no longer to run three determinations than it does to run two. The student looks at the three results and turns in the pair showing the best agreement. If this practice exists there should be some interest in the consequences. First, the average agreement between such a selected pair is very much better than the average difference found between two unselected measurements. The difference between the selected measurements averages four-tenths $(3-3\sqrt{3}/2)$ that of the differ-

ence for honest duplicates.[1] Consequently, this practice would lead a chemistry professor to form an altogether optimistic picture of the precision he can properly expect from students. It would be tough going for a student who just ran duplicates.

There is, however, something more to be said about this selection of the best two out of three. How about the behavior of the average of such a selected pair? The dispersion of this selected average is greater than the dispersion shown by the average of duplicates. In other words, the student may lose on that part of his grade. The operation makes the average behave as though the measurement errors were about 12 per cent larger than they actually are.

There are certainly many individuals who have been tempted to save the best two out of three measurements. Until very recently, no one, experimenter or statistician, had thought to explore the consequences of such selection. Most people are very much surprised to find how pronounced an effect there is on the apparent agreement of two measurements if they are selected from three. Misinformation on the real dispersion of measurements can be as misleading as tampering with the actual values.

The temptation to discard one of three measurements comes from the frequent occurrence of sets in which two measurements are in close agreement and the remaining measurement is considerably removed from the pair. Intuitively, one might argue that perhaps some blunder was made on this far removed value. Unless there is available a realistic picture of the ways three measurements may distribute themselves, when there is nothing whatsoever wrong with any of the measurements, any judgment as to whether or not a blunder has been committed is almost certain to err in discarding good values. Again, mark the measurements off on a linear scale, and denote the distances between the measurements by d and D. It has been shown that



the interval D is ten or more times as large as d in 15.7 per cent of sets of three measurements. Rather more than one-third of the time, D is at least four times as large as d. Evidently, very uneven spacing of the measurements is a commonplace. Values of the ratio D/d exceed 32.57 once in twenty times.[2] If other knowledge about the dispersion is lacking, there is scant justification for discarding the remote value unless it is removed from the closest pair by an amount some thirty times the difference between the measurements constituting the closest pair.

Given three measurements, there is always a smallest one. Ordinarily, in order to find out which is the smallest measurement, it is necessary to have all three available. There are circumstances that reveal the smallest measurement directly. There is a tensile test for steel alloys used in turbo-engines that specifies that test specimens be maintained at a constant temperature in the neighborhood of operating temperatures and subjected to a constant load. The time to rupture is recorded. Often this test extends over weeks. Imagine that three test specimens are mounted in tandem and the load attached to the bottom specimen. Eventually, one of the specimens yields—the one with the smallest strength. It is not necessary to continue the test to determine the times for the remaining two specimens. Now, suppose that this test on three specimens is repeated over and over and the results for all the least of threes examined. They will be found to have much less dispersion among themselves than the results of tests with single specimens. Indeed, the dispersion of these smallest values is about that shown by the averages of two single tests. Apart from the bias below the average, each least of three is nearly equal to the average of two ordinary measurements as far as dispersion is concerned. The bias can be allowed for if the dispersion is known or estimated from a reasonable amount of data. Besides the diminished dispersion, there is the further advantage that the average time for the least of three is shorter than the time for a single specimen (by the amount of the bias), and thus more experiments may be run. The whole subject of extreme values has received much attention in the past few years.

Sets of three measurements sometimes arise even when the usual schedule calls for taking duplicates. Frequently the chief function of the second measurement is to reassure the experimenter that no gross blunder was made on the first measurement. Assuming that a familiar measuring procedure is being used, the experimenter will have in mind the average agreement between duplicates for this procedure. He will need to set up some operating rule to warn him that one of the measurements may be subject to an error beyond that usually associated with his procedure. It is known that five per cent of the pairs will, in the normal course of events, turn up with a difference between the measurements that is 2.45 times as large as the expected difference between members of a pair.[3] It may seem desirable to take some action in such cases lest this large difference really arise from some undetected

slip. This rule will catch slips at an insurance premium of five per cent of the work needlessly held for further examination.

There is no way, merely by looking at widely separated results, to tell which one of the two measurements is really at fault, if a slip has been committed. It would be a tremendous boon to scientists if, simply by looking at the results, they could always pick that measurement which is actually nearer to the true average of the class of measurements to which these two belong. If this measurement could be singled out, it would be best to throw away the other measurement because this chosen measurement has just about the dispersion that averages of three measurements show!* Lacking any convenient crystal ball, the experimenter must have recourse to taking at least one more measurement. In this way a set of three measurements is generated and the experimenter looks at these to decide what to do next. Apparently there is no generally accepted set of rules to guide the experimenter in this situation. Fortunately, if a serious blunder has been made on one of the original pair, the third measurement usually pairs closely with one of the first two and the remaining measurement is clearly so far away that it can be discarded without hesitation.

The experimenter is still left with about five per cent of his sets which appear not to have a serious blunder. Shall he continue to use the rule of selecting the closest pair? Notice that this is not the same closest pair discussed earlier. It is the closest pair subject to the requirement that the *third* measurement be one of the pair. Examination of these sets of three will reveal that, for about half of them, the over-all range for all three measurements is not unreasonable for sets of three. Ninety-five per cent of sets of three have an over-all range not exceeding 2.96 times the average difference for a pair. Sets that meet this requirement may be considered to have been freed from suspicion and the average of all three results may be taken at best representing the quantity sought. The remaining sets of three have a suspiciously large range, and it appears reasonable to discard that one of the first two which is chiefly responsible for the large range. It is assumed that the third measurement turns out not to differ too much from the closer one of the first pair. Experimenters will be inclined, therefore, to take the average of the third measurement and the one closer to it. There is a surprising property about the averages of these pairs. These averages appear to have just about the same dispersion as the single third measurement. Exact statistical theory is not yet available on this point. The experimenter may either accept the third measurement (discarding the first pair) or take the average of the third and the closer one to it. The latter procedure is certainly the better choice when a blunder has been made; so it would appear that it is simpler always to take the average of the closest pair, one of the pair being the third measurement.

Just recently, a statistical solution has been found for one more problem involving sets of three.[4] The problem is particularly interesting because the set of three, instead of referring to three measurements on one object, has to do with measurements on three different objects. The purpose here is to be able to pick out, with any desired frequency of success, the object having the largest value of the property being measured. A little reflection suggests that the frequency of success will depend upon how much larger than the other two the largest object really is. Success will also depend upon the dispersion of the measurements and the number of measurements made on each object. For simplicity, assume that just one measurement is made on each object. What are the conditions so that, 95 per cent of the time, the largest measurement recorded will actually come from the largest object? The largest object must in fact differ from the nearer of the smaller two by an amount equal to 2.40 times the average difference betwen two measurements on the same object. (The dispersion of the measurements is assumed to be known.) For the first time, a firm guide exists to a long prevailing problem of picking with high assurance the best catalyst, the best tire, or any other entity. A simple rule leads directly to the number of measurements that must be made on *each* of the three objects, if it is seen in advance that it is desirable to succeed, even if the difference important to detect is less than the above multiple of 2.40 times the average difference for pairs of measurements on the same object. To cut the multiple to 1.20, that is, one-half, will require four measurements on each object. To cut it to 0.80, or one-third, will require nine. To cut the factor to unity, or the same magnitude as the average difference, takes six measurements on each.

Perhaps the more important point about the above problem is that the experimenter can, and must, do his statistical thinking in advance of approaching the three objects with his measuring equipment. In effect, he has to decide what magnitude of difference he would regret failing to detect, and he must know his equipment and the rule.

* My colleague, E. P. King, derived for me the variance of the less erroneous one of two measurements. It is $1 - 2/\pi$.

He knows then that, *if* one of the objects differs from the others by at least this amount, his largest measurement (or average) will point to it at least 95 per cent of the time. This is not at all to say, after the measurements have been taken, that one is 95 per cent sure that the largest measurement came from the largest object. Obviously if the objects are all equal, or very closely so, any object has about equal chance (one-third) of yielding the largest measurement. The experimenter must remember that he has already decided that, if the objects are that close, he does not care which one he picks. He does know that he is reasonably sure that he has picked the largest object *if* it differs appreciably from the others.

References

1. LIEBLEIN, J. *J. Research Natl. Bur. Standards,* **48,** 255 (1952).
2. The fallacy of the best two out of three. *Natl. Bur. Standards (U. S.) Tech. News Bull.,* **33,** 77 (1949).
3. PEARSON, E. S. *Biometrika,* **24,** 404 (1932).
4. BECHHOFER, R. E. *Ann. of Math. Stat.* In press.

SCIENTIFIC RESEARCH—1915*

As members of the body politic, we can assist the development of science in two ways. *Firstly,* by doing each our individual part towards ensuring that endowment for the university must provide not only for "teaching adolescents the rudiments of Greek and Latin" and erecting imposing buildings, but also for the furtherance of scientific research. The public readily appreciates a great educational mill for the manufacture of mediocre learning, and it always appreciates a showy building, but it is slow to realize that that which urgently and at all times needs endowment is experimental research.

Secondly, it is vital that public sentiment should be educated to the point of providing the legal machinery whereby some proportion, no matter how small, of the wealth which science pours into the lap of the community, shall return automatically to the support and expansion of scientific research. The collection of a tax upon the profits accruing from inventions (which are all ultimately if indirectly results of scientific advances) and the devotion of the proceeds from this tax to the furtherance of research would not only be a policy of wisdom in the most material sense, but it would also be a policy of bare justice.——The author, born in Edinburgh, attended the University of Adelaide and from 1905 to 1916 he was a member of the faculty of the University of California. In 1920 he became Professor of Biochemistry and General Physiology at the University of Adelaide. He was the author of numerous technical articles and books.

* From *The Cash Value of Scientific Research,* by T. Brailsford Robertson, THE SCIENTIFIC MONTHLY, 1, 140 (1915).

Time-Binding and the Concept of Culture*

M. F. ASHLEY MONTAGU

The author is chairman of the Department of Anthropology, Rutgers University, the State University of New Jersey. Born in London, England, he received his Ph.D. in anthropology from Columbia University in 1937. He was on the faculties of New York University and the Hahnemann Medical College before becoming professor of anthropology at Rutgers in 1949. Dr. Montagu is the author of numerous articles and books on anthropology and allied subjects. For many years he has been interested in the borderlands at which the biological and social sciences meet.

IN June, 1921 an important book was published by an unknown author. The book was entitled *Manhood of Humanity,* and its author was Alfred Korzybski. By December, 1921 it had gone into its third printing, and in December, 1923 there was a fourth printing. Obviously, quite a number of people had found the book of interest, but who they were I cannot tell. Certainly, it seems, the book did not make much rumor in the world or leave too deep an impression. I read this book for the first time in 1950 in its second posthumously published edition of June, 1950. It was only then that I learned that Korzybski's theory of time-binding had been developed long before the appearance of *Science and Sanity* in 1933. It is to be hoped that with the republication of *Manhood of Humanity* the book will, at last, come into its own, for it is perhaps more timely for our own day than it appears to have been for the time during which it made its first appearance. For in *Manhood of Humanity* Korzybski attempted to lay the foundations for the science and art of what he then called by the new term Human Engineering—a term which has since been adopted by others, but not quite in the sense in which he meant it. Such a science, Korzybski pointed out, must be based on a true understanding of the nature of man, upon what is essentially characteristic of him; and he endeavored to show that what distinguishes man from all other living things is his capacity for time-binding.

Before proceeding to the discussion of this brilliant conception I must say a few words about the dangers of making one's appearance too early. Intellectually, Korzybski was a prematurely born child. The views set out in *Manhood of Humanity* were from 25 to 30 years too early. While in 1921 Korzybski was occupied with science and its relation to ethics, with science and its relation to values, his contemporaries in the sciences and in philosophy were denying that science or philosophy could provide a sound scientific foundation for leading the good life. We find Bertrand Russell, for example, writing, and Korzybski quotes this very passage, "The hope of satisfaction to our more human desires, the hope of demonstrating that the world has this or that ethical characteristic, is not one which, so far as I can see, philosophy can do anything whatever to satisfy." With this viewpoint Korzybski was entirely unable to agree, and I think, if we are to judge from his latest writings, Russell would today be more inclined to agree with Korzybski's 1921 judgment than he would with his own of earlier years.[1] Said Korzybski, in 1921, "The scientists, all of them, have their duties no doubt, but they do not fully use their education if they do not try to broaden their sense of responsibility toward all mankind instead of closing themselves up in a narrow specialization where they find their pleasure. Neither engineers nor other scientific men have any right to prefer their own personal peace to the happiness of mankind; their place and their duty are in the front line of struggling humanity, not in the unperturbed ranks of those who keep themselves aloof from life. If they are indifferent, or discouraged because they feel or think that they know that the situation is hopeless, it may be proved that undue pessimism is as dangerous a 'religion' as any other blind creed."[2]

This was a view which was not popular when

* Based on a paper presented at the first annual series of the Alfred Korzybski Memorial Lectures held at the Waldorf-Astoria Hotel, New York, on March 5, 1952, under the auspices of the Institute of General Semantics, Lakeville, Connecticut.

Korzybski wrote. Scientists were inclined to an ivory-towerism and a hand-washing indifference to the consequences of their work. Thirty years later scientists are quite generally agreed that they must take a more responsible view of their place in the world. Korzybski's warning has come home to roost, "If those who know why and how neglect to act," he wrote in 1921, "those who do not know will act, and the world will continue to flounder."[3]

Korzybski was among the first to point out that "it is the great *disparity* between the rapid progress of the natural and technological sciences on the one hand and the slow progress of the metaphysical, so-called social 'sciences' on the other hand, that sooner or later so disturbs the equilibrium of human affairs as to result periodically in those social cataclysms which we call insurrections, revolutions and wars."[4] In the history of the development of the consciousness of this fact Korzybski's name will come to occupy an honored place. When a society places its emphases on technology it produces a nation of technicians. Korzybski saw quite clearly where the emphasis must be placed if mankind is to survive—namely, upon the understanding of man's nature and the development of human relations on the basis of that understanding. The good life was what Korzybski was passionately interested in, and so he said, "Ethics is too fundamentally important a factor in civilization to depend upon a theological or a legal excuse; ethics must conform to the *natural* laws of human *nature*."[5] Korzybski clearly stated the consequences of "A system of social and economic order built exclusively on selfishness, greed, 'survival of the fittest,' and ruthless competition,"[6] and prescribed the remedy. The period of the *childhood* of humanity was one of arbitrary thought and confusion. "The period of humanity's *manhood* will," Korzybski wrote, "I doubt not, be a scientific period—a period that will witness the gradual extension of scientific method to all the interests of mankind—a period in which man will discover the essential nature of man and establish, at length, the science and art of directing human energies and human capacities to the advancement of human weal in accordance with the laws of human nature."[7]

Korzybski died on March 1, 1950. Happily, he lived to see his best hopes for the application of the scientific method to some of the interests of mankind get started, even though fitfully. The dangers of being born too early are largely personal. One suffers the pains of inappreciation for being ahead of one's time, and when one is caught up with by the rest of mankind one has to have sense of humor enough to bear with equanimity the comment that after all there was nothing new in one's ideas, or that they were known all the time. Those who knew him can testify to the fact that Korzybski had a delightful sense of humor, so that neither the barking of the dogs of St. Ernulphus nor the patronizing gestures of the world of pernicious academia too much disturbed him.

In the present essay I wish to consider one of Korzybski's most significant contributions toward the better understanding of the nature of man, namely, his conception of time-binding. Korzybski distinguished between the classes of life in the following manner: Since plants capture one kind of energy, convert it into another, and store it up, he defined the plant class of life as the chemistry-binding class of life. Since animals are characterized by the freedom and faculty to move about in space he defined animals as the space-binding class of life.

"And now what shall we say of *human* beings?" asks Korzybski, "What is to be our definition of Man? Like the animals, human beings do indeed possess the *space-binding* capacity but, over and above that, human beings possess a most remarkable capacity which is entirely peculiar to them—I mean the capacity to summarize, digest and appropriate the labors and experiences of the past; I mean the capacity to use the fruits of past labors and experiences as intellectual or spiritual capital for the developments in the present; I mean the capacity to employ as instruments of increasing power the accumulated achievements of the all-precious lives of the past generations spent in trial and error, trial and success; I mean the capacity of human beings to conduct their lives in the ever increasing light of inherited wisdom; I mean the capacity in virtue of which man is at once the heritor of the by-gone ages and the trustee of posterity. And because humanity is just this magnificent natural agency by which the past lives in the present and the present for the future, I define HUMANITY, in the universal tongue of mathematics and mechanics, to be the TIME-BINDING CLASS OF LIFE."[8]

Very properly, Korzybski emphasized the importance of grasping the meaning of this definition of humanity as a starting-point for discovering the natural laws of human nature—of the human class of life. Korzybski pointed out the immeasurable evils which have resulted from regarding man as a mere space-binder, an animal. In other words, Korzybski was from the first aware of the dangers of "biologism" or what I have elsewhere called "the pathetic fallacy," the belief that man is nothing

but a function of his genes—a fallacy which many contemporary thinkers have even yet not succeeded in avoiding. Korzybski recognized that man was characterized by "properties of higher dimensionality"; that everything which is really time-binding is in the human dimension.

Now, Korzybski's definition of humanity, of man, is virtually identical with the present-day anthropologist's definition of culture. Here are some definitions of culture taken from the latest anthropological works by leading anthropologists. Firth writes, "If society is taken to be an aggregate of social relations, then culture is the content of those relations. Society emphasizes the human component, the aggregate of people and the relations between them. Culture emphasizes the component of accumulated resources, immaterial as well as material, which the people inherit, employ, transmute, add to, and transmit. Having substance, if in part only ideational, this component acts as a regulator to action. From the behavioral aspect, culture is all learned behavior which has been socially acquired. It includes the residual effects of social action. It is necessarily also an incentive to action."[9]

Kluckhohn writes, "By 'culture' anthropology means the total life way of a people, the social legacy the individual acquires from his group. Or culture can be regarded as that part of the environment that is the creation of man. . . . The general abstract notion serves to remind us that we cannot explain acts solely in terms of the biological properties of the people concerned, past experience, and the immediate situation. The past experience of other men in the form of culture enters into almost every event."[10]

Herskovits writes, "*Culture is the man-made part of the environment.* Implicit in this is the recognition that man's life is lived in a dual setting, the natural habitat and his social 'environment.' The definition also implies that culture is more than a biological phenomenon. It includes all the elements in man's mature endowment that he has acquired from his group by conscious learning or, on a somewhat different level, by a conditioning process—techniques of various kinds, social and other institutions, beliefs, and patterned modes of conduct."[11]

White writes, "The physical category is composed of non-living phenomena or systems; the biological, of living organisms. The cultural category, or order, of phenomena is made up of events that are dependent upon a faculty peculiar to the human species, namely, the ability to use symbols. These events are the ideas, beliefs, languages, tools, utensils, customs, sentiments, and institutions that make up the civilization—or *culture*, to use the anthropological term—of any people, regardless of time, place, or degree of development. Culture is passed down from one generation to another, or, it may be borrowed freely by one tribe from another. Its elements interact with one another in accordance with principles of their own. Culture thus constitutes a supra-biological, or extra-somatic, class of events, a process *sui generis*."[12]

Compare with these definitions Korzybski's definition of culture or civilization in the 1921 *Manhood of Humanity*. Korzybski writes, "Civilization as a process is the process of binding time; progress is made by the fact that each generation adds to the material and spiritual wealth which it inherits. Past achievements—the fruit of bygone time—thus live in the present, are augmented in the present, and transmitted to the future; the process goes on; time, the essential element, is so involved that, though it increases arithmetically, its fruit, civilization, advances, geometrically."[13]

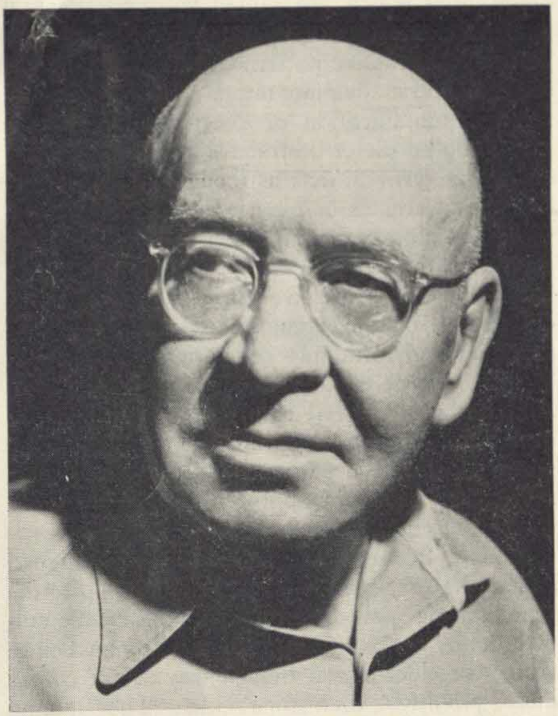
The faculty peculiar to the human species, namely, the ability to make complex use of symbols, to which White refers, and upon which the development of the time-binding or cultural capacity of man is dependent was, of course, fully grasped by Korzybski, and here, too, Korzybski was a forerunner of later thinkers in this field. Chapter IV of *Science and Sanity* is devoted to symbolism, and its very first words are "The affairs of men are conducted by our own, man-made rules and according to man-made theories. Man's achievements rest upon the use of symbols. For this reason we must consider ourselves as a symbolic, semantic class of life, and those who rule the symbols, rule us."[14]

This viewpoint had already been worked out by the distinguished philosopher Ernst Cassirer in his *Philosophie der symbolischen Formen*, published in three volumes in 1923–1929,[15] and in part made available in a new English version in *An Essay on Man*, published in 1944.[16] In the latter work, Cassirer writes, "Man has, as it were, discovered a new method of adapting himself to his environment. Between the receptor system and the effector system, which are to be found in all animal species, we find in man a third link which we may describe as the *symbolic system*. This new acquisition transforms the whole of human life. As compared with the other animals man lives not merely in a broader reality; he lives, so to speak, in a new *dimension* of reality. . . . Reason is a very inadequate term with which to comprehend the forms of man's cultural life in all their richness and variety. But all these forms are symbolic forms. Hence, instead of defining man as an *animal rationale*, we should define him as an *animal symbolicum*. By so doing we

can designate his specific difference, and we can understand the new way open to man—the way to civilization."[17] Writing at about the same time, and quite unbeknownst to each other, Korzybski and Cassirer had arrived at the same conclusions. Writing in 1944, Cassirer appears to have been quite unaware of Korzybski's work.

Sufficient, I hope, has been said to show that Korzybski's conception of time-binding and the anthropological conception of culture are virtually identical in character. It is rather sad to reflect that almost all anthropologists have overlooked this fact. It is to be hoped that this oversight will soon be remedied. Meanwhile, let us proceed to discuss certain consequences which follow from the general theory of time-binding or culture for the development of a science of human nature and the science and art of human relations.

Korzybski pointed out that "Human nature, this time-binding power, not only has the peculiar capacity for perpetual progress, but it has, over and above all animal propensities, certain qualities constituting it a distinctive dimension or type of life. Not only our whole collective life proves a love for higher ideals, but even our dead *give* us the rich heritage, material and spiritual, of all their toils. There is nothing mystical about it; to call SUCH a class a *naturally* selfish class is not only nonsensical but monstrous."[18] In this passage Korzybski comes very close to the discoveries which are only at this moment in process of being made concerning the nature of human nature. How right Korzybski was in referring to the belief that humanity is naturally selfish as nonsensical and monstrous. The belief in the inherent selfishness of man, his innate naughtiness, inborn evil, aggressiveness and hostility, has taken many forms, and in each of its forms it has done untold personal and social damage. In the first place, that belief has conditioned our attitude towards our fellow men, and not only to our fellow men, but to those utterly defenseless and dependent potential time-binders, the classes of babies, infants, children, and adolescents. Our belief in the inherent naughtiness of man has caused us to make out of the process of child-rearing a discipline of restraints and frustrations, which has had the effect of seriously crippling and distorting most human beings who have been exposed to it. There is rather good reason to believe that most of the personal and social tragedies which mankind has created and suffered from have been due to this erroneous belief and the consequences flowing from it as a result of the child-rearing processes it has conditioned. The belief in the innate naughtiness of man is very old, but in the nineteenth century it accrued unto itself a strong reinforcement in the doctrine of "the survival of the fittest." "The struggle for survival," "Nature red in tooth and claw," "dog eat dog," and similar phrases were used to describe the condition of animals in a state of nature. Since man is an animal, it was argued, he has inherited the same fundamental drives that keep the rest of the animal kingdom going. Hence, man is by nature aggressive and competitive, and, it was asked, do we not see this in the evolution of human societies, and in the struggles of the classes in society? The Spencerian application of the theories of Darwin to the struggle for survival in human societies, known as Social Darwinism, further lent enchantment to this particular view of the relations of men to one another.


Alfred Korzybski (Photo courtesy K. S. Keyes, Jr.)

Searching for the inherent naughtiness and aggressiveness of the infant at birth and thereafter, contemporary investigators have been unsuccessful in finding any evidences of it. The distinguished New York child psychiatrist, Dr. Lauretta Bender, having handled thousands of children, has written from her great experience, in an important article entitled "The Genesis of Hostility in Children," that far from being inborn, hostility or aggression in the child "is a symptom complex resulting from

deprivations which are caused by developmental discrepancies in the total personality structure such that the constructive patterned drives for action in the child find inadequate means of satisfaction and result in amplification or disorganization of the drives into hostile or destructive aggression." "The child," she writes, "acts as though there were an inherent awareness of his needs and there is thus the expectation of having them met. A failure in this regard is a deprivation and leads to frustration and a reactive aggressive response." Indeed, the creativeness of the organism is directed toward maturation in terms of cooperation. Bender calls it "the inherent capacity or drive for normality." And, as she says, "The emphasis on the inborn or instinctive features of hostility, aggression, death wishes, and the negative emotional experiences represents a one-sided approach which has led our students of child psychology astray."[19]

Maslow, in an article entitled "Our Maligned Animal Nature," writes, "I find children, up to the time they are spoiled and flattened out by the culture, nicer, better, more attractive human beings than their elders, even though they are of course more 'primitive' than their elders. The 'taming and transforming' that they undergo seem to hurt rather than help. It was not for nothing that a famous psychologist once defined adults as 'deteriorated children.'" "Could it be possible," Maslow inquires, "that what we need is a little *more* primitiveness and a little less *taming*?"[20]

Babies are born cooperative. What they want is to be cooperated with and to cooperate. When their needs for cooperation, for love, are frustrated, they may react with aggressive behavior. Aggressive behavior is originally a means of seeking and if possible compelling love. Aggression is practically always, if not always, the effect of love frustrated, or of the expectation of love frustrated. Children are not born selfish, they are made selfish by being forced to attend to their own needs as best they can by the failure of their discipliners to attend properly to their needs for cooperation. The natural selfishness of the child is, indeed, a monstrous notion. It is an unfortunate projection of themselves upon the child of those who hold it. Man is not born evil, nor is he born neither good nor evil, but in a very positive sense he is born good. Good in the sense of conferring survival benefits upon all with whom he comes into social relations. When one analyzes the basic needs of the human organism, those needs which must be satisfied if the organism is to survive, one finds that they are oriented in the direction of cooperation, of wanting to love, as well as wanting to be cooperated with and loved. It is in this sense that the organism may be said to be born good, and it is one of the few senses in which the word "good" means anything.[21]

Even if man had inherited any drives toward aggressiveness and combat, by virtue of his capacity for time-binding he would be capable of controlling the expression of such drives so as to negate and completely nullify their potencies. As for the doctrine of "the survival of the fittest," Korzybski pointed out that this "in the commonly used animal sense is not a theory or principle for a 'time-binding' being . . . its effect upon humanity is sinister and degrading."[22]

Thomas Henry Huxley, in a letter written October 27, 1890, wrote, "The unlucky substitution of 'survival of the fittest' for 'natural selection' has done much harm in consequence of the ambiguity of the 'fittest'—which many take to mean 'best' or 'highest'—whereas natural selection may work towards degradation *vide epizoa*."[23]

What, indeed, is "fitness"? The Social Darwinists overlooked the fact that fitness is related to the current environment of a group, and without further scruple they converted "fittest" into "best." The current environment of man, perhaps more than at any time during his whole history, demands that he realize pretty rapidly what his fitness must consist in if he is to survive. As Korzybski pointed out in 1921, "There is indeed a fine sense in which we can, if we choose, apply the expression—survival of the fittest—to the activity of the time-binding energies of man. Having the peculiar capacity to survive in our deeds, we have an inclination to use it and we survive in the deeds of our creation; and so there is brought about the 'survival in time' of higher and higher ideals. . . . It must be emphasized that the development of the higher ideals is due to the *natural* capacity of humanity; the impulse is simply time-binding impulse."[24]

Fitness for man, perhaps more than for any other creature, has fundamentally always consisted in and must increasingly come to consist in the subordination of individual competition to interpersonal cooperation, and of intergroup competition to cooperative association.* For this purpose it will be necessary to follow the highest ideals. But what are the highest ideals? Are they the same for all mankind? To answer the second question first: The ideals of human fitness are, indeed, the

* This is a précis of the words written by Patrick Geddes and Arthur Thomson in their book *The Evolution of Sex*, London: Scott, p. 311, (1889). "Each of the greater steps in progress is in fact associated with an increased measure of subordination of individual competition to reproductive or social ends, and of interspecific competition to cooperative association."

same for all mankind because all mankind is human and because these ideals are determined by the nature of human nature itself. As Korzybski stated, "Human logic— . . . the logic *natural* for man—will show us that 'good' and 'just' and 'right' are to have their significance defined and understood entirely in terms of human nature. Human nature—not animal nature—is to be the basis and guide of Human Engineering."[25]

Korzybski's contribution to the understanding of human nature did not lie in the fine analysis of its structure, his contribution lay in his distinguishing man from all other living creatures as the time-binder, and to emphasize the fact that this capacity is a natural one. Unlike Thomas Henry Huxley, however, he saw that man does not have to struggle against his inner nature but, rather, that he must realize it; for human nature, innate nature, was good, *not* evil. And Korzybski held that the discovery of the highest ideals should be based on the scientific understanding of the nature of human nature, in other words, that "what is right for man is what is right for human nature." I believe that when everything is said and done this assertion will be found to represent the fundamental formula for the life of man; that what is right for man is what is right for human nature. To some of us who are working in this field it is already evident that this is, indeed, so. For the findings of modern workers have shown unequivocally that in so far as men depart from the biologic demands of their innate nature they fall ill and become disoperative, and that in so far as they conform to the requirements of their nature they function harmoniously and well. But in order to recognize this fact it is first necessary to understand the nature of human nature. This is an area in which the most significant work has been done in the twentieth century, and in which a tremendous amount of work remains to be done before we can really speak of understanding the nature of human nature; but for some who have been interested in putting together the findings of modern science on the nature of human nature the general picture is clear, and it is in complete conformity with Korzybski's views. I now propose to give an outline of that general picture.

Man is, of course, part of the world of Nature. He comes into being in fundamentally the same way as all other creatures, that is, by reproduction. The reproductive process is the fundamentally social process, and it is this reproductive process which determines the pattern of the biologic life of man, and of all other living creatures as well. The process of reproduction is an interdependent dependent one, and that is the essence of the biologic relation between organisms, interdependency and dependency. This holds true whether we are concerned with unicellular or multicellular organisms, for the amoeba or for man. Man, however, is perhaps more strikingly characterized by this interdependency-dependency trait than any other living creature. Following conception he spends nine months in that dependent-interdependent state, and after birth he continues to spend several years in dependent-interdependent relationship of the most dependent kind with other human beings. Dependency is man's basic biologic state and interdependency is man's basic social state. Man comes into the world biologically wanting to be dependent and interdependent. How do we know this for a fact? We know this for a fact because if the interdependency needs of the potentially human organism are not satisfied the organism simply does not develop as a human being, and if its dependency needs are not satisfied it can develop only in a crippled sort of way. The infant's drives are oriented in the direction of love and cooperation, it wants to love and it wants to be cooperated with, and if it is loved and cooperated with it then develops in every way more efficiently as a loving cooperative human being, and also as a healthy physical organism. How do we know these things for facts? We know these things for facts because if the potentially human organism is not adequately loved or cooperated with it fails to develop as an adequate human being, as a being who is functioning harmoniously and who is adequately capable of loving and cooperating with others. The evidence for these relationships has recently been ably discussed and summarized in a World Health Organization publication entitled "Maternal Care and Infant Health" and written by the English psychiatrist John Bowlby.* As for the relation between adequate love in infancy and proper physical growth and development, the work of Dr. Ralph Fried at Cleveland[26] and of others elsewhere is conclusive. This should not surprise us in the least, for the organism is a whole, and one cannot deprive it of necessary stimulations in any part of it without affecting the whole organism—that is what the word "organism," properly understood, in part implies. Not only this, but we now know that when children within the first year and during the greater part of their first six years are adequately loved they grow up to be adequately loving and cooperative healthy persons, persons who are capa-

* Issued in the United States by Columbia University Press, New York, 1951.

ble of taking the stresses and strains of life very much more efficiently than those who have not been adequately loved during the first half dozen years.[27] From the studies which have been made among the non-literate peoples of the world, the so-called "primitive peoples," anthropologists have found that the way in which children are brought up is closely related to their personality structure; and on the whole, the conclusion is that the more adequately children have been loved during their first six years the more loving they are as adults, whereas the more frustrated children have been during their first six years the more frustrated they are as adult personalities and the more unloving. This may be putting the essence of the findings in an oversimplified form, but I do not know of any better way of stating the facts in a few words than in this manner.

What, in brief, has been discovered is that to live as if to live and love were one is the only way of life for human beings, because, indeed, this is the way of life which the innate nature of man demands. The highest ideals of man, therefore, spring from his own nature, and the highest of these ideals and the one which must inform all others is *love*. This is not a new discovery in the world; what is new is that scientists should have made it by scientific means. What contemporary scientists working in this field have done is to give, without in most cases being aware of it or intending to, a scientific validation to the Sermon on the Mount: To love thy neighbor as thyself: To do unto others as you would have them do unto you; principles which were enunciated by many philosophers, prophets, and seers, long before the birth of Christ. Indeed, every people has at sometime or another arrived at this elementary piece of wisdom on the basis of experience, and most peoples have more or less successfully attempted to live by these principles because they have discovered them to be the most efficient to live by. This, at any rate, is their theory enshrined in their religious and civil codes, but the practice seems to lag far behind the religious theory and the civil codes so frequently derived from and based on those religious theories. What is the reason for this difference between theory and practice?

In the light of our modern studies the answer to this question would not seem to be a very difficult one, although any answer that is returned is likely to be an oversimplification. The reason why people do not live by the principle of love is that they have not been raised by it. On the other hand, most of them have been raised by the principle of systematic frustration—which in our culture we often call "discipline." Most of the so-called civilized world simply has not loved little children adequately enough, no matter what their holy books have said about love, and if you have been pushed around as a child you are likely to grow up as a pusher-around of others when you are an adult. You can be terribly interested in love if you have been deprived of it as a child, but you will be unable adequately to receive it and unable adequately to give it. One may tell children that one loves them at the same time that one is frustrating them. What they remember is the frustration, not the "love." For what children believe in the behavior of others is what they do, *not* what they say.

How, then, is one to escape from this vicious circle: Frustrated children growing up into frustrating parents who in turn produce frustrated children who grow up to be frustrating parents? The answer is: By educating the world of human beings in the facts, by showing all who are capable of learning what the true nature of human nature is, and why it must be respected; what it is that human nature demands and why those demands must be obeyed, for as Bacon put it, "Nature to be commanded must be obeyed." We must teach all who are capable of learning—and everyone is capable of learning—what happens when you do not obey the innate demands of one's being, and what happens when one does. We must, in short, teach the art and science of human nature, the art and science of human relations. We must relieve mankind of the load of myths which has been weighing it down for so long about the nature of nature and the nature of human nature. We must disabuse mankind of the myth of its inherent naughtiness, and present the facts which will permit men to judge for themselves what the truth really is. But above all else we must teach men how to love. This can all be achieved by many different approaches and at innumerable different levels, but the one method in which I place the greatest faith is education. Such education must be conceived as the bringing out of the best that is within the person by making available to him all the encouragements and supports and stimulations which he requires to enable him to become a loving, cooperative, non-conflictful person; one who is not only aware of what is right with the world but also what is wrong with it, and who is equipped with both the knowledge and the desire necessary to improve it nearer that ideal of what it should and can be. Such a person will not be a competitor, but a cooperator, a person for whom altruism will

be a passion and selfishness a disorder; a person wise enough to know that

> He who would love his fellow men
> Must not expect too much of them;

a person who will want to improve the world as he finds it, and not accept things as they are, but who will also have the wisdom to know what things to accept and what to change; a person who will not risk wrecking the social machinery by exceeding the speed-limit of rational inquiry; who will not abolish anything but merely make it necessary to discontinue it, dispelling fear by supplying facts and knowledge; who will recognize the strange necessity of beauty; who will have a sense of personal responsibility for decency and justice; who will never burn the smoke of incense before an empty shrine; a person, in short, who, having had a loving order made within himself, will make loving order in the world.

> —be to other souls
> The cup of strength in some great agony,
> Enkindle generous ardour, feed pure love,
> Beget the smiles that have no cruelty—
> Be the sweet presence of a good diffused,
> And in diffusion ever more intense.
>
> George Eliot

I conclude with some words from Korzybski's *Manhood of Humanity*:

"In humanity's manhood, patriotism—the love of country—will not perish—far from it—it will grow to embrace the world, for your country and mine will be the world. Your 'state' and mine will be the Human State—a Cooperative Commonwealth of Man—a democracy in fact and not merely in name. It will be a natural organic embodiment of the civilizing energies—the wealth-producing energies—characteristic of the human class of life. Its larger affairs will be guided by the science and art of Human Engineering—not by ignorant and grafting 'politicians'—but by scientific men, by honest men who *know*.

"Is it a *dream*? It is a dream and science will make it a reality."[28]

References

1. Russell, B. *The Impact of Science on Society.* New York: Columbia Univ. Press (1951).
2. Korzybski, A. *Manhood of Humanity.* Lakeville, Conn.: Institute of General Semantics, 10 (1951).
3. *Ibid.*, 11.
4. *Ibid.*, 22.
5. *Ibid.*, 34.
6. *Ibid.*, 40.
7. *Ibid.*, 44.
8. *Ibid.*, 59.
9. Firth, R. *Elements of Social Organization.* London: Watts, 27 (1951).
10. Kluckhohn, C. *Mirror for Man.* New York: Whittlesey House, 18 (1949).
11. Herskovits, M. J. *Man and His Works.* New York: Alfred A. Knopf, 17 (1948).
12. White, L. A. *The Science of Culture.* New York: Farrar, Straus, 15 (1949).
13. Korzybski, A. *Manhood of Humanity.* Lakeville, Conn.: Institute of General Semantics, 106 (1951).
14. ———. *Science and Sanity,* 1st ed. Lancaster, Pa.: Science Press, 76 (1933).
15. Cassirer, E. *Philosophie der symbolischen Formen,* 3 vols. Berlin: Bruno Cassirer (1923–1929).
16. ———. *An Essay on Man.* New Haven, Conn.: Yale Univ. Press (1944).
17. *Ibid.*, 24.
18. Korzybski, A. *Manhood of Humanity.* Lakeville, Conn.: Institute of General Semantics, 143 (1951).
19. Bender, L. *Am. J. Psychiat.*, **105**, 241 (1948).
20. Maslow, A. H. *J. Psychol.*, **28**, 273 (1949).
21. Montagu, M. F. A. *On Being Human.* New York: Henry Schuman (1950).
22. Korzybski, A. *Manhood of Humanity.* Lakeville, Conn.: Institute of General Semantics, 142 (1951).
23. *Life and Letters of Thomas Henry Huxley,* vol. 2. Leonard Huxley, Ed. New York: D. Appleton, 284 (1901).
24. Korzybski, A. *Manhood of Humanity.* Lakeville, Conn.: Institute of General Semantics, 143–145 (1951).
25. *Ibid.*, 149.
26. Fried, R. and Mayer, M. F. *J. Pediatrics,* **33**, 444 (1948). *Life Stress and Bodily Stress.* Harold G. Wolff *et al.*, Eds. Baltimore, Md.: Williams & Wilkins (1950).
27. Halliday, J. L. *Psychosocial Medicine.* New York: Norton (1948).
28. Korzybski, A. *Manhood of Humanity.* Lakeville, Conn.: Institute of General Semantics, 199–200 (1951).

Psychology as a Science

GREGORY A. KIMBLE*

The author is Associate Professor of Psychology at Duke University. He holds degrees from Carleton College, Northwestern University, and The State University of Iowa. Before becoming affiliated with Duke, he taught at Brown University and later at Yale University. His research interests are in the psychology of learning, a field in which he has published extensively.

THE approach I would like to take to this discussion of psychology as a science is simply that, whether or not such is generally the case now, it is possible to approach the problems of psychology scientifically. Furthermore, it seems to me that about our only hope in this task lies in making use of the things that the philosophers of science are able to tell us. I have, therefore, tried to decide just what lessons these philosophers can teach us and how they can be applied to psychology. My sincere hope is that the summary of my thinking on the topic which I am about to present does not do the philosophers too great an injustice, or, if it does, that they can at least be philosophical about it.

Let us begin by stating the subject matter with which psychology deals. The standard definition of psychology is that it is *the science of behavior*. This definition probably needs clarification. On the one hand it covers less than you may have expected. On the other, it covers too much. Note first that the definition says nothing about mind, or experience, or consciousness. This is not merely an attempt to finesse the problems raised by these three terms. If they are to be used at all in a science of psychology, they will have to be introduced in a way to be described later. A second point is that the behavior with which the psychologist deals tends to be limited to its grosser aspects. Where internal physiological processes are a part of psychology, they are conceived as aids to conceptualization rather than as a part of its basic subject matter. I shall return more than once to the question of the relationship between psychology and physiology; let us turn now to a discussion of the use of the term, "science" in the expression, "science of behavior."

The word, science, refers to the attempt that people called scientists make to bring order into the world of observable events. Scientists are, of course, not the only ones trying to bring order into the world. Others are the theologians, some literary people, and the philosophers. The scientific way of approaching the task differs from these latter ways, chiefly in terms of its initial data. All science begins with the sensory experience of a perceiving scientist. These sensory experiences, which are private and not a part of science, give rise to its initial data, that is, to a publically observable *report* of such experiences. In this sense, psychology is exactly like any other science. The report with which psychologists are provided may be about rats running a maze, physiological processes, test results, or even the scientist's own introspection. But the element of all of this that is useful is the report that is open to public inspection.

The particular task which the psychologist has taken for himself is that of trying to make some sense of behavior. But we have just seen that, as scientists, psychologists must concern themselves with events that are publicly observable and, therefore, verifiable. This restriction on the activities of psychologists raises a question: Just what kinds of observations can they make on behavior that are of this sort? The answer to this question comes in two parts. First, psychologists have found (just as other scientists in other fields have found) that human behavior is so complicated that it is impossible even to talk about all of the activity of an individual at any one time. So, in practice, they restrict their observation to limited portions of behavior called responses. Second, even responses cannot, without the aid of photographic assistance, be recorded in detail. So, their reports of behavior typically deal with some abstracted characteristic

* This (somewhat revised) is a paper read before the Rhode Island Philosophical Society at Brown University in 1949. Its purpose is to present in simple language the ideas that are basic to modern behaviorism. A more technical development of these ideas (and probably some of the happier phraseology) can be found in the writings of Bergmann and Spence. The sophisticated reader will note that this account underemphasizes the deductive aspects of theorizing. The omission does not mean that I regard this process as unimportant. It means, instead, that the purposes of this paper could be achieved in a more straightforward manner by stressing less technical matters. I am indebted to Professor Bergmann for reading an earlier version of the manuscript, and to Professor C. J. Ducasse of Brown University, whose interest in and criticisms of this paper have led to a number of basic improvements.

like the speed or the magnitude of the response.

Having stated the general manner in which the psychologist views behavior, a word must now be said about a primary assumption, which is that these various aspects of behavior are related to events in the person's past and present environment. Although the collection of variables in this category might better be called antecedent and attendant circumstances, the psychological convention is to call them stimuli. When the psychologist speaks of stimuli he usually is referring more or less broadly to events or changes in events in the environment. So far as these stimulus variables are concerned, they are so numerous that no attempt to enumerate them here is possible. A few examples will have to do. In the field of learning, for one example, the efficiency of a learned reaction is known to depend in a regular fashion upon the number and spacing of practice trials, upon the amount of reward the subject is given for making the response, and upon many other variables. In vision, what a person sees (really what he *says* he sees) is known to depend upon the area of the visual stimulus, upon its brightness, upon the duration of its presentation in the visual field, and so on. One thing that should be noticed about these particular examples of environmental events is that they are quantifiable in terms of some standard physical measuring scale. Not all variables with which psychologists have to deal are yet subject to measurement in any very respectable sense of the word. For the kind of information that is needed to build a science of psychology, however, quantification of these independent variables is a necessity of which psychology is acutely aware.

The final task in the development of a behavior science is simply that of stating the relationships that exist among the variables that have been isolated. What psychologists are after is a set of laws of the general type: $R = f(S)$, where R is some aspect of a response, S is a stimulus event and f represents the functional relationship. These are what I will call S-R laws. It should, of course, be realized that the formula, $R = f(S)$, is highly schematized. Behavior is seldom predictable from knowledge of a single S-variable. The S in this equation stands for an indefinite number of antecedent conditions which in actual practice will have to be discovered along with their relevance to behavior.

Because of this complexity in the determination of behavior, the S-R laws have turned out to be very difficult to discover. Where this has happened, psychologists have sometimes had more success using a different approach, in an attempt to discover the relationships that exist among *response* variables. As a convenient example of this, one may take the very important work of the clinical psychologists and the psychological testers. They almost never deal with the S-R type of relationship at all. Instead they use what are sometimes called R-R laws. The distinction between these two types of relationship is shown in the following example. There is some rather good evidence to suggest that an individual who is frustrated in his various strivings develops a tendency toward aggressive behavior. For purposes of exposition, it can be assumed that this is a well-established relationship. On this basis, if one knew that an individual had been frustrated many times in his efforts to achieve important goals, it could be predicted that he would show aggressive behavior. In this case, an S-R law would be used to make the prediction. The determining S-variable is the frequent blocking of goal-directed behavior, and the dependent response variable is the aggressive behavior. If it were known that aggressive behavior in some form always occurs as a consequence of frustration and that it never occurs in the absence of frustration, there would be some point in saying that aggression occurs *because* of frustration. Furthermore, given a control over the person's environment and a knowledge of the S-R law, the amount of his aggressive behavior could be controlled.

A clinical psychologist, on the other hand, might give this same person a psychological test and come out with the statement that he has strong aggressive tendencies and may be expected to show a great deal of hostile behavior on many occasions. This prediction (which would be as correct as the other one) would be made in terms of a different kind of law. What the clinician has done is to observe a small segment of the individual's behavior in a controlled situation called a test, and, from his behavior in this situation, to make a prediction about other behavior. His prediction is made in terms of an $R_2 = f(R_1)$ kind of relationship, in which R_2 is the predicted aggressive behavior, R_1 is the predicted-from-test behavior, and the function is a correlation coefficient. These R-R laws are diagnostic laws, and carry no implications of causality. (The statement that the person tested is aggressive because the clinician has given him a test is obviously absurd, at least until such a time as the clinician's services are more expensive and, therefore, more frustrating than they are now.) Furthermore, the R-R laws provide no control over behavior in the absence of further information of the S-R type.

The commodities in which the psychologist deals are, then, two types of relationship which I have called S-R and R-R laws. The precise statement of

a large number of such laws would be extremely valuable, but it would not make psychology a high-order science. What would still be needed would be the collection of the *S-R* and *R-R* relationships into some more orderly or integrated kind of formulation. In actual practice, it has been found useful to attempt the formulation without the comprehensive set of empirical laws it is hoped eventually to integrate. Such attempts at formalization are called systematic psychology or psychological theory.

The form that psychological theorizing has taken has been toward the development of intervening state variables which are considered as standing between S and R or between R_1 and R_2, depending upon the kind of relationship. The intervening states that have been postulated have been of two general sorts, physiological states and hypothetical constructs. If we consider first the type of intervening variable used by the more physiologically minded psychologists, the paradigm in terms of which they attempt to explain behavior may be expressed in the following way: *S-O-R*. Stimulation according to this scheme is thought of as producing some physiological or organic change in the individual. And this physiological change, in turn, is thought of as being responsible for whatever behavior is being observed. Such psychologists investigate the relationships between neural, glandular, circulatory, and muscular events, on the one hand, and behavioral events on the other.

Another large group of psychologists make use of the hypothetical construct type of intervening variable in their explanation of behavior. The paradigm, in terms of which such psychologists work, is the following: *S-H-R*. Antecedent stimulus conditions are thought of as defining a hypothetical intervening state in terms of which responses are predicted. Examples of such hypothetical constructs in fairly general use are: habit, motivation, fatigue, attitude, symbolic processes, and the like. About these hypothetical constructs, there are a number of things that need to be said. The first of these is that there is no reason in the world to assume that they *must* have any actual existence inside the organism or anywhere else. Some of them, at least, are analogous to the physicists' concept of velocity which derives its meaning from its definition: $V = S/T$. The physicists do not spend much time worrying about what or where velocity really is. Psychologists, on the other hand, spend a good bit of time looking for the neurophysiological events which they think *have* to exist and to correspond to their concepts, however imperfect. What is worse, they spend even more time speculating about them. There are just two comments that I wish to make about this. One is that, although all behavior probably has its neurophysiological aspects, mere speculation about them is a fruitless procedure. The other is that, although physiological *information* may someday turn out to be useful for psychology, there is nothing in the program of psychology which makes such information indispensable. For all practical purposes, it is possible to construct a science of psychology in which the organism is considered as empty. For my own part, I can conceive of a psychology based on stimulus and response events entirely, one in which the existence of the organism is a completely unimportant fact. The scientific account will, after all, deal with behavior in the abstract. Such a science no more needs to refer to the organism than the science of gravitation needs to refer to stones or to the Leaning Tower of Pisa.

Whether the constructs that are introduced between stimuli and responses are physiological or purely hypothetical, they have to meet certain criteria to be acceptable. The most important of these criteria can be summed up in one word: *meaningfulness.* Intervening variables must be meaningful in two ways. The first is what has been called *operational meaning.* The meaningfulness of a concept in this sense depends upon the adequacy of its definition. A concept may be said to be adequately defined providing there exists a set of defining statements for it which lead eventually to phenomena in the sensory experience of the scientist, and providing further that these statements are such that any properly qualified and equipped person can put them to a test. This requirement is no more than a particular application of the more general criterion of public observability.

Concepts for which no such definition is *possible* are of no use to science. This is not, however, to say that concepts which have never been adequately defined never will be. They may or may not be. Concepts such as mind, consciousness, and experience are in the class of intervening variables for which no satisfactory operational definition exists. This is in part because of the complexity of the phenomena involved, and in part because of their inherent privacy. But this state of affairs does not demand the conclusion that respectable definition is impossible.

On the other hand, an operational definition of these terms (assuming it is achieved) does not guarantee their utility. The second kind of meaningfulness remains to be demonstrated. This point can be made clear with the aid of an illustration. I can define a construct as follows: Kappa (for Kimble) = the square root of the number of hairs on my head divided by my diastolic blood pressure minus the length of my great toe. This concept,

Kappa, although its operational definition is impeccable, has not been used much in psychology because, so far as we know, it has no meaningful relationship to any of the events with which psychologists are supposed to be dealing. In this second sense, a concept is meaningful if it is related to behavior, or aids in its prediction.

Just as there are two major kinds of laws with which the psychologist deals, there are two kinds of operations in terms of which a concept may acquire operational meaning. The first of these is by definition in terms of antecedent stimulus conditions. The second is by an inference from behavior. To illustrate, let us consider a case in which both types of definition have been used and return to the example of aggression. It has already been pointed out that predictions of aggressive behavior might be made either from a knowledge of an individual's history of frustration or from his behavior on a test. Using exactly the same evidence, there can be developed two different definitions of the concept aggression. In terms of antecedent conditions, aggression can be defined as a positive function of the number of frustrations a person has experienced. In terms of behavior, it can be said that a person is aggressive if his pattern of test responses is so and so. The one thing we cannot say is that we now have two different definitions of the "same thing." Different definitions define different concepts. In psychology, especially, there is the need to be cautioned against the tendency to treat things as identical just because they have the same name. Whether the frustration-produced aggression has anything to do with the test-defined aggression is an empirical question which can be decided by research.

As I see it, just one final point needs to be made with respect to the science of psychology as it is developing. It must be presented as a sort of confession. Not all psychologists agree with the analytical, behavioristic (what James would have called "tough-minded") sort of analysis I have been presenting of the field. In psychology, we hear a great deal about "the total situation," "the whole child," "the whole personality," and "global intelligence." Psychologists who use these expressions object to the type of account I have given, chiefly because of the analytical approach which it implies. This cold analysis, they say, is wrong because it fails to capture the warmth and vitality of human behavior as we know it in our common-sense, firsthand experience. Unfortunately for common sense, that is how it is with science. Common sense tells us that the world is flat; science says that it is round. Our firsthand experience tells us that the sun moves slowly around us; science says that we are moving around the sun, and at a breakneck speed. Direct observation tells us that the chairs in which we are sitting are solid; science says that they are mostly empty space. The mere fact that some line of scientific argument produces a description of the world that fails to correspond to our naive experience of it is no obstacle to that argument. And if there are aspects of behavior that cannot be handled in the way I have been describing, I know of no way of finding this out without giving it a try.

At a somewhat different level, the opponents of behavioristic psychology sometimes object that the analysis of behavior destroys its essentially continuous or integrated character, and robs it of its wholeness. They now propose what they call a field theoretical approach to human behavior and very bitterly deplore what they call the *mechanism* of the sort of argument I have been expounding. They say that their new field theories stand to the more orthodox psychology as does the theory of relativity to Newtonian mechanics. The adequacy of this analogy, of course, depends upon some understanding of what the term, "field," means in physics. It seems to have been used in two rather different ways. Basically, by a "field," the physicist means a system of interrelated variables that differs from other systems of physics partly in the kind of calculus which expresses the interdependencies. Unfortunately, the mathematics are difficult, even for a physicist. So, for their nonmathematical brethren, the theoretical physicists have constructed models (as, for example, the Rutherford-Bohr atom) in an attempt to convey what is involved in their equations. Since the so-called field theories in psychology consist of anthropomorphic models in this second sense, it seems to me that they cannot be called field theories in the sense that the theoretical physicist uses the expression. On the other hand, if one wished to consider as field theories approaches to psychology that come closer to the actual mathematical statement of the interdependencies involved, then the very people who bear the stigma, "mechanistic," would have to qualify as the greatest field theorists of them all. Finally, it must be pointed out that science is concerned with the regularities in the world; that is, with events that repeat themselves. Whole situations do not repeat themselves, at least not very often. Analysis of the elements common to somewhat different situations will be absolutely essential in order to achieve a science of psychology.

This, then, is what the science of psychology, as it begins to be rather definitely defined, is like. It is concerned with the prediction of behavior either in terms of other behavior or else in terms of antecedent stimulus conditions. Its proponents are

trying to develop a consistent body of scientific knowledge expressed in terms of whatever lawful relationships may be found to exist among these variables. Because of the tremendous complexity of this task, it has been necessary to use concepts that are considered formally as intervening between stimuli and responses in one case and between predicted responses and predicted-from responses in another. Furthermore, different psychologists develop different kinds of concepts. Some use mechanistic ones, some use field theoretical ones, others think physiologically. Which of these approaches is the best cannot be decided on the basis of *type* of concept. What really counts is the extent to which these concepts aid in the primary task of psychology —the understanding of behavior.

LYCAENOPS

The mammals, as indicated by Dr. Rodbard in the present issue of this journal, may owe their evolutionary success, at least in part, to their warm-bloodedness. This character, with its accompanying mechanisms that permit maintenance of a constant internal environment, has undoubtedly played an important role in the successful establishment of mammals in a variety of external environments. Paleontological evidence strongly indicates that the mammals evolved from the mammal-like reptiles. These, which comprise the reptilian subclass Synapsida, actually were among the first reptilian groups to appear on the earth; indeed, they had already passed their zenith before the coming of the first dinosaur. Primitive mammal-like reptiles made their entry in late Pennsylvanian times and underwent their great evolutionary radiation during the Permian period. By the Mesozoic era they were on a marked decline, and disappeared in the Jurassic period. The mammals are believed to have arisen during the Triassic period from some group of generalized mammal-like reptiles. Although the precise ancestors of the Mammalia are as yet unknown, members of the suborder Therapsida of the mammal-like reptiles, which flourished from middle Permian to middle Triassic times, practically bridged the structural gap between reptiles and mammals: thus, e.g., the lateral surface of the skull, the palate, and the lower jaw are largely of the mammalian pattern; the teeth, instead of being relatively simple cutting cones, as in typical reptiles, exhibit differentiation into incisors, canines, and cheek teeth, as in mammals; and many features of the limb skeleton are distinctly mammalian and strongly suggest a mammalian type of posture and locomotion.

The picture on the cover of this issue is of a restoration of the mammal-like reptile, *Lycaenops,* in a late Permian landscape. This restoration, made by J. C. Germann, under the direction of Dr. Edwin H. Colbert of the American Museum of Natural History, New York City, appears through the courtesy of the American Museum of Natural History. *Lycaenops* is a member of the order Therapsida, suborder Theriodontia, family Gorgonopsidae. It represents one of the more primitive theriodont types probably on, or near to, the line of evolution that led to the mammals. For further information, interested readers can consult such publications as *Vertebrate Paleontology,* by A. S. Romer, University of Chicago Press, Chicago (1945); *The Life of Vertebrates,* by J. Z. Young, Oxford University Press, New York (1950); and The Mammal-like Reptile *Lycaenops,* by E. H. Colbert, in *Bull. Am. Museum Nat. Hist.,* 89, 353 (1948).

SCIENCE ON THE MARCH

OPERATIONS RESEARCH AND INDUSTRIAL ENGINEERING: CONTRAST AND RESEMBLANCE

MORE than a half-century ago Frederick W. Taylor conceived and vigorously espoused his "Principles of Scientific Management." In his justly famous book by that name he described the dramatic experiment with Schmidt, the pig-iron handler, whose daily output was increased from 12½ to 47½ tons by application of new techniques of work measurement and wage incentive.

Taylor's "scientific managment" aroused great controversy during his lifetime and, indeed, still does, as many disputes between labor and management will attest. Despite this extensive controversy, the techniques of time study and the application of wage incentives have spread over a large portion of American industry. Most of what today is called industrial engineering rests upon a foundation of work measurement and wage incentives and, in common usage, there is an inevitable association: *time study* to a considerable extent defines industrial engineering and *industrial engineering* always connotes time study.

In devotion to the techniques discovered and used by Taylor there has been an absence of consideration given to the organizational role that he filled. In his celebrated studies of pig-iron handling, shovelling, and metal cutting, Taylor and his associates were in a unique staff relationship: they were, perhaps for the first time in formal organization, assigned to the task of analyzing operations with the objective of effecting improvements in them. This was and is a role which has no exact counterpart among other staff units. Personnel departments, accounting, finance, maintenance, even research and development, presumably are expert in their respective fields but their tasks primarily are *doing* things in these areas, not in analyzing and seeking to change the operational activities of others. Taylor's unique organizational role was a creation just as significant, perhaps much more significant, than the development of techniques in time and motion study.

Since Taylor's day, industrial engineers unfortunately have followed somewhat slavishly in his footsteps. The Gilbreths contributed notably to motion study and Gantt's famous chart and his insight into human problems were futher advancements of great importance to industry. But, by and large, industrial engineering has been devoted too narrowly to setting time standards for wage incentive application.

II

During World War II a new approach to the analysis of action problems achieved dramatic success. Since the War that approach has come to be called *operational research* in England, the country of its birth, and *operations research* in the United States. In both countries postwar development has been rapid, with attendant controversy that is reminiscent of the early days of scientific management.

Some of this controversy takes place among the operations researchers themselves, who are thus far unable to agree upon a uniform definition of their activities. They do agree that their mission is the study of operations with the objective of advising administrators in decision making, and they do agree that their approach to the study of action problems involves the use of multidiscipline teams, suitably assembled for each particular problem. Thus their staff role is the same as that of industrial engineers but their approach is very much broader.

On the content and scope of the research teams there is much disagreement. One "school" of operations research believes that the disciplines represented should be limited to those of the physical sciences and that counsel given to administrators should be restricted to those findings which can be quantified with precision. Their usual objective is reduction of the problem to a mathematical model. Other practitioners also favor mathematical models but believe that the multidiscipline approach should embrace *all* fields of scholarship, the biological sciences, the social sciences, and the humanities, whenever action problems involve elements of such fields. This group believes that precise, less exact, and even intuitive information may be supplied to the line administration, provided the nature and tolerances of each class of information are identified. Since almost all administrative decisions involve elements which defy quantification, and since every action problem in

human affairs involves the emotions and aspirations of people, the broader scope of the second point of view seems much more realistic to this observer.

Other controversy, centered chiefly in the academic world, concerns allegations that operations research is a new and separate discipline, that the whole is different from the sum of the parts represented by the multidiscipline approach. In these arguments one may detect some measure of defensiveness on the part of operations researchers who desire academic ties and academic respectability. Perchance there is also a measure of intellectual snobbishness on the part of academicians who disdain contact with mundane action affairs.

III

Beyond these arguments, and much more important than the disputation, are significant facts. Operations research occupies the identical, unique organization relationship as industrial engineering, but its analytical staff role is capable of being infinitely more fruitful because its investigations are not bounded by work measurement and incentive concepts. In fairness to industrial engineers it must be said that they too have broadened their vision but, still, operations research literally compels consideration of many facets of action problems which traditionally have not been studied by industrial engineers.

The importance of this unique and now augmented organization relationship is enhanced by the undisputed fact that operations research has brought to bear upon action problems a new group of people, scholars and scientists who are imaginative and resourceful, who have fresh, tradition-free points of view, who are capable of putting to new uses tools of science and the humanities. There are at least two consequences of this entry of new people into a new field: the scholars themselves are discovering the fascination and the difficulty of problems in the world of affairs, and they are achieving dramatically successful results in their operational studies

Here too there are analogies to the early days of industrial engineering. Operations research teams on more than one occasion have proclaimed that improvements in operational situations have derived from application of the "scientific method." What actually took place was the organizational assignment to operations analysis of persons trained in science; what they thereupon did was inventive, ingenious, and altogether commendable, but it was not the "scientific method" in so far as that term implies controlled experiment, measurement and quantification, and reproducibility. Frederick Taylor yielded to just the same impulse, in his day with better cause, when he called his approach "scientific management" because it was analytical and systematic.

Comparison also can be made with the "Jack Horner" attitude too often shown in the past by industrial engineers when they have boasted of increases in productivity and reductions in costs by ignoring or suppressing negative attributes of their work. Not without cause is "efficiency expert" a term of opprobrium. The entry of quacks into the field, or the substitution of vanity or boasting for intellectual honesty, can have the same detrimental effect upon the future of operations research. The practitioner who promises to reduce the ramifications of an organization to a magic mathematical model is of the same harmful breed as the efficiency expert who promised to double production by application of a time study-wage incentive formula.

IV

These admonitions and seeming criticisms are by no means trivial but they are relatively minor. The fact remains that operations research brings new people of great ability and new concepts of great value to the study of problems in the world of affairs. It is often said that science and technology have outstripped our capacity for the peaceful assimilation of new discoveries. It is also said that the humanities and social sciences must meet this need by the rapid extension of their own frontiers of knowledge. Operations research will provide no panacea for the social problems created by science, but the union of scholars and action problems is certain to be beneficial.

Economically, organizations of all kinds need staffs whose task is the analysis and improvement of operations, and realization of this need is just as important as the techniques used or the people employed, whatever they may be called. Despite its history of controversy and the narrowness of its approach, organizations have profited from industrial engineering. It is fair to hope that even greater gains will come from the imaginative and resourceful teams of operations research.

Robert H. Roy
The Johns Hopkins University

BOOK REVIEWS

BIRDS AND BEASTS

Birds and Mammals of the Sierra Nevada. Lowell Sumner and Joseph S. Dixon. xvii + 467 pp. Illus. $7.50. University of California Press, Berkeley and Los Angeles. 1953.

THIS book is far more comprehensive than the title indicates. The short introduction and the first nineteen pages should be required reading for all, preferably early in life.

The introduction points out changes that man is bringing about on the face of the earth and it raises questions on which all should ponder, for they are changes that affect not only the plant and animal life but they have a great and often adverse effect on man as well.

The first chapter, Wildlife Policies and Problems, sets forth the factors that are altering plant and animal life in general wherever so-called civilized man conducts his developments or exploitation, and quotes carefully formulated policies that have been developed by the U. S. National Park Service for administration of National Parks and other areas under its jurisdiction.

From this, we soon see that the principal section will treat of the birds and mammals of the Sequoia and Kings Canyon National Parks, including the General Grant Grove section of Kings Canyon National Park, rather than the entire Sierra range. However, the area has been intensively studied by careful workers and it is typical of the entire Sierras, hence the title is not misleading. A large black-and-white map shows boundaries of the area, some topographic features, trails, and other man-made features.

The second chapter, Life Zones, not only gives the customary specific information regarding the characteristics of the various life zones in the Sequoia-Kings Canyon region with which biologists are familiar, but it gives simple explanations regarding life zones that enable the novice to understand life zones in general and their role in studies of biological subjects. Another large black-and-white-hatched map shows the life zones of Sequoia and Kings Canyon National Parks.

The birds are treated in pages 20 to 275. Each species or subspecies is discussed in three sections: a brief Description, Habits, and Park Status and Record. The descriptions are sufficient to enable the lay student of birds to be reasonably certain of his identification in the field, and it omits the long lists of measurements and technical descriptions that are useless unless one has specimens at hand. Discussion of habits of the various birds properly differ considerably in length. For birds that are well known the accounts of habits are brief, but for those forms whose habits are not so well known the author has used sufficient space to set forth new facts or peculiarities that are of special interest. In Park Status and Records, if the form is of regular and fairly common occurrence it is treated briefly. If it is rare or there are noteworthy records regarding the form, the account is more comprehensive. One hundred sixty-four forms of birds are discussed at length. Twenty-six forms are treated briefly under the chapter Vanishing Species and Questionable Records, Birds.

Seventy-eight forms of mammals are treated in a similar manner on pages 276 to 467. This includes seven forms under the chapter Vanishing Species and Questionable Records, Mammals. The discussions of the mammals are longer, on an average, than those of the birds and set forth many interesting facts that are not generally available or known. The average sizes of mammals are given in terms of total length and length of tail. It is pleasing to note that body weights are also given, a feature which is rare in books on animals. There is a two-page tabulation that should assist both the layman and the zoologist to distinguish between the forms of chipmunks (Eutamias) that frequent the area. The mammal section shows the meticulous thoroughness of Joseph Dixon, its author, and includes facts based on his long, painstaking studies in the field that might easily be missed by zoologists interested only in taking specimens.

The forty-six halftone pictures of animals, as well as views of their habitats, are uniformly excellent, an agreeable step forward. Eight color plates, three of birds and five of mammals, by Allan Brooks, are beautiful additions that will assist both the novice and the professional zoologist. On the three bird plates are excellent representations of seventeen different kinds of small birds that should go far in assisting all observers to distinguish between some forms that sometimes appear to be very similar.

The section headed references lists not only published works that are standard references of zoologists but also memoranda and field notes, and manuscript reports of the National Park Service employees, which are not generally known to exist. The citation of these not only serves to verify the statements in the work but will focus the attention of future workers in this and other areas on the fund of information that exists in certain administrative files.

The index is limited to vernacular names, which are probably so sufficiently well known that the omission of the scientific names will not be a handicap in using the book.

When I was invited to review the book I was pleased because I had been well acquainted with Dixon and knew that his long, careful sympathetic study of the mammals of California would bring much of value to any of his writings. Furthermore, while I have not had the pleasure of being well acquainted with Lowell

Sumner, the author of the bird section, I have heard of him so favorably that I was certain his portion would be a valuable contribution.

In addition to the basic parts played by the two authors of this work, the book has had the benefit of the careful scrutiny and advice of wildlife administrators and zoologists of the National Park Service and the Fish and Wildlife Service, as well as the many capable California zoologists with whom the authors were associated for many years.

By the time I had finished with the examination of the book for this review, I was delighted. The work is not only well done but it is made understandable and interesting to the layman, and it carries educational and conservation values far beyond those found in most treatments of local animals.

Ernest P. Walker
U. S. National Zoological Park
Washington, D. C.

The Birds of Mexico. Emmet R. Blake. xxix + 644 pp. Illus. $6.00. University of Chicago Press, Chicago, Ill. 1953.

THE increasing travel to Mexico by tourists interested in birds has emphasized the need for a compact, accurate, and complete book on the rich avifauna of that country, so written that the informed amateur bird student could use it readily to enable him to identify the native birds. Blake's book is the answer to this need, and it gives every promise of being a good and usable answer. In the space of a volume small enough to be carried conveniently in one's baggage, the author has treated of the nearly one thousand species and more than twice that number of subspecies of birds recorded from Mexico. It includes not only the resident birds, but also those that migrate there from the United States and Canada. Approximately a third of the species are figured in black-and-white pen sketches, while convenient keys enable the reader to distinguish related forms. Each species is briefly described in the text, and the range of each, and even of each race of each species, is tersely stated.

This is a manual for identification, and it wisely does not attempt to go into such matters as habits, nests, or eggs. To do this would have so greatly increased its bulk as to make it a library rather than a field book. Because of the extent of the fauna it covers, the book should prove useful to travelers throughout Central America, although there are many forms of birds in the other Central American republics not included in the Mexican list. They are, however, related closely to those treated in this volume.

The book is well printed on good paper, with clear type, and is well indexed and bound. It may be recommended to all bird-minded travelers.

Herbert Friedmann
Department of Zoology
U. S. National Museum
Washington, D. C.

SEA NOTES

Waves and Tides. R. C. H. Russell and D. H. Macmillan. 348 pp. Illus. + plates. $6.00. Philosophical Library, New York. 1953.

LAY readers have found it difficult to find popular works on various aspects of oceanography which contain much more than a poetic appreciation and sensational treatment of "the mysteries of the deep." For those who want a sober, accurate, elementary presentation of the known facts and ideas about waves and tides, the work by Russell and Macmillan will come as a very welcome surprise.

In recent years waves have received more attention from research workers than tides, and for this reason the first part of the book by Russell is interesting reading and covers material not included in Cornish's books. A number of popular works on tides have been written in the past, for example, those by G. H. Darwin and Marmer, and more recently the Admiralty Manual of Tides by Dodson. The second part of the book on tides by Macmillan covers much the same material as these works but in less detail and in a slightly more popular fashion.

The book can be safely recommended as a gift to mariners and yachtsmen, the audience for which it is evidently intended. It is not likely to be of much interest, of course, to the professional physicist or geophysicist.

Henry Stommel
Oceanographic Laboratory
Woods Hole, Massachusetts

Fishery Science: Its Methods and Applications. George A. Rounsefell and W. Harry Everhart. xii + 444 pp. Illus. $7.50. Wiley, New York. 1953.

IN spite of the great flood of papers and bulletins on fisheries, their problems and procedures, this is the first attempt to prepare an introductory textbook of the subject. It is, according to the authors, a treatise on methods, and "the real purpose is to present the problems that confront the administrator, the research worker, and the student, and to show how to go about solving them." It is assumed apparently, that the user of this text understands basic ecological principles and elementary statistics. Although the book is in many ways an elementary text, it is in other ways an advanced textbook because of this assumption that the readers possess enough background information to follow the rather brief presentations of some of the complex concepts of fisheries and to recognize omissions where they occur.

The authors do not get around to "problems" until the final chapter; the bulk of the book is given over to methods of investigation and analysis of data, appliances, fishways, gear, and stocking policies. It might have been better to state the problems first and then discuss the techniques used in studying them. Because of this obvious concern with means, this book cannot stand by itself as a textbook of fisheries although the

publishers claim it to be a "complete text for all phases of fishery science." Some topics are sketchily treated, and a few are omitted altogether. The most conspicuous omission is that of any discussion of salmon hatcheries although there is a chapter on "salmonids." The authors may well have felt it best to avoid this thermonuclear potato, but the compilers of a text must occasionally venture beyond the angels if they would strive for comprehensive coverage.

As a first approximation, however, this book provides a useful summary of the methods and devices at the disposal of the fishery worker, whether fresh water or marine, and fills a longfelt need for a concise presentation of widely scattered materials. Much more is included than omitted, and its use as a textbook in fisheries methods should soon provide the authors with new material for the improvement of subsequent editions.

JOEL W. HEDGPETH
Scripps Institution of Oceanography
La Jolla, California

Between the Tides. Philip Street. 175 pp. Illus. + plates. $4.75. Philosophical Library, New York. (Printed and bound in Great Britain.) 1953.

IT is fair that a book be judged in terms of its objectives. Those of *Between the Tides* are clearly stated by its author: "This book is intended primarily for the young naturalist. It introduces him to the rich variety of life on the seashore in the hope that it may stimulate him to further study with the aid of more complete guides written for the adult naturalist. The reader I have had in mind . . . is of no particular age [but] usually he will be in his 'teens' . . . " This is a good objective, fairly stated, and on the whole well carried out. *Between the Tides* deals with the common and obvious animals (and seaweeds) of the seashores of Great Britain. Common names are used throughout, plus scientific names in picture captions. In addition a glossary of vernacular and scientific names is given in an appendix. There is an adequate index. The language is nontechnical, and this reviewer found the style interesting, as did his ten-year-old son. The book is illustrated by a large number of good black-and-white photographs, supplemented by line drawings. The text describes the natural history of the forms covered. The illustrations are said to be adequate for most identifications (the reviewer, being unacquainted with the British marine fauna, cannot judge the latter point).

This book is suitable for boys and girls in their early teens, and many parents visiting the seashore would find the book interesting and useful. It is, of course, much less likely to be useful in this country than in Britain, since it deals with a local fauna. The work is not intended for the adult biologist, nor even for the informed amateur naturalist—many other well-known books on the sea shore and its fauna are available both in this country and in Britain. Mr. Street's book is intended to fill the evident gap between such adult books and the large number of "children's books" on the subject. Without claiming great originality, he has done a good job. Books of this type are needed for the coasts of the United States.

By way of criticism: although the book is to be commended on its photographs, it should be noted that the line drawings are poor. There are a few errors of fact and a good many over simplifications, the latter in the main justifiable in a work of this sort. The price of $4.75 seems decidedly high for a book of this size—presumably in Britain where most of the sale must be expected it would cost less. But, all in all, a good book.

RALPH I. SMITH
Department of Zoology
University of California

ABORIGINAL AMERICA

Archeology of the Eastern United States. Edited by James B. Griffin. x + 392 pp. 205 Figs. $10.00. University of Chicago Press, Chicago. 1952.

THIS comprehensive testimonial volume was prepared in honor of Fay-Cooper Cole, Chairman Emeritus of the Department of Anthropology at the University of Chicago, by twenty-six of his former students. There is an introductory essay on "Twenty-five Years of Archeology in the Eastern United States" by Carl E. Guthe, long a professional colleague of Dr. Cole's.

The work contains, in addition to Dr. Guthe's contribution, twenty-eight articles, all save six devoted to the summary treatment of archeological areas east of the Mississippi River. These exceptions comprise a classification of Indian racial varieties of North America by Georg K. Neumann, a consideration of ethnological correlations with archeological remains by Fred R. Eggan, a survey of historic site excavations in the United States by Jean C. Harrington, an evaluation of dendrochronological studies in the Mississippi Valley prepared by Robert E. Bell, a concise synthesis of eastern United States culture periods, and an appendix summarizing the radiocarbon dates for the same area, the last two by the editor.

The numerous illustrations, based on photographs and drawings, were prepared by several persons, some of whom are mentioned in the preface, and they range in quality from good to fair. For certain areas, due no doubt, at least in part, to spatial limitations, the illustrations only partially cover the major trait characteristics of the cultures described.

As stated in the preface, "Unfortunately, the chapter which would have discussed the geographical background of this broad area and its influence on the prehistoric cultural development was not submitted, nor was the section on the Central Plains completed."

About two-thirds of the articles were received by the editor in 1947, all but three of the remainder in 1948. Only two were revised to 1951. Accordingly they are not equally up-to-date. Moreover, they are of unequal value, owing to the additional fact that for some areas the data were drawn largely from older, and often

inadequate, published sources. For the most part they do incorporate the results of the newer field work published or otherwise available at the time of preparation of the individual reports. Editorial footnotes frequently comment on changing opinions and discoveries up to 1951.

From the viewpoint of the professional student, who will, in general, find this book of value as a summary, what appear to be numerous instances of overediting will seem regrettable. For the nonprofessional it may offer, for the most part, difficult reading, and the illustrations may prove insufficient for his purposes.

WILLIAM A. RITCHIE

New York State Museum
Albany, New York

TOURING THE MOON

The Moon. George Gamow. 118 pp. Illus. + plates. $2.50. Schuman, New York. 1953.

A Guide to the Moon. Patrick Moore. 255 pp. Illus. + plates. $3.95. Norton, New York. 1953.

YOU should keep from the young the two books before me for review, and from their elders also, unless you are willing to take the rather expensive consequences. Telescopes suitable for exploring the surface details of the moon can cost from two or three hundred up to many thousands of dollars. The impulse to stride over the plains and climb the steep hills and steeper jagged mountains of the moon will be very strong after an acquaintance with these new rocket-age volumes, which are preparing earth-bound man for the shaking off of his terrestrial shackles.

Dr. Gamow's little book is aimed at the beginner's level and is filled with those clear explanations of technicalities and the graphic drawings and word pictures for which the author is well known through his recent popular books on the origins, ages, and destinies of the earth, sun, stars, and of the total universe. As a distinguished theoretical physicist, he writes on the moon, you might say, from the outside—outside the moon and the lunar field. Patrick Moore, on the other hand, writes from the inside; he writes for the mature and ambitious reader, and still does it so simply and smoothly that the reader can start with no scientific knowledge whatever and come out as something of a lunar expert. With untechnical thoroughness he discusses the problems of the lunar atmosphere, of rockets to the moon, of the origin of the craters, and of the establishment of a lunar colony. He has produced an extraordinarily good book which cannot fail to arouse enthusiasm for knowing more about this tantalizing nearby planetary pal of the earth. Moore's specialties include general astronomy, interplanetary travel, lunar research, and the hope for freedom from idiotic wars that politicians thrive on but which deter man's intellectual adventures. He takes it for granted that before long man will fly around the moon (Gamow agrees), and later actually wander over the moon's spectacular surface. His book is rightly a guide to the hundreds of named special features on the moon, and equally a guide to the best knowledge of the past and present about our satellite.

The earth's isolation from its companions in the solar system began to end, as Moore points out, on March 16, 1926. That was the date when the American physicist and rocketeer, Robert Goddard, pioneering in the field, fired his first effective shot. From then on it was clear that space flight, beyond the atmosphere, was practical. With maps, photographs, and detailed indices, Moore is preparing the lunar traveler for the future landings. Another goal of his volume, which he attains in part, is to spell out for us what obstacles we must overcome in the early days of our interplanetary operations.

Three lunar characteristics, heretofore not generally accepted in the astronomical textbooks, are presented by Moore with convincing argument based on competent observation. The first is that the moon is not wholly dead and changeless. Within the past century or two several visible topographic changes have occurred, the result perhaps of landslides, meteor impacts, seismic shock, or vestigial volcanic activity. The second is that a thin atmosphere exists on the moon, at least fleetingly from time to time. This atmosphere occasionally dims or hides the surface features in certain craters and clefts. The atmosphere is of course not hydrogen or oxygen or other light elements, since the moon's gravitational power could not hold such material, especially when heated by the 214° Fahrenheit sunlight of midday; but the atmosphere may be sufficient to protect future hypothetical human colonies from infalling meteors.

The present reviewer has proposed that a continuously regenerated atmosphere of argon gas must exist on the moon as a result of the radioactivity of isotope 40 of the undoubtedly prevalent element potassium in the lunar rocks. The argon combines with nothing, is fairly heavy, and would not easily escape; but it will not be useful to the lunar expeditionary forces except perhaps in helping provide protection from small meteors.

The third point made by Moore is really only the lending of weight, based on observation and careful physiographic analysis, to the hypothesis that almost all the craters of the moon are attributable to volcanic action and not to meteoric infall.

HARLOW SHAPLEY

Harvard University

THINKING MAN

The Common Sense of Science. J. Bronowski. 154 pp. $2.00. Harvard University Press, Cambridge. 1953.

THIS book is the story of the evolution of science as a part of the cultural history of western civilization. The author believes that the modern concept of science as something separate and distinct from the arts is false and that it is essential to correct this misunderstanding. To achieve a common understanding,

the ideas and methods of science must be expressed in a language understandable to both scientist and non-scientist.

The author traces the history of science from Aristotle to modern times as the history of three dominant ideas. These are the idea of order, the idea of causes, and the idea of chance. The author begins with the idea of order and shows how the Scientific Revolution of the 17th century changed the concept of the universe from a purposeful, willful universe to a strictly deterministic machine in which every effect follows automatically from a specific cause. The discovery of quanta and the principle of uncertainty in turn changed this idea to one of a statistical universe in which chance plays a significant role.

The methods of science are pictured as being an outgrowth not only of a common behavior pattern of man, but also of a common pattern of adaptation of all living things to future probabilities. The mutations of organisms are unconsciously produced solutions to the organisms' problems of adaptation. They constitute a prediction of the future environmental conditions that these organisms will face. The empirical test follows and the organism lives or dies depending on how well it predicted the future. Common sense behavior of man follows the same pattern. His ideas are predictions that guide his behavior. If he predicts the future correctly he survives and prospers, if not he suffers and may die. The theories, laws, and hypotheses of science are similar though more refined and systematized predictions of the future. The concept of uncertainty enters because no prediction can ever be made with absolute certainty. Statistical probability is as close as one can ever come to certainty.

The significance of value to science is illustrated by the relation of the value of truth to scientific endeavor. Throughout the book the close relationship between the growth of science and the cultural environment of the time is stressed. This is an important book and deserves to be widely read.

C. A. Lawson

Department of Natural Science
Michigan State College

Readings in Philosophy of Science. Introduction to the foundations and cultural aspects of the sciences. Arranged and edited by Philip P. Wiener. ix + 645 pp. $5.50. Scribner's, New York. 1953.

"PHILOSOPHY in the full sense," wrote William James in one of these readings, " is only *man thinking.*" This volume includes a selection of what some philosophers, some scientists, and some others, have thought modernly about science. The thinkers represented include not only contemporary writers, whose excerpts constitute the major part of the book, but also such modern philosophers, to mention only a few, as William James, John Stuart Mill, Henri Poincaré, Darwin, and Aristotle. Philosophy of science is unfortunately often subject to unsatisfactorily superficial treatment by scientists who naively presume to have settled philosophical problems whose very existence they but dimly even recognize; it is the virtue of the editor that he has succeeded admirably in choosing authors who are qualified in their fields and who express themselves lucidly.

The contents of the book are grouped around four main subjects: mathematics and physical science, biological and psychological concepts, methods and problems of social science, and philosophical analyses and syntheses. It is itself an illuminating commentary on the thought of our times that the longest of the four sections is that dealing with social science. The selections have been deliberately chosen at various levels of specialization for the benefit of readers with differing backgrounds.

The selections consist in general of full chapters or articles or of reasonably long excerpts, so that as far as possible coherent rather than fragmentary concepts are expressed for each author quoted. Mere chapters of a man's thought are not always enough to achieve full success in this respect, and many of the quotations stop in the middle of an idea. These, however, present a lively challenge to the reader to pursue further the interrupted idea, and the important fact remains that they actually do contain many ideas. That is a great deal to say of any book, and this particular one is enthusiastically recommended for its able and agreeable presentation of the ideas man is thinking about science.

Jane Oppenheimer

Department of Biology
Bryn Mawr College

THOMISTIC PHILOSOPHY

The Philosophy of Human Nature. George P. Klubertanz. xiii + 444 pp. $3.50. Appleton-Century-Crofts, New York. 1953.

THIS volume by an assistant professor of philosophy at Saint Louis University is a text designed primarily for Catholic students of philosophy. The title of the book is rather misleading, since the author's avowed objective is to present "a Thomistic philosophy of human nature" in which St. Thomas' thought on man is developed systematically in a modern setting. "In the entire course of this argument, practically no notice is taken of other systems" (p. 10).

The book is in two parts. The first part, or text, presents a systematic, clear analysis of classic topics of Thomistic psychology, such as, the unity of man, vegetative life and the soul, activity and power, knowledge, sensation, the intellect, the will, the sensory appetites, habits, and the origin and destiny of the human soul. The results of each chapter are summed up in definitions and demonstrated by proofs in syllogistic form. A concluding chapter (xiv) presents a systematic summary of the entire discussion of human nature, supplementing "the way of discovery" of the text with "the way of judgment." Thus, within the compass of a few pages (349–52) the student is presented with an

unequivocal doctrine of the origin of the soul and syllogistic proof of its immortality and divine origin.

The second part of the book (357–436) contains two groups of appendices comprising brief analyses of opposing philosophical systems and discussions of topics not directly part of the course. Approximately two pages are devoted to each of the following subjects: dualism (the Platonic man); idealistic monism (the spirit man); materialistic monism (the mechanical man); positivism (the unsubstantial man); sensism (the animal man); philosophical Freudianism (instinctive man); philosophical evolutionism (the evolving animal); determinism (the predetermined man). There is no discussion of historical idealism or of historical materialism (Marxism).

One of the serious limitations of the book is its almost total preoccupation with classic problems of Thomism. There is no reference to the literature of modern anthropology and culture history, although Catholic ethnologists, such as Fathers Wilhelm Schmidt and Wilhelm Koppers, are keenly aware of the direct relevance of this subject for any discussion of the philosophy of human nature.

I do think, however, that the author has rendered a valuable service to American philosophical scholarship by his clear and systematic presentation of the Thomistic point of view. With the realization that the concept of man constitutes a common ground for students of philosophy and the social sciences, will come a more fruitful approach to the study and resolution of our human problems.

DAVID BIDNEY

Department of Anthropology
Indiana University

MEASURES OF MEN

Historical Metrology. A. E. Berriman. xvi + 224 pp. Illus. $3.75. Dutton, New York. 1953.

HERE we find an engineer exploring the libraries, museums, galleries, and other sites of culture, gathering information on the rich heritages of civilizations and methodically coordinating it into a basic foundation of metrology. He appreciates the true significance of each archaeological item which must fit in the picture.

First, he relates the Greek foot measure to 1/100 second of arc of a great circle of the earth, thus harmonizing the sexagesimal system. Next comes the English acre ". . . the most intriguing of ancient measures . . . virtually equal to the myriad millionth of the square of the terrestrial radius. . . ." Standards are discovered which are based upon the density of water and of gold, each of geodetic origin.

Three constants are called upon to harmonize the values of many ancient measures; $k = 1.296 = 1/10000$ of $360° \times 60' \times 60''$, π and $\sqrt{2}$ the chord-length ratio of the quadrant of a circle.

From recent precision measurements of ancient structures (the Greek Parthenon and many others), Berriman has recreated units and compared them with standards, with surprising coincidences.

More striking is the series of comparisons drawn from the English acre, which in circular dimension can be inscribed by the square Scottish acre, which, similarly expressed, can be inscribed by the Irish acre. (This Irish circular acre has a diameter near that of the earthwork circle at Stonehenge.) A square enclosing this circle will contain 10,000 square yards and is probably equal to the ancient Hindu land unit, nivartana. From the amounts of seed sown per unit of area he confirms the related units as in accord with good practice. The methods employed "make the evidence speak," the ideal of all expert investigators.

His reference to Mauton of Lyons, who in 1670 proposed a geodetic minute of arc decimally divided, makes it necessary to reappraise the idea of the metric system as being an invention of the 18th century.

Many scientists will find lore and facts pertaining to their specialty. Historians, antiquaries, appraisers, and serious numerologists will find enjoyment in studying this volume containing a world-wide coverage of ancient metrology. Numerous cuts, drawings, and tables present the material in a realistic manner.

Answer to the proposed question, "Was the earth measured in remote antiquity?" seems to be "Probably."

WILMER SOUDER

3503 Morrison Street, N.W.
Washington, D. C.

A FRIEND OF BOLIVAR

BOUSSINGAULT. Juicio critico del eminente agronomico del siglo XIX, Su viaje a la gran Colombia y sus relaciones con El Libertador y Manuelita Saenz. (Critique of the eminent agronomist of the 19th century, his journey to greater Colombia and his relations with the Liberator and Manuelita Saenz.) Carlos E. Chardon. 109 pp. Plate. Editora Montalvo, Ciudad Trujillo, Republica Dominicana. 1953.

THIS is a lively and entertaining introduction to a French scientist who has long been shrouded in anonymity. The apt, perhaps too brief, passages quoted from Boussingault's own writings give it flavor, and reveal an inquisitive, humorous, very personable young man and the life he led in 19th century South America.

The warm chronicle of the man is presented largely through appropriate selections from his little known *Memoirs*, published in a very rare limited edition in 1892. The Spanish text consists of four principal parts: The Prologue (pp. 7–18) treats of Boussingault's life up to the age of 20 when he left for Venezuela with a letter of introduction from his friend Humboldt to Simon Bolívar; The Journey (pp. 19–60) is an account of his travels through Venezuela, Colombia, and Ecuador with 13 brief and enjoyable sketches from the *Memoirs*; "Europa" (pp. 61–87) describes Boussingault's return to France after 10 years in South Amer-

ica, records his activities until his death in 1887, and adds a critical review of his scientific studies; "Boussingault, el Libertador y Manuelita Saenz" (pp. 89–109) completes the series with an interesting historical sidelight which reveals Boussingault's familiarity with General Bolívar and his *cara amica,* Manuelita. The anecdotes provide intimate light upon a situation which Spanish-American historians have held incompatible with the memory of the George Washington of South America, and have depreciated as showing no respect for Bolívar. Boussingault's Manuelita, however, is less restrained; he portrays her as an "extraordinary," "beautiful," "irrepressible," and "eccentric" *gran señora.*

Chardon's study is not an abridgment of the five-volume *Memoirs.* It is a series of carefully selected passages, judiciously set into the author's narrative and annotated with 58 footnotes to other references and historical notes. Chardon gives an analysis of Boussingault's 213 contributions to fields of science, based on the bibliography cited by Alfred Lacroix in *Notice historique sur Jean-Baptiste Boussingault,* Paris, 1926. The task of selecting the passages and of combining them with the commentaries is well done, with a scholar's patient attention to detail. The result justifies the author's determination to let Boussingault speak for himself. The volume merits translating into English for it provides excellent and instructive reading especially at this time when North Americans are seeking so much in common with Latin-Americans.

George A. Llano

Research Studies Institute, USAF
Air University, Maxwell AFB, Alabama

NATURE AND NURTURE

Free and Unequal. The biological basis of individual liberty. Roger J. Williams. xiii + 177 pp. Illus. $3.50. University of Texas Press, Austin. 1953.

THIS book is a vigorous presentation of the fact of genetic variability in man and of the serious consequences for human relationships that follow a neglect of it. We can be equally free, the author points out, without being uniform. There is a deep conflict between this idea and the one tacitly assumed by many educators, sociologists, and medical men—that all people are born fundamentally alike and that the differences they later display are the result of the environment in which they are reared. This ancient controversy between the advocates of "nature" and "nurture" is still very real, says the author, and underlies the chief ideological conflict in the world today. If there were no inborn distinctions between human beings, the importance of the individual would vanish and freedom would lose its significance.

The author presents evidence for the existence of a "distinctive and complex pattern of inborn mental capacities" in every individual which is unique for the individual. It is not *man* and his common traits which finally are important, but *men* and the great diversities they display.

Education, he believes, is too often an "assembly line" process which endeavors to *mold* people into a social pattern rather than *fit* them into it by developing their particular capacities. Physicians are apt to generalize about "the patient," whereas each patient is an individual problem. To attempt to treat all workmen alike is also fruitless for they are different, not only in productivity but in many other ways. The radical differences in aesthetic tastes and religious beliefs have a biological basis and should not be the cause of violent disagreements. Harmony is much more likely to come from a friendly recognition of differences than from attempts to make everyone think and feel alike. Disagreement is natural and healthy. We should, of course, oppose discrimination on such grounds as race and religion, but let us not forget that there are real differences among people and that these must be recognized if we are to live happily together.

With the author's viewpoint one must be sympathetic. It is well to have the case for "nature" so strongly stated. The uniformity and regimentation of totalitarianism we abhor; but too great emphasis on inherited differences can lead to racism and Hitler. We should remember that what is inherited is a series of *capacities,* not of traits; a repertoire and not a fixed program. How these capacities are realized depends on training. Man is enormously educable. Our attitudes, motives, and tastes are determined largely by the kind of environmen in which we live. To gain the highest satisfactions of life and to establish the Good Society we must recognize not only human differences but the importance of guiding these differences into the best channels.

Edmund W. Sinnott

Graduate School
Yale University

LETTERS

MATHEMATICS AND THE EDUCATIONAL OCTOPUS

THANK you for presenting at last a discussion of educational policies in which an effort at least has been made to approach the subject with a proper thoughtfulness. It is too bad that Cairns was not able to sustain his desire to illuminate rather than to inflame throughout his paper entitled "Mathematics and the Educational Octopus" in your April number. Perhaps the selection of a pejorative term such as "octopus" in the title itself indicated that it would not be possible for the author to avoid acrimony.

The most regrettable feature of this attitude is that he has been betrayed, perhaps unwittingly, into the use of debater's tactics in brushing off the weightiest piece of opposing evidence as if it were of no importance. Now perhaps it really is not important. But we shall never be able to judge from the cavalier treatment accorded it by Cairns.

His dismissal is so casual that little effort is involved in quoting it in full:

It is interesting also to glance briefly at the so-called Eight-Year Study, used by many educationists as a justification for experimentation. Although it purported to be a controlled statistical study, Vol. I of the report, *The Story of the Eight-Year Study,* states: "Everyone invited to serve on the Commission was known to be concerned with the revision of the work of the secondary school and eager to find some way to remove the obstacle of rigid college prescription." Little wonder that this report as a whole is a gold mine for those seeking samples of statistical fallacies!

Boiled down, this paragraph tells us what?

(1) The people who issued the report admitted their antecedent bias in favor of the conclusions reached;

(2) A mathematician, Cairns, has alleged that there are many statistical fallacies in the report.

Do these items establish that the conclusions of the report were incorrect? Who can say, when Cairns' brief glance is so brief that he does not even tell what the conclusions were except by indicating that they were "justification for experimentation" of some unspecified nature?

If, Sirs, you are concerned with dealing a body blow at Cairns' alleged octopus, then I can think of nothing which would carry more weight than a full and convincing analysis of the reasons for discrediting the major conclusions of the *Eight-Year Study.*

If you wish merely to amuse us with glib ironies, then I should think it would be fun to turn a witty psychiatrist loose on the question of why it is that both sides of what Cairns evidently wishes to view as a "battle" have a tendency to suspect the perfidious opposition of concocting foul plots for the overthrow of all that is good, beautiful, and true—including, of course, themselves.

If you are, by any chance, concerned with serious inquiry into the areas of our ignorance about the learning process, then I should like to suggest a few questions to chew on:

(1) Cairns asks, "Why is it that they should not be obliged to master such useful tidbits as the multiplication tables?" I should like to ask why is it that the conservatives in education never seem to worry about inculcating the addition tables by rote, even though they contain just as many combinations of numbers as the multiplication tables? This is a serious question, not a sarcastic one; for I have noted that it is apparently true that addition is learned more readily than multiplication; and I am curious as to why this should be so, if it is so, as I suspect an understanding of what is going on might be very helpful in teaching arithmetic.

(2) Cairns states that "In English classes, the parts of speech are either not taught at all or are supposed to be picked up in some incidental manner—perhaps after ignorance of them has hampered students in foreign language courses." May we infer from this statement that ignorance of "the parts of speech" hampers students *only* in foreign language courses, and *not* in the use of their native tongue, save perhaps for an awkward, but possibly not grave, embarrassment when one of the semi-literate devises some such locution as "between you and I"? If so, does this condition indicate a fundamental distinction between English and the foreign languages conventionally studied in college? Or does it perhaps indicate that the conventional methods of instruction in the colleges are a decade out of date? These are serious, not sarcastic, inquiries.

(3) Cairns exclaims, "Why we should shun the development of memory as a tool is a mystery difficult to fathom!" Avoiding the invidious assumption that the concept of "development of memory" may be suggested by a half-remembered echo of the long-abandoned belief in "formal discipline," let us assume that we have here an expression of a genuine interest in learning how to make our memories more effective. Let us, then, consider for a moment the implications of a rather striking little demonstration that is popular in elementary courses in psychology. The instructor reads in a monotone a group of syllables such as:

blee — wang — flate — jy — muke — drit — yoll — zux — lape — strell.

The class is then asked to write down what they can remember. No one remembers all of it. Most recall one or two.

Again the instructor reads a group of syllables in a monotone:

tree — grass — wind — leaf — snow — hail — rain — sun — fire — cloud.

Again the class is asked to write. A gifted few may remember all or nearly all the syllables. Most get several of them. Thus the importance of familiarity—the results of "tedious drill" may be estimated; actually,

of course, the "drill" that led to this result may not have been "tedious," because it may have been the by-product of that dubious entity, "life experience."

Again the instructor reads in a monotone:
two — big — planes — came — down — fast — to — the — new — field.

Everyone busily writes; and this time nearly everyone is perfect. What has happened? It is easy enough to say that "meaning" has been demonstrated to be a more powerful aid to memory than years of drill. But *why* is it so? And *how* are we to go about sponsoring experiences which will use this power, if, as Cairns suggests, we are to shun the "grocery-store concept of mathematics"?

A serious consideration of such questions might, I should hope, lead critics like Cairns, who are trying to be objective, to see that if we are to make any real headway in learning how people can learn, not less, but more—an end devoutly wished by all of us—we ought not to hamstring our inquiry by starting out with the assumption that we can identify by inspection certain "essential elements of our intellectual heritage," the values of which must be accepted as an axiomatic without further questioning.

WILLIAM H. NEWBERRY

Alton, Illinois

NIGHT OR DAY

REGARDING the interesting article by Nathaniel Kleitman and Hortense Kleitman entitled *The Sleep-Wakefulness Pattern in the Arctic* which appeared in THE SCIENTIFIC MONTHLY, 6, 349 (1953), there is a general misconception about arctic winters which the Kleitmans apparently have accepted and which, I feel, has certain significance in this study. The article states that at Tromsø, Norway (69° 39′ N) "daylight . . . decreases (after the autumnal equinox) down to nothing by November 21 when a period of complete darkness begins" (p. 350, 2nd par., 6th line).

This popular notion that all people living north of the Arctic Circle spend a prolonged period each winter in total darkness harks back to our primary school geography lessons and is erroneous. Although I have no figures on the light during the winter at Tromsø, I have some for Point Barrow on the northern tip of Alaska and more than 100 miles north of the latitude of Tromsø. Although the sun does not appear above the horizon for about two months in the winter, there are several hours of good daylight each day.

The following are representative figures copied from a daily light table issued by the Navy's Aerological Project at Pt. Barrow for December 1952. The exact criteria used for such things as "usable light" (described in the table as light sufficient to make "objects definable at approximately 100 yards") and "beginning" and "end of twilight" are a little vague. The table was drawn up as an aid in planning outside construction and maintenance work in the Pt. Barrow area. During the period of "usable light," no artificial illumination was required for outside activities.

Date	Begin twilight	Begin usable light	End usable light	End twilight	Hr–Min usable light
1	0659	0913	1520	1742	6–07
11	0707	0932	1510	1732	5–38
21*	0716	0940	1509	1734	5–29
31	0716	0940	1520	1744	5–40

* Shortest day of year.

Even during the middle of December the light at noon in Pt. Barrow is quite similar to that at noon on a dull dreary day in, for example, New York City at the same season. It should be kept in mind that a snow-covered landscape such as is found in northern Alaska and, I presume, Tromsø, brightens the scene considerably.

In Figure 1B in the Kleitmans' article (the graph showing the getting-up time at Tromsø) the line reaches a winter maximum at about 7:30 A.M. At this hour in northern Alaska during December, there is twilight. It is, to be sure, a very dim twilight which progresses very slowly. But is it not true that most of us even in the latitude of New England are accustomed to arising during or even before twilight in the winter? In Canada and the British Isles this is even more the case.

It is not too surprising that the winter going-to-bed and getting-up times of the Tromsø inhabitants are practically the same as those of inhabitants of similar-sized cities in the northern temperate zone (taking into account that the lights in Tromsø go out at 9:00 P.M.), since the situations are not very different. In both places, the people go to bed in the winter long after dark and get up either before or during morning twilight.

If Tromsø did, indeed, have a prolonged period of total darkness, then any differences or similarities between the habits of Tromsø inhabitants and the inhabitants of similar-sized cities in the northern part of the temperate zone might take on real significance.

In review, the Kleitmans, in studying the sleep-wakefulness pattern of the people of Tromsø, have apparently been misled into believing that those people live in "complete darkness" for about two months each winter. Although the amounts of daylight in summer and in winter differ to a huge degree at 69½° of latitude, which in itself makes for an interesting study, the authors misstate the case when they say that "continuous darkness prevails in the winter" (p. 356, last par., 3rd line).

WILLIAM G. METCALF

Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

ON ANIMISTIC THINKING

THE report of Wayne Dennis' survey of animistic thinking among the educated [THE SCIENTIFIC MONTHLY, 76, (4), 247 (1953)], shows with startling clarity how little we know what others think and how prone we are to assume, without justification, that our

very elementary concepts are accepted by nearly all others with equivalent education or experience.

But I am not convinced that such a high percentage of educated adults, if asked fairly, could fail to answer correctly such elementary questions. A study of the instructions used and some experiments with friends suggest that their very sophistication betrays the subjects.

The connotation from these instructions is that some of the questions are not at all simple and that some are hard. The subjects strive to find an answer that is not obvious, for on the surface, they are all simple and none is hard, and so striving, they fall into an unintentional trap.

I believe that if in substance the instructions were, "I am going to ask some simple questions. I want a yes or no answer to a literal question," the results would be different. The significant word is "literal." If these tests do not confirm Mr. Dennis' first findings, his work will still be valuable, for it will show how easy it is to invalidate a test by apparently unimportant details connected with its administration.

I think your readers would like to learn if further tests confirm the low level of educational content among our educated indicated by the first survey.

W. J. Baëza

Industrial Research Company
St. Petersburg, Florida

Dr. Baëza's suggestion that it be made clear, that what is desired in response to questions concerning the animation of objects is a literal answer, is an excellent one. I hope to be able to make use of this suggestion in the near future. However, as I indicated in my original paper, the majority of the answers I have received indicate that the questions have been accepted literally.

So far as the responses of Dr Baëza's friends are concerned, it should be noted that research scientists and their friends should not be confused with the persons whom I questioned, who were public school teachers and average college students. On the whole there exist between these classes of persons great differences in ability, as well as differences in scientific training.

Wayne Dennis

Department of Psychology
Brooklyn College

THE GEOMETRIC WISDOM OF SOLOMON

Gridgeman in his *Circumetrics,* The Scientific Monthly, 77, 31 (1953), has underestimated Solomon. Since this wise king probably knew that π was transcendental, he was smart enough to round off the dimensions in II Chronicles IV: 2 to the nearest significant figures, even as statisticians do now. By accepting the inerrancy of the Scriptures and using the correct value of π, it follows that the circumference of Solomon's molten sea in cubits was between 29.85 and 30.50, and the diameter between 9.50 and 9.71, or 30 and 10, respectively, to the nearest even cubit.

Carl S. Wise

607 Bigelow Street
Peoria, Illinois

ERRATUM

In the article "Circumetrics" by N. T. Gridgeman that appeared on page 31, The Scientific Monthly, July, 1953, there is an error in the equation on the fourth line, second column, of page 32. The equation should be: "$\arctan x = x - x^3/3 + x^5/5 - x^7/7 + \cdots$" and not: "$\pi/4 = 1/(2 + 3^2/(2 + 5^2/(2 + 7^2/(2 + 9^2 \cdots$" as printed.

N. T. Gridgeman

Division of Applied Biology
National Research Council
Ottawa, Canada

A NEW AAAS EMBLEM

The Board of Directors of the Association has approved the design reproduced at the left as a symbol of identification with the AAAS. In the future, this design will appear on the symposium volumes and will be used for other appropriate purposes.

The Association will soon make available to its *members* lapel buttons and pins. (Keys will also be provided if a sufficient number of orders for them is received.) The size will be identical with the illustration. The scalloped border and the lettering will be in rolled gold, the background in blue enamel, and the torch in red enamel. The key, if provided, will have the basic design superimposed on a black enamel background with a second rolled gold border.

Information on prices and how to order insignia will be sent to all members and will appear in our journals in the fall.

THE SCIENTIFIC MONTHLY

OCTOBER 1953

The Age of the Universe

D. TER HAAR

Dr. Dirk ter Haar received his education at Leyden University where he studied under the late H. A. Kramers. After a year and a half in Niels Bohr's Institute for Theoretical Physics in Copenhagen, he was a Visiting Professor of Physics at Purdue University, from 1947 to 1950. Since 1950 he has been Lecturer in Theoretical Physics, United College of the University of St. Andrews, Scotland. He is a Fellow of the Royal Society of Edinburgh and of the Royal Astronomical Society. He is currently writing a book giving a critical and historical survey of existing theories on the origin of the solar system.

WHEN we contemplate the universe, the question comes into mind whether the present situation has existed in essentially the same form since time immemorial, or whether at some time in the very remote past conditions were completely different. Quite clearly, this question can never be finally settled, since we can only draw conclusions about the past by studying the present and by drawing inferences from it. Since these inferences will be based on the laws of nature as we believe them to be correct at this moment, it follows that any change in the accepted laws of nature will involve a change in the conclusions which we draw regarding the past. We thus see that none of the results that will be discussed in the present paper can be treated as presenting us with a final answer to some of our questions, but rather as giving a possible clue to such an answer. It is, therefore, all the more surprising—and perhaps satisfying—that a large number of independent investigations all seem to lead to the same conclusion, namely, that about three to five billion years ago some fundamental process—perhaps best described as a kind of "catastrophe"—happened, but that during the last one to three billion years the universe has existed in essentially its present condition. This period of the last few billion years we shall call the *present epoch* and its length we shall call the *age of the universe.*

Before we discuss in detail how one can arrive at an estimate of the age of the universe, it is necessary to remind ourselves what we really mean when we use the phrase "age of the universe." The best way to see the meaning of this phrase is perhaps to quote Chandrasekhar.[1] "An important phase of modern astronomical research is concerned with the time scale of the universe, i.e., with the specification of a natural unit of time in which it would be most convenient to describe the changing aspects of the astronomical universe. Stated in this manner, it is apparent that the solution to the problem of the time scale will not permit us (not at any rate in the first instance) either to 'date' the present epoch in a 'fundamental' calendar or to forecast with definiteness the 'end.' What it would allow us, however, is to specify an interval of time in which various aspects of the astronomical universe may be expected to change appreciably. Conversely, the

solution to the problem of the time scale will ultimately depend on the study of a variety of different aspects of the universe and the establishment in each case of a time interval during which the aspect studied might change to an appreciable extent. And if such studies would lead us in most instances to time intervals which are of the same order of magnitude, it would not be unreasonable to attribute to a unit of time of this order of magnitude a fundamental significance. It would appear that this is the only manner in which a rational approach to the problem of the time scale can be made. However, in formulating the problem in this manner it is evident that a certain element of arbitrariness has been introduced into the discussion. But this is unavoidable and inherent in a problem in which the emphasis is on an order of magnitude and not on an absolute measure."

We shall discuss, successively, nine subjects, all of which can give us an indication towards the most likely value of the age of the universe following from data at present available. These subjects are, in order of increasing distance from the earth: (i) the age of the earth, (ii) the age of the moon, (iii) the age of the meteorites, (iv) distribution of stars among spectral classes, (v) distribution in kinetic energy of stars, (vi) distribution in separation of binaries, (vii) dynamics of star clusters, (viii) dynamics of clusters of galaxies, and (ix) the expanding universe. At the end of the discussion we shall briefly summarize the results.

The Age of the Earth

In 1926, during his term as Chairman of the Division of Physical Sciences of the National Research Council of the National Academy of Sciences in Washington, D. C., J. S. Ames appointed a committee to prepare a series of bulletins on the physics of the earth. One of these bulletins, published in 1931,[2] deals with the age of the earth. The summary of the principal results of this volume by Knopf begins with the following paragraph: "At the beginning of the present century the problem of the age of the earth was envisaged as requiring the reconciliation of three independent estimates, all of the same order of magnitude. These estimates were G. H. Darwin's, of 57 million years, based on the separation of the moon from the Earth; Lord Kelvin's, of 20–40 millions, based on the secular cooling of the globe; and Joly's, of 80–90 millions, based on the rate of accumulation of sodium in the world-ocean. To these should be added Helmholtz's estimate of 22 million years, based on the source of the sun's heat and its probable duration."

At that time it appeared that the age of the earth was between 10 and 100 million years and that it might be possible by refining the methods used to narrow down the margin of uncertainty. However, the methods on which the above estimates were based were found to be incorrect, as soon as the importance and implications of radioactive processes were realized. Taking these implications into account, one finds that one must adjust the estimate of the age of the earth upward by a factor as large as 30 to 100. As we shall see, radioactive dating presents us with a far more reliable method for determining the earth's age than any of the older methods.

We realize that during the present century the estimate of the earth's age has increased considerably, and it is perhaps of interest to give a brief sketch of the various estimates of this age in the past. Probably the most remarkable estimate is the one by the ancient Hindus who calculated that the earth was created 1,972,949,054 years ago. It is amazing that this estimate is of the same order of magnitude as the most reliable estimate available at the present.

Western civilization, however, did not have such a generous idea of the magnitude of the earth's past until very recently. Up to the eighteenth century, the earth's age was estimated by taking as literal and scientific truth the Mosaic traditions as laid down in the Pentateuch, so that in the early seventeenth century Archbishop Ussher could state that the world was created in the year 4004 B.C. Scientists expressing doubts as to the scientific reliability of this estimate were considered heretics.

In 1715, the well-known astronomer Halley suggested in a communication to the Royal Society that it might be possible to determine the age of the earth by measuring the salinity or salt content of the oceans. Owing to the influence of the established church, Halley was thinking in terms of thousands of years as the possible age of the earth and expressed regret that the Romans or Greeks had not "delivered down to us the degree of the saltness of the sea, as it was about 2000 years ago; for then it cannot be doubted but that the difference between what is now found and what was then, would become very sensible." He expressed, however, the opinion that "the world may be found much older than many have hitherto imagined."

Later in the eighteenth century James Hutton, often called the father of modern geology, was the first to grasp clearly the magnitude of geological times. In considering the geological data then available, he came to the conclusion that it was impossible to find a "vestige of a beginning." Once the

idea won ground that the geological time scale was extremely large, geologists began to expand their periods. Darwin, for instance, estimated in 1859 the period of the formation of certain geological structures in southern England to be about 300 million years. At present we know that it is more likely to be about 60 million years, but all the same, Jukes, commenting on Darwin's estimate, thought that this period might just as likely have been 30 billion years! By the end of the nineteenth century, however, various methods for evaluating the age of the earth were developed. It was estimated to be between 10 and 100 million years of age.

We now discuss briefly a few methods by which one can arrive at an estimate of the earth's age, and give the results obtained by these methods. We refer to the account given by Holmes,[3] or the recent discussion by Sir Harold Jeffreys,[4] for a more detailed consideration. The methods that we will discuss are: (i) estimating the period necessary for the earth to cool to its present temperature; (ii) estimating the period necessary to produce the present salinity of the oceans; (iii) estimating the period necessary to produce the sedimentary rocks; and (iv) estimating the age of igneous rocks from their lead content.

Cooling of the earth's crust. In 1862, Lord Kelvin considered the problem of estimating the time necessary for the cooling of the earth's crust from its original molten state to its present temperature. He used the fact that the temperature is increasing when we penetrate downward, and from the data available at that time he estimated the age of the earth to be between 20 and 400 million years, an estimate that he revised in 1897 to an age lying between 20 and 40 million years. His estimate was questioned by Perry, who, on the basis of different values for the thermal conductivity of the interior of the earth, arrived at a value of the order of 4 billion years.

Clearly, it was impossible for Kelvin to take radioactivity into account, and this means that his estimate has to be revised. The presence of radioactive materials in the earth's crust means that there is a source of energy that will, of course, retard the cooling process. From the known abundance of these radioactive sources one can arrive at a new estimate of the rate of cooling. In this way Holmes[3] arrives tentatively at an estimate for the age of the earth of between 2 and 4 billion years.

Salinity of the oceans. In 1898, Joly used Halley's suggestion to determine the age of the oceans. Measuring the total amount of sodium carried annually to the seas by the rivers and also the total amount that is present in the oceans, he arrived at an age of about 80 to 90 million years.

In 1942 and 1943, Conway re-examined this question in detail and showed that most of the sodium carried by the rivers is "second hand" sodium, that is, sodium that had previously been deposited by the sea, and that only a fraction of the sodium in the rivers is "new." This fact alone would raise the age of estimate by a factor between 2 and 3, leading to an age of about 150 to 250 million years. As Holmes has pointed out, however, it is very likely that in the past a lesser area was exposed to denudation, so that the rate at which sodium is carried to the seas has increased with time. It hardly needs emphasizing that such a variable timekeeper as the sodium content of the oceans cannot lead to any definite estimate of the age of the earth. Tentatively, Holmes estimates that the slower rate in the past should lead to an increase of the age by a factor between 8 and 27, so that we are finally led to an age of between one and seven billion years.

The formation of sedimentary rocks. The igneous rocks, which are at present exposed so that they can produce sediments, annually produce about one-fifth of a cubic mile of sediments. The total volume of sediments at the present time is estimated to be about 70 million cubic miles. If the rate of sedimentation had been constant, this would mean that it had proceeded for about 350 million years. However, it is most probable that this figure should be multiplied by a factor of the order 10, for the same reasons as given in the previous paragraph.

Radioactive methods. The main difficulty with the three preceding methods is that none of them deals with a process of which the rate has been constant in the past. Clearly, a method based on such a constant process would be greatly superior to the other ones. Fortunately, we possess such a process in radioactive decay, as there is reasonably reliable evidence[5] that the rate at which radioactive nuclei decay has not changed appreciably over at least the last billion years.

It is very easy to obtain an upper limit for the age of the earth, either from the fact that uranium and thorium are still present in rocks, or from the present isotopic-abundance ratio of uranium. H. N. Russell pointed out that since the half-lives of U^{235}, U^{238}, and Th^{232} are, respectively, 0.7, 4.5, and 14 billion years, the mere fact that they are still present on the earth in appreciable quantities indicates that the earth cannot be older than about 10 billion years.

At present, the ratio of U^{238} to U^{235} is about 139. Since U^{235} is decaying nearly seven times as fast

as U^{238}, we can calculate at what moment in the past the two isotopes would have been equally abundant. Since isotopes of odd atomic weight are always less abundant in nature than those of even atomic weight (apart from a few special cases), this period would give us another upper limit to the earth's age. This method, which we owe to Rutherford, leads to an upper limit of about 6 billion years.

More reliable methods for determining the age of the earth are based on the fact that U^{238}, U^{235}, and Th^{232} all decay to produce a stable lead isotope. The three end products are, respectively, Pb^{206}, Pb^{207}, and Pb^{208}, while the lighter lead isotope Pb^{204} probably existed in the primeval lead that was present from the beginning of the earth's history. The isotopic constitution of lead was modified by the production of "radiogenic" lead, that is, the decay products of uranium and thorium. At a certain moment in the development of the earth, minerals were formed, and, in general, minerals containing lead were separated from those containing uranium and thorium. One can now either consider the minerals containing uranium and thorium and from their lead content determine their ages and thus a lower limit for the earth's age, or one can consider the lead ores and, by a method to be described presently, arrive directly at an estimate of the age of the earth. The first method is the easier one, because the age of a mineral containing radiogenic lead and, say, thorium follows by a straightforward calculation from the abundances of Pb^{208}, and Th^{232} and the known rate of decay of thorium. The age of a great number of minerals has been determined in this way, and the results vary between 60 million and 2 billion years.

The second method that has been developed by Holmes[3,6] is probably the most reliable one for determining the age of the earth. The basic idea of this method is that the lead in lead ores will contain partly primeval lead and partly radiogenic lead, and that the proportion of radiogenic lead will be larger for the younger minerals. By a rather complicated analysis, it is possible to calculate the isotopic constitution of the primeval lead and hence

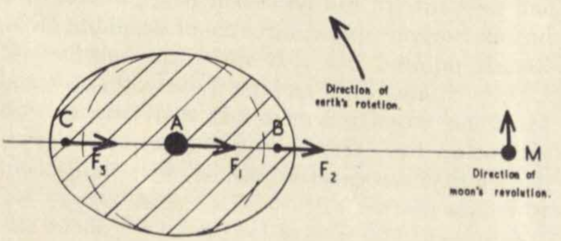


FIGURE 1.

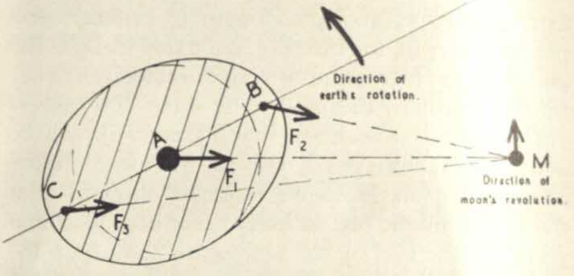


FIGURE 2.

the time before the lead ore was deposited. Adding to this period the age of the ore itself, one has obtained a value for the age of the earth. In this way, Holmes' method leads to an age that is just over 3 billion years.

The Age of the Moon

It is well known that the moon produces tides on the earth, but it is not always realized that these tides in turn influence the moon's orbit. Due to the fact that the earth is not perfectly elastic and that the oceans are not perfectly fluid (that is, the water in the oceans possesses viscosity), so-called *tidal friction*[7] will occur. It is only a small phenomenon but its effects are secular; that is, they accumulate over long periods and they may have been the cause of great changes in the orbit of the moon. Let us consider this in somewhat more detail. In Figure 1, we have pictured the situation that would arise if the oceans were perfect fluids. The tides would be exactly on the straight line connecting the center of the earth (A) with the moon (M). For the sake of simplicity, we have assumed that we may divide the earth's mass into three masses, situated at A, B, and C, corresponding respectively to the mass of the solid core of the earth and to the masses of the tides on the two sides of the earth. (It must be remembered that apart from the tide immediately under the moon, there is also a tide furthest away from the moon which is due to the fact that the moon is pulling harder at the mass at A than at the oceans at C.) In the case of Figure 1, the forces exerted by the moon on the masses at A, B, and C (indicated by F_1, F_2, and F_3) are all along AM and their resultant which is equal, but opposite in sign, to the force exerted by the earth on the moon, is also along AM. In this case, the moon would proceed along a circular orbit.

The situation is, however, different in the case depicted in Figure 2. If viscosity is present, the tides will lag behind and will only reach their highest point after they have passed under the moon. Since the earth is rotating faster than the moon is revolving around the earth, the situation will be as

pictured in Figure 2. Now the three forces F_1, F_2, and F_3 are no longer colinear, and since F_2 is larger than F_3 and also the angle *BMA* is larger than the angle *AMC*, there will be a resultant force on the moon that has a component perpendicular to *AM* in the direction of the moon's revolution. This force will tend to increase the moon's angular velocity and thus the distance *AM*.

From the analysis given in the preceding paragraph, we see that the moon must have been nearer to the earth in the past. Since the total angular momentum of the moon-earth system must remain constant, it follows that the earth's rotation must have slowed down, or, in other words, the days have lengthened. The effect is small, however, and Jeffreys estimates that the day has lengthened by about one second over the last 100,000 years. The effect also gives us a method for estimating the age of the earth-moon system, for which we can take, for instance, the period over which the moon receded from one-half of its present distance to its present distance.* The calculations are rather complicated and they lead to a value of between 2 and 4 billion years for the age of the moon and thus to a lower limit for the age of the universe.

The Age of Meteorites

We saw that the age of minerals could be determined successfully by examining their lead content. The same method has also been used, notably by Paneth and co-workers,[8] to determine the age of meteorites. Apart from this method, they also use the helium content of meteorites as an indicator of their age. The decay of uranium and thorium is by α-radioactivity and provided all α-particles are captured and no helium is produced by other processes, the helium content of a meteorite should be as reliable a guide to its age as is its lead content. By examining the helium content of several meteorites, Paneth arrives at ages ranging from 60 million to 7 billion years.

However, Bauer[9] has pointed out that there was a strong correlation between age and size of the meteorites or between helium content and size, in such a way that the smaller meteorites had the larger helium content and thus were the older ones on Paneth's scale. From this correlation, Bauer concluded that the large helium content in small meteorites might be due to a surface effect and not to radioactive decay. Indeed, cosmic rays are sufficiently efficient helium producers to account for practically all the helium in the small meteorites.

Re-examining the evidence, Bauer arrives at a common age of about 60 million years for the meteorites examined by Paneth. This would give us thus a lower limit for the age of the meteorites and also for the age of the universe. In view of the fact that many authors tend nowadays to the opinion that the breakup of a planet between Mars and Jupiter may have produced, among other things, the meteorites, the age determination of the meteorites may well be one of the more important clues to the past history of our solar system. If one accepts the idea that all meteorites have a common origin, one is tempted to suggest Holmes' method as a means to determine the age of the hypothetical planet that produced the meteorites.

Distribution of Stars Among Spectral Classes

In our quest for clues to the age of our universe we now leave the realm of our solar system and begin to consider the stars in general. We can either consider their physical properties and from the distribution of stars among the spectral classes arrive at an upper limit for the age of the universe, or we can consider their dynamical properties and from the distribution of their velocities try to derive data about the age of the universe. We shall discuss the second method later and for the present concentrate on the first method.

It is nowadays well established that the source of stellar energy is nuclear and, put in the simplest possible terms, is the conversion of four protons (or hydrogen nuclei) into one α-particle (or helium nucleus). If we know how large a fraction of a star is helium and how much energy the star radiates per unit time, we can calculate an upper limit for its age, if we assume that all the helium of the star was produced from hydrogen and that the energy output of the star has not changed during its life history. There are good reasons to believe that most stars, with perhaps a few exceptions, have the same chemical composition and that most stars contain (by weight) about 42 per cent helium and 51 per cent hydrogen. It then follows that the age of the various stars is directly proportional to their mass and inversely proportional to their energy output, or luminosity. In this way, Unsöld[10] has computed the maximum age of stars of different spectral classes. The sun, for instance, could at most be 40 billion years old, if we may use this yardstick. However, *O, B,* and *A* stars, the brightest stars in our galaxy,† which have slightly larger masses than the sun, but

* Fortunately, the age estimate is extremely insensitive to the initial distance *AM* and we would have obtained practically the same result if we had chosen the period over which the distance increased by a factor ten, or only by twenty per cent.

† The brighter stars in Orion are *O* and *B* stars, and Sirius is an *A* star.

far higher luminosities, could only have produced energy at the present rate for a period varying from a few million years for *O* stars to about one billion years for the brightest *A* stars. Clearly, we cannot use these data to determine the age of the universe, but rather we are led to the conclusion that stars are probably still being created, even at this moment. There are, indeed, many other indications that this is probably the true state of affairs.

Even though this first attempt at arriving at an estimate of the age of the universe by studying stellar evolution was not successful, nevertheless, as was shown by Russell,[11] it is possible to obtain an upper limit for the age of the universe by considering the evolution of a typical main sequence star, such as the sun. Let us therefore consider the evolution of the sun. At present the sun contains (by weight) 51 per cent hydrogen. Assuming that the carbon-nitrogen cycle[12] is in operation at present and has been in the past and will be in the future,* one can calculate both the luminosity and spectral class of the sun at any moment in the past and in the future, and the period that the sun would spend or has spent in each spectral class. It turns out that the sun, contrary to what one might expect, will start as a rather dim *K*-star and gradually brighten until it would finally become a hot, bright *B*-star. The reason is that when hydrogen is converted into helium the mean molecular weight will increase and therefore according to Bethe's theory of stellar energy production,[12] so will the central temperature. Since the energy production increases very strongly with increasing central temperature, the sun's luminosity will greatly increase. In order to get the heat flow in balance, the sun will expand; it will become slightly larger and much brighter and hotter.

From Russell's detailed considerations, it follows that the sun would spend the first few hundred billion years of its life as a *K*-star and, after that, about 30 billion years as a *G*-star, its present classification. In the near future (astronomically speaking), the sun would spend 5 billion years as an *F*-star, 3 to 4 billion years as an *A*-star and the last 500 million as a *B*-star. If the universe, or rather our galaxy, had existed for as long a period as 100 or 1000 billion years, we should expect to see stars of masses about equal to that of the sun distributed over the various spectral classes, roughly in proportion to the period spent in each spectral class. This would mean that in our galaxy there would be about six times as many *G*-stars as *F*-stars, and about seven times as many *A*-stars as *B*-stars of solar mass. Now, *A*-, *B*-, and *O*-stars are so much brighter than *F*-, *G*-, or *K*-stars, so that they can be seen at much greater distances. The distribution of stars observed on the earth over the spectral classes would therefore be different from the one that would be proportional to the period spent in each spectral class, since it is necessary to correct for this brightness effect. In fact, if one applies this correction, it turns out that the distribution of stars of solar mass observed on the earth would be such that there would be about three times as many *G*-stars as *K*-stars, and about ten times as many *A*- and *B*-stars as *G*-stars, if an equilibrium distribution had been reached. Actually, there are practically no stars of solar mass belonging to the spectral classes *A* and *B* and we can thus conclude that our galaxy is not old enough to have produced these stars. This leads us to an upper limit of the age of our galaxy of, say, 100 billion years.

* It is immaterial for the present discussion whether or not this assumption is well founded, as it very well may not be in the light of recent evidence.

As far as our sun is concerned, Ledoux[13] has recently arrived at an estimate of its age by assuming that the chemical composition of its outer envelope—which, as we saw a moment ago, contains about 40 per cent helium—is different from that of its core. It may be remarked here that the question whether or not mixing of various layers of a star occurs is still far from satisfactorily or finally settled. The difference between the chemical composition of the outer layer and of the core would be due to the transmutation of hydrogen into helium in the central parts. Considering a suitable model for the sun, Ledoux finds that whereas the envelope contains about 40 per cent helium, the core may contain as much as 70 per cent. Assuming that the outer parts show the original composition of the sun, Ledoux could estimate the age of the sun, for which he found a value of about 5 billion years.

As Bok[14] points out, stellar evolution as a possible indicator of the age of our universe becomes extremely unsatisfactory as soon as we try to take white dwarfs, which are probably relatively old stars, or giants and supergiants, which are probably relatively young, into account. The exploration of this large and important field has only just started.

Distribution in Kinetic Energy of Stars

The apparent approach to equipartition of kinetic energy for the stars in the neighborhood of the sun has been in the past one of the most powerful arguments in favor of the so-called long time-scale, that is, of adopting as the age of our universe a period of the order of a thousand billion years or even longer. This long time-scale was favored by the astronomers in the nineteen-twenties and early

nineteen-thirties. In 1929, for instance, Jeans[15] summarized the generally accepted point of view with the words "a good many lines of evidence converge in indicating ages of the order of from five to ten million million years for the main mass of the stars . . . and all the nebulae in the sky may be of the same age." This point of view was mainly based on Jeans' own researches on the ages of moving clusters and on the statistics of binary orbits, but work by Bok,[16] Ambarzumian,[17] and Kuiper[18] between 1934 and 1937 showed that Jeans' estimates were in need of a drastic revision; and, as we shall see presently, recent investigations of these subjects also lead to the short time-scale, that is, an age of the universe of a few billion years.

Let us return to the distribution of kinetic energy in our Milky Way. As early as 1911, Halm[19] pointed out that the average kinetic energy of stars in a given spectral class, with the exception of the *B*-stars, was roughly the same, independent of the spectral class—a result later verified for *M, A, F,* and *G* stars by Vyssotsky and Williams.[20] Since the relaxation time, that is, the time necessary to produce an equilibrium distribution leading to equipartition, in the neighborhood of the sun is of the order of 10,000 billion years, our galaxy should be at least a thousand billion years old.* However, the situation is not as simple as we have just pictured it.

Bok,[14] in his report to the Royal Astronomical Society in 1946, comes to the conclusion that there are too many deviations from total equipartition to enable us to arrive at an estimate of the age of the universe.† He concludes this section of his report with the following paragraph: "The whole problem of equipartition of kinetic energies and deviations from the mean reduces itself to a basic problem of galactic dynamics and of stellar evolution, one that we have hardly begun to recognize as a problem and for which the solution is not yet in sight. Formulated concisely, it reads: *How can we explain certain observed correlations between the physical properties of the stars (such as mass, period, spectrum) and their dynamical, or orbital characteristics in the galaxy?* (The italics are Bok's.) For the present we can only guess that from these correlations we may ultimately be able to draw important conclusions relative to the cosmic time-scale. We may, however, state with confidence that the equipartition argument can no longer be quoted in support of the long time-scale."

Recently, Gondolatsch[21] has considered this problem very carefully and although his results agree with Bok's conclusions, he feels that he is able to say that the distribution of kinetic energy among the various spectral classes points to an age of the universe of the order of a few billion years. However, this conclusion does not seem to be very vigorously founded.

* As has been pointed out recently by R. Kurth, it is questionable, to say the least, whether an open system like our galaxy would ever attain a Maxwellian distribution. This difficulty also occurs in the discussion of clusters and of clusters of galaxy, but fortunately does not impair our conclusions.

† Bok lists the following four exceptions: the kinetic energies of early *B*-stars are too small; the kinetic energies of *K* and *M* giants are too large; the kinetic energies of cluster-type variables are too large; and, star streams exist.

Distribution in Separation of Binaries

For many purposes we may consider the gravitational field in our Milky Way to be a smoothed-out field of force, but sometimes it is necessary to take into account the fact that it really is produced by single stars. This means that we must consider the fluctuations in the gravitational field. As was shown by Chandrasekhar,[22] these fluctuations will ultimately cause a binary system to dissolve. The argument proceeds as follows: Since the two components of a binary star are slightly separated, the gravitational field due to a star passing in their neighborhood will differ for the two components. This difference in field strength will result in a difference in actual force acting on the two components and hence in an acceleration of one component relative to the other. This acceleration will be present only as long as the star is in the neighborhood of the binary system, but successive encounters will give rise to an accumulative effect and after a sufficiently long period, or a sufficiently large number of encounters, the relative velocity of the one component with respect to the other will be large enough to lead to a disruption of the system. Since the effect will be more pronounced the greater the separation of the two components, we shall expect the time of dissolution to decrease with increasing size of the binary orbit.

From Chandrasekhar's analysis it follows that a binary with a mean separation of 100 AU‡ will be dissolved in about 2000 billion years, one with a separation of 1000 AU in about 70 billion years, and one with a separation of 10,000 AU in about 2 billion years. If the age of our galaxy were more than, say, 70 billion years, we might expect the distribution of binaries with separations larger than 1000 AU to be distributed according to an equilibrium distribution, which, according to Ambarzumian,[17] would mean an increase of number of binaries with increasing separation. This distribution is certainly greatly different from the observed

‡ 1 AU (astronomical unit) = 15.10^{13} cm = mean distance of the earth from the sun.

TABLE 1

Method	Estimate of age	Condition at the beginning of the period
Cooling of earth's crust	2–4	formation of earth
Salinity of oceans	1–7	formation of oceans
Sedimentary rocks	~ 3–4	formation of oceans
U^{235}/U^{238} ratio in minerals	< 6	formation of earth's crust
U/Pb ratio in minerals	> 2	formation of earth's crust
Lead ores	3	formation of earth's crust
Moon	> 2–4	formation of solar system
Meteorites	> 0.06	formation of solar system
Stellar evolution	< 100	formation of our galaxy
Sun	~ 5	formation of our galaxy
Equipartition of kinetic energy	~ 2–4(?)	formation of our galaxy
Binaries	< 10	formation of our galaxy
Star clusters	~ 4–5	formation of our galaxy
Clusters of galaxies	~ 2–4	formation of the universe
Red-shift	~ 4	formation of the universe

distribution,[23] where the number of binaries rapidly decreases with increasing separation. From the observed distribution of binaries among the various values of their separation, we are thus led to an upper limit for the age of our galaxy of the order of 10 billion years.

Dynamics of Star Clusters

The stars in our galaxy often appear in groups. Here and there obvious *clusters* of stars appear, even to the naked eye. Some of these have been known since antiquity, such as the Pleiades and the Hyades clusters in the constellation Taurus, the cluster forming the constellation Coma Berenices, and the Praesepe cluster in Cancer. These belong to the group of galactic or open clusters, containing from a score to a few thousand stars. The distances between the stars in such an open cluster are relatively large. On the other hand, there are the globular clusters containing at least 50,000 stars that are all very near together. The nearest and brightest one of the globular clusters is situated in the constellation Centaurus. For our present discussion it is more convenient to divide the clusters into loose and dense clusters. Typical examples of loose clusters are the Taurus cluster, of which the Hyades cluster is the nucleus, and the Ursa Major cluster, whereas the Pleiades and Praesepe clusters and all of the globular clusters are typical dense clusters.

In a loose cluster the star density is so small that we may neglect interactions between cluster members and they will disintegrate partly because of "tidal" forces produced by the central mass of our galaxy,[16] and partly because of encounters with other stars that will dissolve a loose cluster in the same way as they dissolve binary systems. A quantitative analysis of these processes indicates that a typical loose cluster will disintegrate in a period of between two and three billion years.

The effects that lead to the disintegration of the loose clusters are unimportant in the case of the dense clusters. In the latter case the disintegration proceeds, since due to mutual interactions of the cluster members some of them will acquire sufficiently large velocities to escape. The disintegration is thus comparable to the loss of a planetary atmosphere due to the escape of the tail of the Maxwell distribution. Chandrasekhar[24] has shown that in the case of dense galactic clusters such as the Pleiades and the Praesepe clusters, disintegration will follow in a period of between three and six billion years. However, globular clusters have a much longer lifetime and can probably exist practically unaltered for as long as a few thousand billion years.

We see thus that both the loose and the dense galactic clusters are relatively short-lived organisms. The first category is subject to disintegration by galactic tidal forces and the second one to evaporation by internal interactions, and in both cases the mean lifetime of a galactic cluster is of the order of 2 to 3 billion years. Since there are at least several hundred galactic clusters in our Milky Way we are led to assign to our galaxy an age of at most 4 to 5 billion years.

Dynamics of Clusters of Galaxies

Just as stars appear in clusters in our own galaxy, so galaxies themselves can and do appear in groups. In this way our Milky Way is part of the so-called local group which consists of at least thirteen galaxies, or extragalactic nebulae. Our nearest neighbors are the two Magellanic Clouds, which are satellites of our Milky Way, and the well-known spiral nebula, the Andromeda nebula. The local group contains only a small number of galaxies, but there are known a number of clusters of galaxies that contain several thousands of galaxies—for instance, the Virgo and Coma clusters.

These clusters of galaxies will behave like dense star clusters, which means that due to mutual interactions some members of the cluster will acquire sufficiently high velocities to be able to leave the cluster. Their lifetime is, however, of the order of

a few hundred billion years.[25] Since these clusters are a rather common phenomenon, we are led to an upper limit for the age of the universe of about one hundred billion years.

From a study of the density distribution in such clusters of galaxies, Omer[26] has come to the conclusion that this distribution is only compatible with an age of about 2 to 4 billion years.

The Expanding Universe

It is well known that monochromatic light emitted by a source moving away from an observer will show a shift towards the longer wavelengths. This red-shift is proportional to the velocity with which the source is receding. Since distant extragalactic nebulae showed such a red-shift, it was assumed that, indeed, these galaxies were moving away from us. The velocities of recession calculated from the observed red-shift turned out to be directly proportional to the distance of the nebula from us, as was shown by Hubble. The interpretation of this effect was that the universe as a whole was expanding from an original state of extremely high density. From the observed velocity-distance relation it followed that this expansion had started about 2 billion years ago. Recently, however, it was realized that the yardstick for measuring distances, which was based on the luminosity of the variable Cepheids, was wrong and the present estimate of the period evolved since the expansion started is about 4 billion years. It must be stressed, however, that the basis of these estimates is a homogeneous model of our universe. Our universe is certainly not a homogeneous system and Omer[27] has recently shown how much estimates of this kind have to be changed, if only the slightest inhomogeneity is admitted. It seems therefore that the estimate of an age of 4 billion years for our universe, based on the velocity-distance relation, is still far from rigorously established.

Conclusion

We are now in a position to collect all the data and to see whether they fit together. This has been done in Table 1. The first column gives the method used to obtain an estimate for the age of the universe, the second column gives the result in billions of years, and the last column indicates the period whose length is estimated.

To one's great surprise it turns out that the values in the second column agree remarkably well and all lead to the conclusion that a few billion years, say between 1 and 5 billion years, is the length of the present epoch. The data of Table 1 afford, as far as the present author can see, one of the strongest arguments against the hypothesis of continuous creation.[28]

Probably we can best end this paper as we started it by quoting Chandrasekhar.[1] "To conclude, then, we see that the geochemical evidence bearing on the age of the earth and meteorites, the galactic star clusters, the statistics of binary stars, the clusters of extragalactic nebulae and finally the system of the nebulae, all agree in pointing to a time scale of the order of a few billion years. It does not seem that this can be accidental."

References

1. Chandrasekhar, S. *Science,* **99,** 133 (1944).
2. Knopf, A., *et al. Bull. Natl. Research Council* (*U. S.*), no. 80 (June 1931).
3. Holmes, A. *Endeavour,* **6,** 99 (1947).
4. Jeffreys, H. *The Earth,* 3rd ed. Cambridge, England: Cambridge Univ. Press, Chap. IX (1952).
5. Henderson, G. H., *et al. Proc. Roy. Soc.* (*London*), **A145,** 563, 582, 591 (1934).
6. Holmes, A. *Nature,* **157,** 680 (1946); **159,** 127 (1947). Bullard, E. C., and Stanley, J. P. *Ver. finn. geod. Inst.,* **36,** 33 (1949).
7. Jeffreys, H. *The Earth,* 3rd ed. Cambridge, England: Cambridge Univ. Press, Chap. VIII (1952).
8. Arrol, J. W., Jacobi, R. B., and Paneth, F. A. *Nature,* **149,** 235 (1942); Bok, B. J., *Monthly Notices Roy. Astron. Soc.,* **106,** 61 (1946); Unsöld, A. *Z. Astrophys.,* **24,** 278 (1948).
9. Bauer, C. A. *Phys. Rev.,* **72,** 354 (1947); *Ibid.,* **74,** 501 (1948).
10. Unsöld, A. *Z. Astrophys.,* **24,** 278 (1948).
11. Russell, H. N. *Sci. Monthly,* **55,** 233 (1942).
12. Weizsäcker, C. F. von *Physik. Z.,* **39,** 633 (1938); Bethe, H. A. *Phys. Rev.,* **55,** 434 (1939).
13. Ledoux, P. *Ann. Astrophys.,* **11,** 174 (1948).
14. Bok, B. J. *Monthly Notices Roy. Astron. Soc.,* **106,** 61 (1946).
15. Jeans, J. H. *Astronomy and Cosmogony.* Cambridge, England: Cambridge Univ. Press, p. 381 (1929).
16. Bok, B. J. *Harvard Circular,* no. 384 (1934).
17. Ambarzumian, V. *Nature,* **137,** 537 (1936); *Russ. Astron. J.,* **14,** 207 (1937).
18. Kuiper, G. P. *Publ. Astron. Soc. Pacific,* **47,** 201 (1935).
19. Halm, J. *Monthly Notices Roy. Astron. Soc.,* **71,** 610 (1911); Seares, F. H. *Astrophys. J.,* **55,** 165 (1922).
20. Vyssotsky, A. N. *Astrophys. J.,* **97,** 38 (1943); Vyssotsky, A. N., and Williams, E. T. R. *Ibid,* **98,** 187 (1943).
21. Gondolatsch, F. *Z. Astrophys.,* **24,** 330 (1948).
22. Chandrasekhar, S. *Astrophys. J.,* **99,** 54 (1944).
23. Kuiper, G. P. *Ibid.,* **95,** 212 (1942); Öpik *Publ. Astron. Observ. Univ. Tartu,* **25,** (6), (1924).
24. Chandrasekhar, S. *Principles of Stellar Dynamics.* Chicago, Ill.: Univ. of Chicago Press, Chap. V (1942).
25. Tuberg, M. *Astrophys. J.,* **98,** 501 (1943).
26. Omer, G. C., Jr. *Sky and Telescope,* **8,** 123 (1949).
27. ———. *Astrophys. J.,* **109,** 164 (1949).
28. Hoyle, F. *Monthly Notices Roy. Astron. Soc.,* **108,** 372 (1948); **109,** 365 (1949).

An Introduction to a Popularized Symposium on Evolution*

RICHARD B. GOLDSCHMIDT

Richard Goldschmidt is Professor Emeritus of Zoology at the University of California. He formerly was Director of the Kaiser Wilhelm Institute for Biological Research in Berlin, and for two years (1924–26) was a guest professor in Tokyo, where he carried on his famous studies of the genetics and sex-determination of the gypsy-moth, Lymantria. *His published work deals mainly with cytology, genetics, and evolution, and has included such well-known books as* The Physiological Theory of Heredity, Physiological Genetics, Evolution, *and the more popularly written* Understanding Heredity. *He was recently honored by being elected the President of the Ninth International Congress of Genetics, which met in Italy during August of this year.*

When Mills College, in celebration of its centennial, decided on a biological conference which would be of interest to a general audience, the choice of an appropriate subject was not difficult. The biologist knows of many important discoveries made during the last century which inaugurated a new era in his science and made their imprint on it to last for a long time. Such was the rise of comparative anatomy and of comparative embryology in the sixties of the last century; the rise of cytology in the seventies; the rise of bacteriology and serology a little later; that of experimental embryology in the eighties and nineties; that of endocrinology mostly in this century; and the phoenix-like flight of genetics since 1900. But there is only one single generalization which has remained the center of biological thought since its final statement, only a few years after the foundation of this college. This is the doctrine of evolution as formulated by Charles Darwin in 1859. Vague ideas of the evolution of the cosmos, as well as of the world of living creatures, had been produced for over 2000 years since the time of Eratosthenes and Anaximander. Kant and Laplace had proposed plausible theories of cosmic evolution which are coming into their own again nowadays; and Lamarck and Geoffroy de St. Hilaire had presented convincing ideas of organic evolution which were strange mixtures of deep insight and wild guesses. But Darwin's work struck like lightning biological thought, which was ready for the theory of evolution, because he presented together with the material in favor of evolution an elaborate and convincing theory in explanation of why evolution takes place. This brought home at once the truth of evolution to the biological profession. Furthermore, it gave them a central, pivotal idea which shed light on all the different phases of their work and actually forced them to measure the meaning of their work by the yardstick of the brilliant generalization.

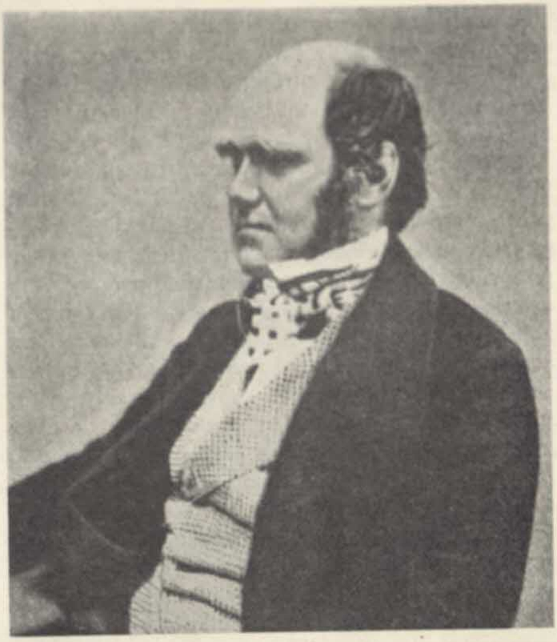
There are dozens of all-important scientific discoveries which failed to impress the thinking of their time outside of the profession, nay, even failed to come to the notice of the public. There is, for example, no public repercussion to the quantum theory, which has completely changed the physics and the natural philosophy of our time. But the doctrine of evolution, from the outset, affected the minds of layman and scholar, of naturalist, philosopher, law giver, theologian, sociologist, moralist, in short, of every thinking individual, and this

* This symposium for the layman took place at the centenary of Mills College, Oakland, California. The general introduction by one of the organizers is presented here.

deep effect lasted from about a hundred years ago to the present day. One potent reason for this is found in the fact that man now became included in the inexorable laws of nature, which led to his democratic dethronement from the self-appointed role of master to that of an integral, though small, part of nature. Another reason is that evolutionary thinking opened a way to free the mind from medieval fetters when it replaced self-perpetuating fantasies of prescientific times with natural laws capable of study and analysis. Still another reason is that the type of thinking introduced by the new insight, namely, thinking in terms of change from the simple to the complicated by the action of immanent laws without guidance from without, suggested new approaches to almost every field of intellectual endeavor.

Thus, for a hundred years, the doctrine of evolution has been the center of the biological sciences as well as of much of the progressive thought in all other sciences, from chemistry to anthropology, to medicine and jurisprudence, to psychology and philosophy. There have been ups and downs, peaks and valleys, as in all actively progressing fields. There have been days when a book with the title *At the Deathbed of Darwinism* could be printed and taken seriously. There have been days when laymen and fanatic partisans of some credo tried to burn the doctrine of evolution at the stake. There is not much left of this today. Within the biological profession the study of evolution is now maybe at the highest peak ever reached, and outside of the profession even the strongest opponents have made their peace with the doctrine, though reserving for themselves some points of a metaphysical nature outside of the realm of biological research.

The present-day theory of evolution is, as might be expected, in many respects highly technical. Nobody within the biological sciences would think of generalizing in this field without a knowledge of geology and paleontology, comparative morphology and development, taxonomy and ecology, cytology and genetics, mathematical statistics, and even biochemistry. But this does not mean that the essentials could not be presented to a cultured audience without going into the technical details and without discussing still controversial points. This is exactly what this centennial program will try to do and, I hope, will succeed in doing in spite of the formidable array of leaders in the different fields who will be the speakers. Thus it is my duty, as a kind of keynote speaker, to sketch, in quick out-



Charles Darwin

lining strokes, the quintessence of present-day evolutionary thought. The details of this portrait will be painted afterwards with loving detail by the group of jubilee lecturers.

The theory of evolution is a biological theory, not a philosophical one. It is a generalization meant to explain the features of the organisms of the present and the past by the history of their ancestors and as a result of the action of natural forces within and without the organism in the course of time. In this statement the emphasis lies upon the words *biological* and *natural*. The scientist can work only with data which are derived from observation and experiment and which can be checked, measured, analyzed, confirmed and, in the case of the experiment, repeated. Thus his work is both factual and pragmatic. The basis of his work is the belief that the facts he deals with can be understood within the known or still to be discovered phenomena of nature which resolve themselves in the end into the basic notions of physics and chemistry. If he meets with difficulties, as invariably he will, he hopes for a future solution in the same basic terms, and he works toward this goal. He refuses to take refuge in metaphysics, believing this to be scientific defeatism. It is well known that in this attitude he is opposed, sometimes by outsiders and occasionally also by an insider with an inclination toward mysticism. The biologist's answer is this: more observation and more experiments.

Actually the theory of evolution has, since its origin, been especially favored by the attention of groups who tried to introduce metaphysical ideas into a field which should belong to the naturalist. Such writers frequently had great success with the layman, who tends to be impressed by such vague ideas as "creative evolution" or "holism," ideas which try to solve real difficulties of scientific analysis by a meaningless metaphor which cannot be proven or disproven, checked or analyzed, or even described unequivocally. The names of Bergson and Lecomte du Noüy will suffice as two examples among many. Be it stated therefore, at the outset, that such ideas, whether coming from philosophers, novelists, naturalists, theologians, or even great statesmen, have never helped our science to make the smallest step forward into unknown territory. They have never done anything but replace temporary ignorance by an appeal to unknown, supernatural forces or agents, and lure untrained minds into the self-deception of mistaking a cleverly circumscribed confession of ignorance for a deep insight and explanation. Therefore the overwhelming majority of students of evolution leave aside creative evolution, finalistic causes, emergent evolution, entelechies, and similar intangible word-plays, and continue to do their work.

By evolution we mean here the evolution of the living organisms from their beginnings up to the highest types like flowering plants, insects, man. But there is also an evolution of the atom, of the elements, of the complicated molecules, of the cosmos, of the solar system, and finally of the earth. Here we consider most of these facts of inorganic evolution as generally given. But some of them are closely linked with our study of organic evolution, furnishing, as it were, the backdrop to the stage upon which the evolution of the organisms has taken place. Thus our first topic is this: The starting point for the evolution of the living world. Evolution from the first beginnings of life to man requires time, much time. Therefore, our first problem is whether the development of our planet has taken enough time to give organic evolution a chance. Today we have really good information about geological time, which turns out indeed to be long enough for evolution to take place, and the first speaker, a distinguished geologist, will tell us the important facts.

Given the time required for finishing organic evolution, the next requisite is to have it start, which requires the presence of the proper physical and chemical conditions for the origin of life. I have seen the time when such phantasmagories as a "primordial mucus," from which the first cells originated, were taken as scientific theories. Since that time chemistry, physical, and colloidal chemistry, and biochemistry have taken over, and though they have not yet solved completely the old puzzle of the origin of life, it no longer appears so formidable. The second speaker, a famous biochemical geneticist, will give you authoritative information on this great problem.

Given the chemical evolution of living substance, we are anxious to know how the simplest living molecules, if we may use this not very satisfactory designation, may have turned into organisms. Again we do not have the solution as yet. But there are organisms known, namely the viruses, of such relative simplicity in a chemical sense, though quite complicated in a biological sense, which furnish interesting information about the beginnings of life. You will have the pleasure of studying this problem under the guidance of the father of modern virus research. Thus the first three papers will furnish the background for the stage upon which evolution of the living world has taken place and perhaps still is taking place.

The next major section of evolutionary fact and thinking may be entitled "Evolution as a fact." We know with certainty that all the manifold facts of biology lead inevitably to the conclusion that all organisms are the products of past evolution, which has built up the higher types out of lower ones. Thus the present living world may be represented as the top branches of an evolutionary tree. Though this is a logical conclusion from the analysis of a myriad of facts, it is still a historical conclusion, not an observed fact. Fortunately there is a science which is able to observe the progress of evolution through the history of our earth. Geology traces the rocky strata of our earth, deposited one upon another in the past geological epochs through hundreds of millions of years, and finds out their order and timing and reveals organisms which lived in all these periods. Paleontology, which studies the fossil remains, is thus enabled to present organic evolution as a visible fact, and a renowned representative of this discipline will make you partners of this knowledge.

Having thus established evolution as a scientific fact, we turn to the third major chapter, the one dearest to the biologist proper, the great question: What causes evolution to take place?

In the introductory remarks we mentioned the fact that Darwin is the actual father of the theory of evolution, because, unlike his predecessors, he found the explanation for evolutionary change.

Much has been learned since without changing the general principles discovered by Darwin. The details have been modified, less clear notions replaced by clearer ones, new methods of study introduced, and basic discoveries in newly developed biological fields have been made. This has enabled the study of evolution to develop into a modern, flowering, and exact science. The basic concepts of Darwin which have stood the test of time are the following:

1. Organisms show variation. This means that in a population of many individuals of the same species small differences between the individuals are observed in regard to a number of characters. Some individuals are, for example, a little larger, others a little smaller than the typical size of the species. Some are darker, others are lighter, some are more fertile, others are less fertile. Frequently these differences are hereditary.

2. Of all the offspring of a pair of organisms only two can survive under normal circumstances to replace the parents, thus keeping up the equilibrium between different organisms, predators, and prey. This has also been called the struggle for existence, though not intended in a literal sense.

3. Within a varying population which is as a whole well attuned to its normal environment—a condition which is called adaptation—some of the variants fit better, others less well, the milieu. The better-fitted ones have a greater chance to survive and to propagate their kind (provided their superior traits are hereditary) than the less well adapted ones. This is called the survival of the fittest by means of natural selection, with its corollary the destruction of the less fit ones. Natural selection, much misunderstood in early Darwinian days, is thus not an abstract principle which produces or creates something. It is nothing but a name for the fact that certain hereditary types within a definite environment have a greater chance of survival than others.

4. Natural selection, working upon hereditary variants in a mixed population, thus can replace one type by another, and if this continues for many generations a very different new type may evolve —a new species.

It is our intention to show, in the following group of lectures, what form this basic Darwinian doctrine has assumed after almost 100 years of study, how it was made specific, freed of ambiguities, and underpinned with a huge construction of observation and experiment.

The basic fact of evolution is hereditary variation. Evolution, which, after all, means change from one heredity to another, is not possible without the appearance of different types. This is hereditary variation. Non-hereditary variation, which also exists, is left out here because it does not contribute to evolution. Nowadays, we call hereditary variation "mutation" and such a variant a "mutant." We know also to a considerable extent what mutation is. It is a sudden change in the framework of the chromosomes within the nucleus of the cell. If the tiniest section of a chromosome which is capable of changing (mutating) is called a gene, mutation produces the mutant gene which controls a new, different character. The mutated gene always reproduces its kind, and a new hereditary type has arisen. We know further that mutation is constantly occurring within a population, possessing a known frequency for each individual sort of gene, and we know that mutation can be experimentally increased by powerful radiations or chemicals. The geneticist, in producing and analyzing mutation and simultaneously weeding out non-hereditary variation, is thus able to give Darwinian variation a definite, exact meaning, as one leading geneticist will soon tell you in more detail. It should be added that in the firmly established group of facts to be presented by different speakers no room is left for an assumed heredity of direct environmental effects. Only mutation, caused by still unknown spontaneous happenings within the architecture of the chromosomes, can result in a new heredity.

In a freely interbreeding population of a species of animals or plants in which mutation occurs of one or the other gene with a definite frequency, it can be calculated how frequent the mutant will be in a population of known size after a certain number of generations. This is possible because all mutant genes are inherited according to Mendel's laws, which show that the genes are handed around and sorted out in the offspring of two different parents according to simple statistical rules. Thus the geneticist has established one aspect of what in Darwin's time was called variation.

This becomes still clearer if we look at the fate, not of a single mutant in a population but of many of them together, which is the actual situation, because all kinds of genes show the phenomenon of mutation. Again, genetics has established the facts. One of the consequences of the laws of Mendelian inheritance is that in the offspring of a pair of individuals differing in a number of pairs of genes these will be reshuffled in such a way that all possible combinations appear in definite numerical ratios which are controlled by simple statistical laws. In a mixed interbreeding population, finally,

there will be established an equilibrium, meaning that the different mutant genes and their haphazard combinations in all possible assortments will be present in definite numbers which can be calculated. One calls such a shuffling and reassortment of different genes "Mendelian recombination." Thus we see that hereditary variation in a mixed population means not only the presence of individual mutants but also of an array of mutant recombinations. As mutation occurs all the time, though with a small frequency, and as free interbreeding of the many individuals within a population results in recombination of mutants, any normal interbreeding population of any organism will contain an array of hereditarily different forms, the relative numbers of which are controlled by the rate of mutation and the statistical consequences of breeding. This will occur if nothing interferes with the working of pure chance, which is the meaning of the words: "statistical consequences." This, then, is the real sense of what Darwin vaguely had to call "variation." I trust that you will hear more of it in the geneticist's paper and that these facts will come up again in many of the further discussions by various lecturers in connection with different aspects of the problem.

Thus far we have looked at populations of individuals within one species with the eyes of the geneticist. But it is also possible to study them with the methods of the taxonomist, i.e., the student of animal and plant systematics; or with the outlook of the ecologist, who studies the relations of the organism to its environment; or with the experiences of the animal and plant geographer who studies the distribution of the organisms in space. All these sciences, which will be represented later by prominent spokesmen, for both the animal and plant kingdom, have assembled the next important set of evolutionary facts, which assume special significance if interpreted by the results of genetics. The taxonomist finds that the species is not a simple, invariable unit. Actually within the species lower entities can be isolated, the subspecies, which can be described and recognized by definite characters which are hereditary and are based, as the geneticist could show, on Mendelian mutants and their combinations. These subspecies occupy definite geographic areas within the distributional area of the whole species, and they are characteristic of their area. As all subspecies are fertile inter se when crossbred, hybrid populations may occur where the subspecific areas meet. Further study reveals that the subspecies, though as a rule the last unit which the systematist labels, are not necessarily uniform themselves. Actually still smaller units may be distinguished, occupying subareas, and even within these, genetically different isolated colonies may exist. Thus there is no limit in principle to the genetic and spatial subdivision of the species, though in nature some forms may be more subdivided, some forms less subdivided.

At this point the ecologist enters the picture. In studying the organism in its environment he realizes that many, if not all, traits which characterize these subdivisions are of an adaptive nature. This means that the observed trait or combination of traits enables the organism to live in a definite environment or habitat. The prostrate growth, time of flowering, and life cycle, all of them hereditary traits typical for an alpine plant, enable it to thrive in the peculiar alpine environment. With alpine or desert forms, or other groups peculiar to an extreme environment, this is easily seen and also proved. The respective subspecies or still lower unit is clearly adapted to the specific environment. It is an ecotype, as it is called. Though this is not always so obvious, a closer study frequently reveals that traits which at first sight seem hardly to have any meaning for the welfare of the organism, are in fact one conspicuous facet of a complex of physiological conditions, themselves highly adaptive. Such adaptive physiological traits as, say, tolerance or need for a certain chemical, are recognizable only when properly studied, at which time they may turn out to be the real thing hidden behind an unimportant, concomitant, visible character.

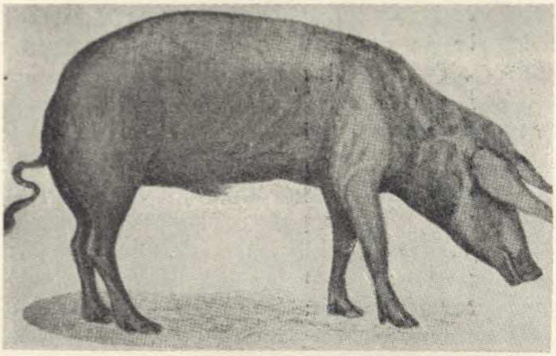
With the statement of these facts, which will come up again in many of the subsequent papers, especially those by the taxonomists and ecologists, we have reached the next problem of evolution, after the prior analysis of genetic variation. How does evolution—thus far still below the level of the species—produce the different subspecific or still lower forms which are adapted to their environment by heredity? The same question, of course, appears also upon the level above the species, as species, genera, or families are also adapted to their specific surroundings: think of a woodpecker or a sea lion adapted to its peculiar mode of life. The process of evolution which involves adaptation and the occupation of diverse geographic or environmental areas may be called adaptive radiation, and the specific environment to which the adaptation has taken place is called the environmental or ecological niche. Adaptive radiation then puts the taxonomic groups into their proper ecological

niche, and you will hear of interesting examples in the papers of botanical and zoological experts in this field.

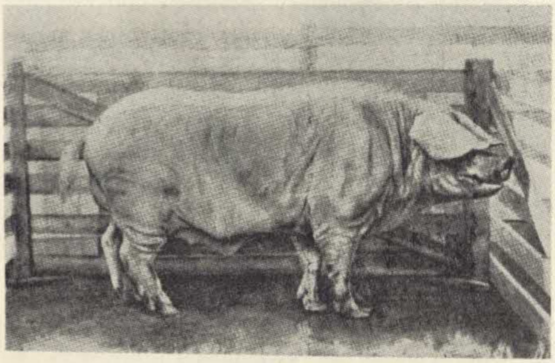
The old Darwinian answer to this problem would have been: selection is the cause of adaptive radiation. Though this is true as it stands, we require a more detailed explanation in harmony with the genetical facts. This has been found and is actually a very simple one. We saw that in an interbreeding population of a species a whole array of mutants and their recombinations are always present. They are perpetuated in the population if they are not deleterious or inferior in some way. In the latter case they are weeded out by selection because they cannot compete with genetically better-fitted brethren. But there are even definite statistical conditions known in which somewhat inferior mutants and combinations may carry on in the population. Whatever the details of all this, populations will always contain numerous different combinations of genes. Among all these genetical variants some will be of a kind that would enable them to live in another ecological niche, and as soon as they get there by whatsoever chance—say by migration or by change of the local climate, soil, or food—they are adapted to the new niche. The niche then does not make the form adapt itself to its features, but a chance preadapted variant enters its proper niche. Life in a cave does not make animals blind, but genetically blind animals find their way into caves where their disability is not at a disadvantage; or animals that can see become cave dwellers, in which situations mutations that impair their vision are no longer disadvantageous and may become fixed characters of the population. Thus mutation, genetic variation in a population, preadaptation, and adaptive radiation explain the taxonomic, geographic, and ecological picture found within the species. We may add that, once given the origin of diverse species of different genetic constitution, the same reasoning applies also to the explanation of *their* occupation of definite niches.

With the last phrases we have approached the next problem. The categories below the species are completely fertile inter se as well as intra se. But species, certainly in the animal kingdom, are intersterile or produce sterile offspring, some exceptions notwithstanding. Species, then, are genetically isolated, and our former deductions regarding the happenings within the species, which are based upon free interbreeding or, as this is sometimes called, upon gene exchange, do not apply any more to the relations between species, whether they are "ecospecies" occupying different niches or species living side by side in the same habitat (sympatric species). The evolutionary step from one species to the next one involves thus another evolutionary principle. This is isolation. This may mean physical isolation by any macro- or microgeographical, or ecological barrier. It must mean in any case genetic isolation by hereditary, morphological, physiological, or psychological barriers. This includes the presence of mutants for incompatibility on a morphological, physiological, or psychological basis; as well as chromosomal differences which prevent normal cellular behavior of the hybrid and therewith produce sterility, or serological differences. There is no difficulty in visualizing genetic isolation appearing by chance mutations and recombinations between groups which had first been physically isolated. There are many geneticists and taxonomists who assume that the best chance for accomplishing an isolation of genetic groups within a species, thus raising the genetically isolated groups to the rank of new species, is present when subspecies are adapted to very different environments and are physically prevented from interbreeding for a long time, say, subspecies occupying the extreme ends of a geographic series. There are difficulties encountered when one tries to work out the details of such evolutionary happenings, and there are still greater difficulties when one tries to make such happenings the prototype of the origin of new species. But this is not the place to discuss controversial subjects. A variety of the facts as found in specific cases will come up in the lectures of the taxonomists and ecologists.

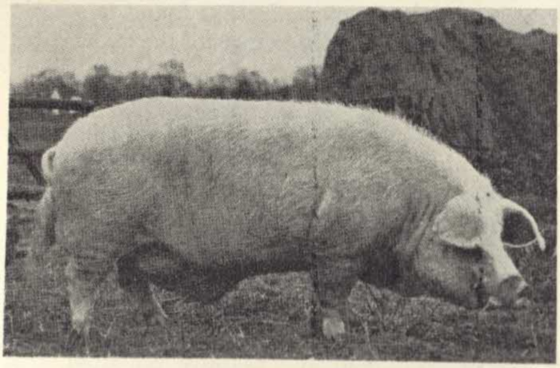
Genera, families, orders, and classes are called the higher systematic categories, and the evolutionist has to explain how species are transformed into genera and how genera are transformed into families, etc. The Neo-Darwinians, following in Darwin's footsteps, take it as granted that the slow accumulation of mutants and their recombinations within the different species slowly transform these into higher and higher categories by means of preadaptation, selection, and adaptive radiation. It is at this level that difficulties arise for the Neo-Darwinian viewpoint, which is derived solely from happenings in interbreeding populations within the species. To mention only one point: in Darwin's time it was frequently emphasized that complicated adaptations can hardly be built up in small steps favored by selection because selection cannot favor a new adaptive character before it has reached a functional level. Take the origin of whalebone whales with their amazing mode of feeding, based



Boar from the middle of the 18th century



Boar from about the year 1900



Boar from 1935

The development of the Danish Landrace. Scientific breeding was carried out to develop a pig that satisfies the producer in regard to fecundity, one that has the ability to yield large and well-developed thriving litters, and, at the same time, one that satisfies the customer in regard to quality.

Photographs from *Denmark Agriculture,* p. 224, The Agricultural Council, Copenhagen, 1935. Reprinted with permission.

upon the giant strainer of their whalebone-covered jaws. They must have evolved from toothed whales, and their embryos still develop first nonfunctional teeth. Is any transitional series of small steps between the two groups imaginable? Innumerable comparable cases have led some evolutionists to ask themselves whether additional evolutionary happenings exist that solve the difficulties created by the Neo-Darwinian tenets for understanding macroevolution as opposed to microevolution. The solution is found in the assumption of large mutations or saltations, meaning mutants which affect basic processes of early embryonic development. This results in major changes of subsequent development affecting many parts of the organism at once and producing a completely changed but still harmonious organization by means of the known regulatory embryonic potencies. Facts in favor of this hypothesis and examples for it are accumulating. But this is a highly technical subject which we are satisfied only to mention.

It is obvious that a complete study of evolution will embrace many sidelines or additional information, which I have not mentioned in this rectilinear introduction to the conference. However, some of them will be presented in three subsequent lectures. There is first one type of evolutionary process, probably almost confined to the plant kingdom, in which an interesting special behavior of the chromosome set is the agent of evolution. In the last paper on the program this fascinating subject will be presented by an outstanding authority. Because we are people, some of us at least are more interested in man than in flies and flowers. The doctrine of evolution touches human affairs in many fields. There is, of course, the evolution of man from his animal ancestors and the difficult problem of evolution of the human mind. At least another day would be needed to discuss this topic. There is further the known and visible evolution of domestic animals and plants, which had already made such a great impression upon Darwin. We shall learn from the mouth of an eminent investigator, of today's aspect of the important problem, at least in plants, where spectacular insight has been gained. Finally, man's breeding and providing of his animal and plant food is a major feature of his own cultural evolution, and it appears appropriate that a lecture on this interesting topic by the best man in the field will follow the banquet.

Mills College has developed in the course of a century from simple beginnings to an educational

institution of national standing. Almost the same century has seen the development of the doctrine of evolution from Darwin's first approach, to a major field of research which is still continuing and expanding in scope. In arranging this conference on this festive occasion, the biologists assembled here wish to congratulate their host, Mills College, on its great performance and to express their confidence that it can look forward to another century of progressive evolution, whether in the Neo-Darwinian way by slow accumulation, or in the saltational way by leaps and bounds.

TWO OPPOSING LAWS

Two opposing laws seem to me now in contest. The one, a law of blood and death, opening out each day new modes of destruction, forces nations to be always ready for battle. The other, a law of peace, work and health, whose only aim is to deliver man from the calamities which beset him. The one seeks violent conquests, the other the relief of mankind. The one places a single life above all victories, the other sacrifices hundreds of thousands of lives to the ambition of a single individual. . . . Which of these two laws will prevail, God only knows. But of this we may be sure, that science, in obeying the law of humanity, will always labor to enlarge the frontiers of life.—Pasteur, on the occasion of the opening of the Pasteur Institute, November 14, 1888.

The Geneticist's Analysis of the Material and the Means of Evolution

CURT STERN

The author was born and educated in Germany where he received his Ph.D. in Zoology at the University of Berlin (1923). From 1924 to 1926 he was a Rockefeller Fellow studying Drosophila genetics in Thomas Hunt Morgan's laboratory at Columbia University. His brilliant demonstration of the cytological basis of crossing-over formed a landmark in the general acceptance of the Chromosome Theory of Heredity. Since 1932 he has been a resident and, in time, a citizen of this country. For many years at the University of Rochester, he is now Professor of Zoology at the University of California. His main interests are in the fields of General Genetics and Human Genetics. His recent book, Principles of Human Genetics, *is recognized as authoritative in that field.*

WHEN the modern student of the evolution of stars approaches his subject, there dissolves before his mind the image of great suns and the beauty of the heavens. Instead, he delves into the properties, energy exchanges, and transformations of invisibly small elementary particles, electrons and protons, neutrons and atomic nuclei and a host of other physical units in order to derive from the knowledge of their interactions the majestic events of cosmogony. Likewise, when the geneticist enters the study of organic evolution there fade into the background the formed products of the history of life, the animals and plants whose complexity and interrelationships first posed the problem of evolution. Instead, there appear on the scene elementary units, the genes, whose properties, energy exchanges, and transformations are thought to account for the miraculous spectacle which life has played on our globe.

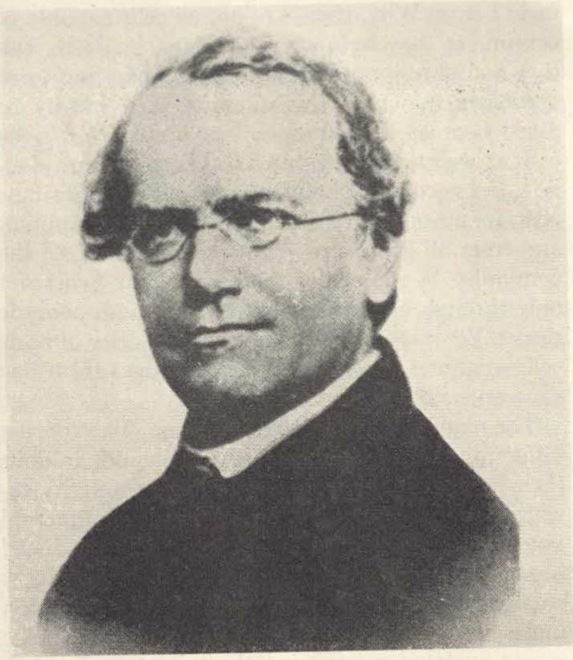
If the undertaking of the astrophysicist seems audacious, what adjective should one apply to the attempt of the geneticist? Atoms and electrons are accessible to direct study by themselves, and the extrapolation to cosmic events of knowledge thus derived is a reasonably safe step. However, while separate physical particles exist independently of formed physical bodies, naked genes are not known. Genes are always parts of an organization, of cells, whether they function singly or as parts of multicellular plants or animals. The properties and actions of genes thus can be investigated only within the complex, historically conditioned organisms themselves. The elements we employ in order to explain evolution are themselves encased in that which needs explanation!

This indeed makes a complicated problem more complex yet, but it does not make it inaccessible to analysis. Let us then see how the geneticist—or at least many a geneticist—analyzes the material and the means of evolution.

To the geneticist the material of evolution is not the organism as a whole but its hereditary basis, that which Weismann called the germ plasm. Weismann's distinction between the soma, the body, and the germ plasm, with its implication that the former is a passing, mortal structure while the latter is the lasting and biologically immortal material, is not a philosophical abstraction but the formulation of observable facts. Every organism which has developed from a fertilized egg owes its bodily structures, so similar to or identical with the structures of his parents, not to some strange process through which the eyes, the limbs, the internal organs of the parents gave rise to the eyes, the limbs, the internal organs of the child, but to a still more

miraculous process through which these parts of the child's body were recreated by the activity of the genetic material in his germ plasm. The bodies of our parents, our own bodies, and those of our descendants are the ephemeral creations of the inconspicuous genetic material. This genetic material, we now know, is largely contained in the nucleus of the cell and, more specifically, in the chromosomes. The analyses of Mendel and his illustrious followers have demonstrated that this material is subdivisible into units which we call genes. While it is a problem of great importance, and one not yet fully solved, whether these units are independent of each other in the sense that each can exist and act within the cell as a separate structural entity or whether they are parts of larger units, we shall here adopt the former view, which has much to support it. We thus think of the genes as structures linearly arranged within the chromosomes.

What do we mean by the word "structure" as applied to a gene? An answer to this question is provided by a consideration of the size of genes. We have various means of estimating the number of genes present in a nucleus. While these estimates are based on several unproved assumptions that do not permit us to place too much reliability on the results, they all agree in leading to a number of, at least, several thousand genes per chromosomal group. From such estimates of total number of genes, it is easy to derive the maximum average volume of a gene. One has only to divide this number of genes into the measurable volume of the chromosomal group. If the chromosomes consist of nothing but genic substance, the resulting figure is the average volume of a gene. If the chromosomes consist of both essentially genetic substance and of accessory material, then the average volume of genes is, of course, smaller than calculated. The outcome of such calculations is clear. Genes are entities whose size is at the molecular level. Whether this means the level of macromolecules consisting of millions of atoms which are perhaps divisible into many identical smaller molecular subunits, or whether this means the level of more conventional organic molecules is a question of secondary importance to our discussion. The essential discovery is that the units of the germ plasm are not any more living units themselves in the sense in which a cell is a living unit, but that they are molecular elements which do not possess properties of life but only properties of chemical compounds. The miracle of their coordination in the organization of a cell the evolutionary geneticist largely leaves aside, taking it for granted himself, and turning it over for analysis to the cell physiologist and the student of development.



Gregor Mendel

From an evolutionary standpoint the basic fact about genes is that each one of them is descended from another gene. Genes arise only by reproduction of formerly present genes. "Reproduction of genes"—this concept needs clarification. For genes as molecular entities reproduction is essentially unlike the process by which a complex whole organism delegates the power to a part of itself to continue the existence of the species. Genic reproduction consists in the biochemical processes, still largely guessed at, by means of which a gene molecule assembles molecular parts from its cellular environment and organizes them into a copy of itself.

The continuity of the germ plasm has become the continuity of genes. Weismann believed, on the basis of certain accurate observations in special groups of animals, that the continuity of the germ plasm was assured by the setting apart, during development, of special cells which would preserve the totality of the genetic material and later become the germ cells of the adult. His theory of development assumed that the body cells were genetically incomplete, having received only various fractions of the total genetic materal of the egg from which they derived. We are now inclined to think that all cells of an organism contain in their nuclei essentially replicas of all the genes with which its develop-

ment began. Why, then, are not all cells capable of continuing the chain of generations, i.e., why can they not all function as germ cells? There are some organisms in which this question need hardly be asked because reproduction *can* occur from body cells as well as from germ cells. Asexual reproduction, from groups of body cells as well as from single cells, is known from numerous invertebrate animals and from all groups of plants, giving proof of the continuity, by molecular synthesis, of the genes not only through germ lines but also through somatic tissues. Yet it remains true that the majority of body cells seem incapable of serving as bridges from one generation to the next.

The reasons for this remain obscure. The specific differentiation of the body cells, it is said, inhibits their genic content from expanding its activity toward the fuller realization of its developmental potencies. How this inhibition is actually produced is one of the riddles of differentiation. Interestingly enough, some modern theories of differentiation make use of concepts similar to Weismann's. The recent evidence concerning the existence of extra-chromosomal, extra-nuclear gene-like elements in the cytoplasm of cells has led to the suggestion that the germ cells retain complete assortments of all possible kinds of such cytoplasmic genic elements, but that these elements are distributed differentially to the various types of body cells so that these are no longer totipotent. At present we do not know enough about these extra-nuclear genetic units to evaluate their general role. We shall leave them out of consideration in the rest of our discussion, at the risk of having to regret this omission at some later day.

The descent of all existing genes from previously existing ones is the basis of the continuity of living forms—but evolution requires continuity plus change. We must therefore explore the ways in which a descendant may receive a different genetic makeup from that of a parent. We may distinguish two different ways, a conventional and a revolutionary one, without implying that the consequences of one way are greater than those of the other.

The conventional way rests on the exploitation of available resources. The cellular processes involved in the formation of germ cells and in their fertilization bring it about that the great majority of all organisms contain in most of their nuclei two like chromosomes of each kind which is characteristic for their species. One of each pair of chromosomes comes from the father, the other from the mother. Since each kind of chromosome carries a linear series of many different genes, one only of each kind being represented, and since these different genes are all characteristic of their chromosome, it follows from the presence of pairs of chromosomes that most nuclei contain pairs of each kind of gene, one member of the pair having been derived from the father, the other from the mother. These pairs of genes, all of which differ strikingly from each other, pair for pair, are usually designated by different letter-symbols, as, for instance, one pair by A, another by B, a third by C. In addition, the two members of any one pair often show a difference between each other, a difference which may be symbolized by calling one member of the A pair A^1 and the other A^2, or one member of the C pair C^1 and the other C^2. The chemical analysis of the genes has not gone far enough to give us a well-founded picture of the differences between genes belonging to different pairs and those belonging to the same pair. One suspects that genes of different pairs represent rather fundamentally different molecular structures while the different partners of the same pair are distinguished by only minor variations of a basically similar molecular pattern.

The existence of such variations between members of genic pairs forms a rich source of hereditary differences between parent and offspring. The well-known ways of endowing the mature egg or sperm cells with genes work in such a manner (1) that every egg or sperm cell receives one and only one partner of each genic pair carried by all other cells of the germ-cell-forming individual, and (2) that all possible combinations of different partners of the different pairs are formed. If, for example, an organism carries A^1 and A^2, two like partners B^1, and C^1 and C^2 among its thousands of genic pairs, if he thus is $A^1A^2B^1B^1C^1C^2$, he then may form germ cells of all possible four different combinations, namely $A^1B^1C^1$, $A^2B^1C^1$, $A^1B^1C^2$, and $A^2B^1C^2$. If he carries two variations of the genes of three different pairs, his germ cells may be of $2 \times 2 \times 2 = 8$ different constitutions; if he carries two genic variations of still as few as 25 different pairs he may produce $2 \times 2 \times 2 \ldots = 2^{25} = 32{,}555{,}432$ different germ cells! This simple process of reshuffling a relatively small number of alternatives in order to get large-scale variation is used by nature as well as by planning men. A building contractor who proposes to build whole tracts of houses of basically identical design frequently offers his clients choices in minor items, as for instance, steel window frames vs. wooden sashes, front or back porch, corner or side window, gas or oil heating, stucco or brick surface. With only five alternatives he can satisfy 32 different demands. Moreover,

some of the alternative combinations of features may make the houses appear so different as to obscure their basic similarity.

The mechanism of forming new combinations from the available resources of different varieties of the genes of each pair is employed in double fashion in the formation of sexually produced organisms. Both parents usually contain two varieties among the partners of many genic pairs. Moreover, the two varieties of a father's pair, for instance, A^1 and A^2 are frequently different from those of the corresponding pair of the mother which therefore may be designated as A^3 and A^4. Such parents will produce four different kinds of offspring, all different from themselves, namely, A^1A^3, A^1A^4, A^2A^3, and A^2A^4. If the two parents contain four different genic varieties for, let us say, 200 genic pairs there will be possible $4 \times 4 \times 4 \times \ldots = 4^{200}$ different genetic constitutions among the offspring. You may be tired of seeing me pile up large numbers, but there is a reason for doing this. The number 4^{200} is not simply larger than any possible comprehension but has a property which is relevant to our biological theme. The number is far larger than the sum of all individual organisms of all kinds that ever lived on the earth. This means that the production of hereditarily changed offspring by means of recombining the available genic varieties of the parents has actually realized only an infinitesimally small fraction of the possible types of organisms which could be born to a single pair of parents. The exploitation of the available resources for the creation of hereditary changes has hardly gone below the surface.

There have been some geneticists who were so impressed by these considerations that they regarded the endless recombinations of existing genic varieties as the exclusive material of evolution. This extreme view was never held by many and, at present, is probably not held by anyone. It would imply that all species contain exactly the same number of genic pairs and that their ultimate common ancestral form not only itself contained that full number of genic pairs but also all varieties of any genic kind which are now found separately in different species. Where the corporate genic content of two present species, II and III may be represented by:

II: $A^1, A^2, A^3, A^4; B^1; C^1, C^2; D^2 \ldots$ and

III: $A^5; B^2, B^3; C^3, C^4, C^5; D^2 \ldots$

their common ancestor, I, would have had to be, at least:

I: $A^1, A^2, A^3, A^4, A^5; B^1, B^2, B^3; C^1, C^2, C^3, C^4, C^5; D^2; \ldots.$

There is much validity in a notation such as the above. It pictures the established facts that each species contains an array of varieties of many genes; that different species may have certain genic varieties in common; and that different varieties may be substituted in one species for those present in the other. Yet while it contains the truth, the scheme certainly does not represent the whole truth—to borrow a legal phrase whose meaning to a scientist must be highly relative in spite of its verbal absolutism.

One of the difficulties of the scheme is that it fixes the number of genic pairs as equal for all products of evolution. While this may perhaps hold for certain short branches or twigs of the tree of life, it seems most unlikely to be correct for its larger expanses. Surely it cannot be true for that part of evolution which is closest to the origin of life! Another difficulty of the scheme is its feature of endowing the ancestral species with all the genetic material which the descendant species obtain only in divided-up fashion. The idea that evolution at large consists in the unscrambling of complexity rather than in its creation runs counter to our general impression and our basic expectations.

The geneticist has ways out of these difficulties. He has been witness not only to the conventional mechanism of genetic change, the recombination of available genic material, but also to ever-recurring revolutionary processes which change the nature and numbers of genes. These processes he calls mutations.

We now have numerous, carefully controlled observations, in bacteria and protozoa, multicellular plants and animals, including man, of the sudden, unexpected production of a new variety of a gene from an old one. A gene of the variety A^1 which for many times has faithfully reproduced its own kind may give rise to one specimen of variety A^2 or A^x. This new variety A^2 or A^x will now faithfully reproduce, for many times, its own kind, but some day in its turn it will give rise to one specimen of a still other variety, A^3 or A^y or, possibly give rise to the original variety A^1.

Not only the fact of occurrence of these mutations is established, but also its frequency has been measured. Different genes have different rates of mutation but a range of from 1 mutation in 10,000 reproductive steps of a gene to 1 in less than 1,000,000 includes most of the known rates of mutations. The stability of most genes, in terms of faithfulness in their reproduction, is high, even with the lowest rate mentioned: 1 mutation in 10,000 still means a rate of stability versus change of 9,999 to 1. But from the point of view of evolution the apparently low instability is of considerable consequence. With an intermediate rate of 1 mutation in 100,000, a

species like *Homo sapiens* which occurs at present at a population size of somewhat over two billion individuals will release each generation more than 40,000 newly mutated genes per genic pair into the species' pool of genic varieties. There is then no theoretical need of endowing a given species with all the genic varieties which its evolutionary descendants will possess. By means of new mutations the future will take care of itself.

Gene mutation provides manifoldness for new combinations. What processes bring about changes in the total number of genes? Genes are part of chromosomes. Any events which change the number of chromosomes in a cell, or add or subtract sections of chromosomes, are therefore changing the number of genes. Chromosomes normally double between nuclear divisions, and sister duplicates get distributed to sister nuclei. Any mishaps in the distribution may lead to new genic numbers. Thus if all duplicated chromosomes are kept together in a single nucleus the total gene number in that nucleus is doubled. Or, if normal distribution to the nuclei takes place in regard to most, but not all, chromosomes, one nucleus may get more than its standard share of genes, and the other correspondingly less. If similar anomalies happen during formation of the nuclei of egg or sperm, too many or too few of the parental genes may be handed down to the offspring. Sometimes a chromosome loses a part, be it an end segment or a section from its middle. Thus cells may be formed lacking only a few genes normally present. Sometimes the "lost" region becomes in reality joined to another chromosome. In such cases, fertilization may result in offspring with a few additional genes present as compared to the parents.

All such changes in genic numbers are self-duplicating. The mysterious regulation which coordinates the duplication of all chromosomes and their genic material so that they all just double from mitosis to mitosis applies likewise to nuclei with changed chromosomal or genic number. The extra genes are reduplicated regardless of the fact that they are extras, and the loss of genes is no incentive to the cell to make up for it by more than duplication of the remaining genes.

The fact that gene mutations as well as chromosomal changes once having occurred are reproduced in their own form makes the distinction between the two at times very difficult. Coarse chromosomal changes involving whole chromosomes or large sections of them are of course easily discoverable under the microscope, but many chromosomal changes are very small, some of them discernible as minute losses or duplications with the best optical equipment only, and only in cells with unusually large chromosomes. Whenever the microscope fails to show a chromosomal change the question arises whether a submicroscopic change involving a whole gene or whole genes has occurred, or whether a real gene mutation, an intragenic change, was present. One answer to this question suggests that the question itself is wrongly put. Is it justified, the argument goes, to think of genes as fully separable units within the chromosomes, like peas in a pod, or should we conceive of them as integrated parts of much larger chromosomal units? The chemist knows that certain properties of a molecule are dependent upon the existence of a certain atomic group in one region, and other properties on the existence of another atomic configuration at some other position of the molecule. It would be mistaken to ascribe these properties exclusively to the atomic groups involved. These groups are functional as parts of the whole molecule only, and changes either in the groups considered or in other atomic groups of the molecule may result in similarly changed chemical properties. Similarly, it is reasoned, the discovery that specific functions of an organism are controlled by localized spots in the chromosomes does not necessarily mean that these spots, the genes, are independent molecular units. Whatever the final decision in this question will be, it remains justified to distinguish mutational events of coarse chromosomal nature from changes at a molecular level.

Both types of mutations are the raw material of evolution. This will appear obvious for the qualitative, molecular mutations, but may be surprising for purely quantitative changes in genic number. Why should simple duplication of genic material, or simple loss of it, make for new types of organisms? The answer may be given in terms of a comparison with an orchestra. If the members of the orchestra represent the assemblage of genes, then the sound of the symphony played by them may be compared to the harmony of development controlled by the genes. A change in the score of any player would change the symphonic unit. It would correspond to the effect of genic mutation. In addition, a change in the number of players of a given instrument or of a group of instruments would change the symphonic impression. Similarly a change in the number of genes, without change in their individual action, will change the balance of development and result in a new type of organism.

If the material of evolution is hereditary newness, created by recombination of genic varieties and of quantitative changes in gene numbers and

made possible by the existence of mutations, it still needs to be asked what is the moving force, the cause of occurrence of these mutations. The answer has sometimes been searched for in terms analogous to purposeful human thought. Because organisms are built and function as if a purposeful human mind had devised them—though infinitely more wonderful than any human mind could really have dreamt of—it has been speculated that a special force inside the organisms directed the occurrence of mutations so as to bring about their appearance and their specific type in response to "the needs of Nature." As methodologically doubtful as such views always were, we do not need to challenge them solely on the basis of general considerations. We have evidence for the statement that the cause of mutations may be seen not in the power but in the frailty of matter.

Mutations involving loss or duplication of whole chromosomes or coarse chromosomal parts are visibly the results of accidental breakdowns in those mechanisms which normally serve their orderly distribution. Gene mutations too are due to accident, on the molecular level. Accidental variations in the internal energy distribution of a gene may result in chemical reorganization, and accidental production in the cell of reactive atomic groups near the genes likewise may lead to such changes. "Accidental," of course, is not equivalent to "without causes." Causes not only exist but can sometimes be specified, as when radiation or temperature shock infringe on cells. The essentially accidental aspect of mutation consists in the lack of relation between the functioning of the organism as a whole and the intracellular change. Neither does the state of the organism direct in a specific way the occurrence of mutation, nor do the mutations usually, having occurred, guide the development of the offspring into well-suited new channels. All this is not surprising. When the geneticist recognizes that the material of evolution consists of entities at the molecular level, he must accept the consequence that changes at that level will be related in an indirect way only to the evolution of the organismal superstructure. A break in a peptide bond in a gene molecule or the replacement of a carboxyl group by some other radical may perhaps be dependent on some aspect of the physiological state of the organism, but the dependence seems at best very loose. The consequences of these molecular changes upon the development of an egg in which the changed gene molecule is present are fortuitous, as seen from the aspect of the necessary harmony of the existing species.

The problem of the evolution of ever new harmonious forms is thus different from that of the origin of hereditary newness itself. A hundred years ago this problem was recognized, so clearly, by Charles Darwin. His solution of it has reappeared, only slightly changed, in modern dress. Neo-Darwinism, which is the name for the genetic theory of the means of evolution, bases itself upon the existence of random mutations and analyzes the means of establishing successful genic combinations. This analysis is carried on in various ways by means of theoretical investigations, by observation, and by experimental procedures. Theoretically, it is possible to calculate how the genetic constitution of a population will change, given such factors as mutation, selection for certain gene varieties in preference to others, migration of parts of the population into new habitats or new ecological niches, or by random changes inherent in the fact that an element of pure chance enters into the processes by means of which the various gene varieties present in a group of parents are handed down to their young. Observationally, one may follow the changes which occur in natural populations, as for instance, snails, by taking a census of the frequency of various genetic types, from year to year or over longer periods. Experimentally, one may study over many generations the fate of artificially constructed populations kept in large breeding vessels called "population cages," into which are introduced known mixtures of individuals of different genetic constitutions, for instance of *Drosophila*.

The primary means of evolution as seen in these studies are natural selection and random change. The Darwinian principle of natural selection translated into genetic terms concerns the differential ability of different types to transmit their genic varieties or combinations of varieties to later generations. Sewall Wright, who has been one of the principal creators of the mathematical theories of evolution, has pictured the host of genetic combinations as being distributed over a symbolic landscape of peaks and valleys whose elevations indicate degrees of adaptiveness as measured by reproductive survival values. A species which by recombination of its genic or chromosomal varieties constantly produces new combinations explores, so to say, the field of adaptive peaks and valleys. Natural selection will hold a species on an adaptive peak, once it consists of a majority of individuals with the appropriate constitutions, and it will also guide it to other adaptive peaks if it happens to form new genetic combinations which are suitable for survival under different, usually ecologically different, circumstances. Moreover, the peaks and valleys of the symbolic landscape are not permanent

features. Geologic secular changes in the physical environment as well as changes in the living environment of a given species will make ill-adapted genetic types that were formerly well-adapted, and well-adapted formerly ill-adapted ones. Hills become dales and dales become hills. The task of natural selection thus is never-ending. In this view "progress" in evolution is mostly holding one's own while swimming against the stream!

In addition to natural selection of the random products of mutation and recombination, shifts in the genetic makeup of populations can be caused by nonselective events. If a group of individuals contains two or more varieties of a given gene and if these different varieties are all of equal survival value to the individuals which carry them, then one might not expect a change in the proportions of the varieties from one generation to another. While for instance, a specific A^1A^1 individual may accidentally have more offspring than a specific A^2A^2 individual, another A^1A^1 may have less offspring. So, random inequalities should even themselves out. This qualitative consideration is valid if the population is infinitely large so that the number of relatively infertile A^1A^1 individuals is exactly balanced by an equal number of relatively overfertile ones. No population, however, is infinitely large. In small populations it has been shown that chance can bring about great fluctuations in the proportion of genic varieties from one generation to the next. These fluctuations, due to accidental inequalities in reproduction and survivorship, are called genetic drift. Drift might even lead to complete accidental elimination of genic varieties, changing an A^1A^1, A^1A^2, A^2A^2 mixture of genotypes into one consisting of A^1A^1 or of A^2A^2 only. Genetic drift is a formerly unsuspected source of evolutionary change. It can account for the existence of non-adaptive neutral differences between populations. It might be added that an intense discussion is proceeding at present between students who uphold the importance of random drift and others who, while denying its theoretical possibility, disclaim that there ever exist truly selectively neutral genetic differences. Even should this doubt be justified, drift and natural selection would play simultaneously on the keyboard of evolution.

The picture of the means of evolution which emerges is one of an interrelated group of agents all of which may contribute to a shift from one genetic type present in the great majority of the individuals of a population to another. If most members originally could be given the formula A^1A^1 B^1B^1 C^1C^1 etc., with only a few A^2, B^2 and C^2 varieties present in some individuals, the various factors discussed above might well result in the establishment of a population of which most members might be A^2A^2 B^1B^1 C^2C^2 etc. A Species II would then have evolved from Species I.

There is room for differences in emphasis in the genetical theory of evolution. Neo-Darwinists usually stress the fact that most mutations or recombinations which lead to striking newness in form or function are selectively disadvantageous. In order to explain the equally valid fact that evolution does produce striking newness of an adaptive, advantageous nature, resort is made to assuming a gradual accumulation of greater and greater divergence by means of natural selection of many genetic varieties with individually minor effects. A dissenting minority view is held by others, who feel that essential newness in evolution is the result of such types of mutations as happen immediately to produce striking divergence from the original pattern of the species. What is a monstrosity as measured by the standard of the existing species may contain the hopeful element of becoming established, as a species well "pre-adapted" to a new mode of life. On this view the attainment of new adaptive peaks may not be the result of slow exploration of the adaptive mountain landscape but of audacious leaps from one peak to another. That the latter method is even more hazardous than the former is obvious, since a random leap from a peak has little prospect to lead to a successful landing on another. Yet, given sufficient numbers of such leaps, one of them may reach a peak which slow exploration might never find.

To the geneticist the majestic flow of evolution represents the outward calm of an unceasingly stirring world. Everywhere he discovers chance: chance in the origin of mutations, chance in their consequences upon development, chance in their shuffling into innumerable combinations. Indeed, the realm of chance is awe-inspiring. Granted the origin of life on the earth, the evolution of none of its specific, present or extinct forms seems to have been of a higher necessity. Their peculiar existence, our own existence, is or was accidental. Of the endless number of possible courses which the evolution of life could have taken, the few hundred millions of species which at most originated on our planet bear witness of only an unbelievably limited accidental sample. But perhaps more awe-inspiring yet than the chance aspect of evolution is the fact of evolution itself. Given the existence of matter in its elementary physical form, it was inherent in this matter to compound it-

self into self-reproducing elementary biological units. It was inherent in these genes to be able to elaborate superstructures: unicellular forms and multicellular plants and animals. That these superstructures actually are *Paramecium* and *Chlorella,* oak trees and mushrooms, earthworms and man was among the possible but not deeply necessary events, but that there *would* be evolution into superstructures, that there would be, if you want, successful escape from annihilation into some of the possible channels of change, seems to have been a fundamental endowment.

The geneticist who analyzes the material and the methods of evolution in terms of molecules and of chance is not by that token a callous fellow. To reproach him with lack of religious humility is nothing but a remnant of long worn-out pseudo-religious bigotry. One might wonder whether lack of humility is not present rather in those who try to evaluate the processes of the universe by the limited and changing standards of man's intellect and, shocked by the inapplicability of their standards, want to deny the existence of those processes. The geneticist's probing into the elementary events that underlie evolution opens up vistas into the methods of the world's being and becoming which must impress *all* faculties of the human mind.

OCTOBER

Now the autumn rains begin, and suddenly, where there were none, the fungi rise all through the woods. Not only the familiar stalk and gilled cap of the agaric family, but the brain puff balls, the little earth-measuring stars, sprawling their six points on the ground like black starfish. The old logs burst out with moon growths of white coral; the wet boughs are flecked with delicate tremulas, like shaking bits of dark wine jelly, and around the base of the oaks a great polypore has pushed out, a rich velvety mass of mouse-gray flounces, each ruffle bordered in deep black.—*An Almanac for Moderns.* Donald Culross Peattie, G. P. Putnam's Sons, New York. 1935. p. 219. Copyright G. P. Putnam's Sons, 1935. Reprinted with permission.

Thomas J. Howell taken in 1910 by Huron Smith. (Courtesy U. S. National Museum.)

Thomas J. Howell, Pioneer Oregon Botanist

ERWIN F. LANGE

The author is with the Portland State Extension Center of the Oregon State System of Higher Education, where he is teaching courses in the history of science and lower division chemistry. His hobby is collecting material about the scientific history of the Northwest.

ALTHOUGH great quantities of plant specimens were collected by the early scientific expeditions* into the Oregon Country and many publications resulted from these collections, not a single volume dealing with the flora of the great Northwest appeared before 1900. It remained for an amateur in the field of science, an adopted son of Oregon, to complete the first such work. This amateur, Thomas Jefferson Howell, is one of botany's least known investigators. Although a number of brief biographic sketches have appeared, none is complete and none lists all of his contributions to the development of botany in America. This paper summarizes many unpublished facts of Oregon's pioneer botanist.

Thomas Jefferson Howell was born in Cooper County, Missouri, October 8, 1842. He crossed the plains with his family and a group of immigrants. The party left Missouri in April of 1850 and, after six months of slow traveling in two wagons drawn by oxen, arrived in the Willamette Valley in October 1850. The Howells settled at Hillsboro in Washington County.

The next year, in order to take advantage of the then thriving river traffic, the Howell family settled on Sauvies Island twelve miles below Portland at the mouth of the Willamette River, an island visited by nearly all of the early scientific explorers. Two years later the father took on a 240-acre donation land claim and it was here that young Howell, with his two older brothers, John and Joseph, spent many years of his life. Here the Howells farmed and cut down trees to improve the land. At the death of Joseph Howell, also a botanist of considerable note, a Portland paper commented that the Howells "were steady and successful farmers."

* Beginning with Archibald Menzies, 1792; Lewis and Clark, 1805–6; David Douglas, 1825–30; Thomas Nuttall and J. Kirk Townsend, 1833–36; The Wilkes Expedition, 1838–42; and others.

The formal education of Thomas Howell consisted of three months schooling in 1855 in the first school on Sauvies Island. The remainder of his learning consisted of Howell's self-education in reading with the aid of his father. The latter, Benjamin Howell, was well educated and was a medical doctor by profession. His mother was Sarah Rittenhouse of the same family as David Rittenhouse, colonial mathematician and philosopher.

Since childhood young Thomas Howell had an interest in learning about plants. His search for literature to help him in identification of plants proved futile, as there was not a single book describing the flora of the Northwest. He could find plants described only in scattered references. About 1870 he began a systematic study of botany. In 1876 he started collecting and describing specimens. He also began to lose interest in farming.

In 1881, Howell published his first catalog, listing all of the known plants of the great Northwest. In 1887, a second pamphlet, 28 pages in length, entitled, "A Catalogue of the Known Plants of Oregon, Washington, and Idaho," priced at 25 cents, appeared. In the preface he says:

> In 1881 I published (as an advertisement) a list of all the species that I knew had been reported from the territory embraced in the present catalague (sic). At that time I could only find 1844 species and varieteis (sic). The present catalogue contains 2152 species and 227 varieties, 2379 in all; an increase of 535 species and varieties, or about 30 per cent in six years.
>
> No doubt the next six years will show a greater rate of increase, for the Botany of this region is but imperfectly known.

Howell spent the spring and summer of each of these early years searching for new species in the many wild and not easily accessible places of the Northwest. Because of the inadequacy of reference books and materials, the new plants discovered were sent to eastern botanical centers for deter-

mination and identification. The grasses were sent to George Vasey, Department Agrostologist with the United States Department of Agriculture, the sedges to L. H. Bailey at Cornell University, the Gamopetalae to Asa Gray at Harvard, and the Polypetalae to Sereno Watson, also at Harvard University. The botanical literature of the period contains numerous publications by Vasey, Gray, and Watson reporting on Howell's new discoveries. From these references it is possible to list the areas in which Howell collected during the various years. These are:

1879–Columbia River around Hood River
1880–John Day Valley in Eastern Oregon
1881–Southern Oregon
1882–Tillamook, The Dalles in Oregon; Mt. Adams in Washington
1884–Southern Oregon, from Coast Mountains to Siskiyou Mountains, and Northern California.
1885–Eastern Oregon, Wasco County, John Day, and Harney Valley
1887–Southern Oregon, Ashland and Grants Pass
1889–Southern Oregon, Grants Pass

In 1895 he spent two months botanizing off the southern coast of Alaska. Accompanying him on this trip was Martin Gorman of Portland, Howell's good friend and also a well-known amateur botanist.

In addition to the many trips taken during these years, Howell published a number of catalog leaflets (the writer found a number of surplus copies at the Oregon Historical Society library) which were sent to prospective customers. The buyer could check the plant specimens desired and return the leaflet to Howell. The prices quoted varied somewhat during the years, from eight to ten cents each for specimens purchased in small quantities and from four to eight cents per specimen in large orders. This practice was continued until 1896, when the last such catalog leaflet was published.

During the earlier years of Howell's botanical interests he made two of his most important discoveries. The first, made in 1878 jointly with his brother Joseph, was a pond plant growing in his home environment of Sauvies Island. Asa Gray[1] brought lasting fame to the Howell brothers by naming the plant Howellia aquatilia. In reporting the plant, Gray wrote, "It is dedicated to the discoverers who are assiduous collectors and acute observers and who have already much increased the knowledge of the botany of Oregon."

Howell's most important discovery was the weeping spruce, the last of the coast conifers to be found, but it does not bear his name. It was found in June, 1884, at a high elevation in the Siskiyou Mountains on the head waters of the Illinois River in northern California. The tree was named *Picea breweriana* by Sereno Watson[2] who said regarding the name:

> The specific name is given in compliment to Prof. W. H. Brewer, who in connection with the California State Geology Survey had so much to do with the botany of the state, both in the field and in the after disposal of the collections of the survey. As he took especial interest in the trees of the coast, and collected a large amount of material for their study, it is fitting thus to connect his name with the forest trees of California.

Travel in the Northwest was difficult during Howell's time and many hardships were endured during the lonely journeys in the often rough, out of the way places. The best extant description of two trips is found in the unpublished Memoirs of Louis Henderson,[3] a Portland school teacher and later a botanist at the University of Oregon. He describes the journeys with Howell in 1882 as follows:

> Thomas Howell, then living at the homestead on Sauvie's Island, just where the Willamette joins the Columbia, was a great friend of mine, and I had visited him frequently, rowing down the river from Portland, staying with him, hunting ducks and geese or studying plants Saturdays and Sundays till it was time for me to row back to Portland for my classes on Mondays. So in 1882, he proposed our taking a trip together in his express wagon, first down to Tillamook Bay, then back and up to Mt. Adams. We set out a bright day in June for McMinnville, thence crossed the Coast Range by the old wagon road over the mountains, and camped one night on the Trask. While catching a mess of trout for supper, it was my good fortune to run onto a peculiar Rosaceous plant about 2 feet high. I took it back to Howell to see whether he knew it, as he had collected in the Coast Mountains far more than had I, and consequently was more acquainted with the flora. This was the first time he had seen it, so we collected it in quantity. Later we sent it on to Drs. Gray and Watson, and the latter named it FILIPENDULA OCCIDENTALIS. By many this genus has been reduced to SPIRAEA, but FILIPENDULA holds the field again. The next day we were soon in the town of Tillamook, consisting of but few houses at that date. We left our team of horses and wagon at a livery stable, hired a row-boat, and were soon rowing down to the spit, where now is Bay Ocean; but at that early day there was not, as I remember it, a single habitation showing from the water on the whole bay, save a little hamlet or a very few houses at Garibaldi. We went into camp just above high tide, and soon found that the "fly in our ointment" was to be on the one hand mosquitoes, on the other lack of good water. The first we had to endure day and night; the second we overcame by digging with our spade a hole in the sand close up to the spruces, where moist sand alone showed the presence of underground water. Here we lived for 2 days, literally *combing* the dunes, tide-lands, and even shallows for specimens. Most of the plants we gathered were already known to the books, but a few were new species, as we afterwards found out. Among these were the grasses *Poa macrantha* and *Poa confinis*, both named by Vasey, and *Sanicula howellii*, of Coulter and Rose. At the end of our two days, wishing for a good meal of rock-oysters, for which we had heard Garibaldi was noted, we

broke camp and rowed across the outlet to the bay. Very foolishly we took a strong ebb-tide, and only by most vigorous rowing did we escape being carried out to sea. After dinner we waited for an in-coming tide, and made our way back to Tillamook. The row back, as well as out, though long, was hardly tiresome, so busy were we in noting the bird life. In fact, at this early day, our advancing boat was always heralded by flocks of ducks which rose in front of us, circled and then dropped just back of us. The whole surface of the bay was almost covered by these hordes of ducks of dozens of species. We returned to Sauvie's Island to dry out and deposit our large collections, and then proceeded up the Columbia, mainly by wagon over the old road, and finally reached the ferry at Hood River. Near the great rockslide we discovered on the rocks that peculiar, light-colored grass, known as CALAMAGROSTIS HOWELLII, and named by Vasey. The next day we were across the Columbia and on the way to Camas Prairie. On reaching the north end of the valley, we turned westwardly towards Mt. Adams, expecting to find the "good road" for our team and wagon, about which we had been so repeatedly informed since leaving White Salmon. While the yellow pine woods lasted, all went well, the trail forcing us only occasionally to go around some fallen tree, which was easily accomplished in those open, grassy woods. As we ascended the mountain higher and higher, the pine woods gave out, while fir, white pine and mountain or black hemlock took their places. More and larger trees were now across the trail. At first we tried to go around these, but the brush becoming at times impenetrable, we became exasperated by the delays, and finally Howell himself made a most surprising proposition. It was that we both get out and walk, he driving the team and I catching hold of a wheel and helping team and wagon *over the logs!* As the wagon itself was comparatively new and well-made we thought we ran little risk of breaking it to pieces, especially as our load was almost of no weight. So we proceeded for several miles, he whipping up the team as we came to a log, which was practically every minute or two, the horses jumping the log with their front legs, then yanking over the front wheels as they cleared the log with their hind legs, and then, with bumping and crashing, pulled over the back wheels, I all the time assisting with arms and shoulders to keep the team going. This we found was the main point, for if they halted when only the front wheels were over, it was almost impossible to get the hind wheels to follow, if the log was several feet through, as many of them were. Thus, by very exhaustive work, we were able to reach the snow line and a most beautiful camping spot by night. And the glory of those subalpine and alpine slopes of Mt. Adams at that early period (sic). Stock, especially sheep, had not ruined the native pasture at that time, and there were succulent bunch and other grasses up to your knees. Now these grasses have largely vanished under over-pasturing, and often you can barely find any sustenance for your horses if you wish to camp there. Probably the most beautiful and succulent of these grasses is FESTUCA VIRIDULA, then a new species and found by us for the first time on Mt. Adams, though Suksdorf, who was up there at the same time with a band of sheep, first sent it to Vasey. This grass and some of the other bunch-fescues and Poas were then so abundant on the open slopes, that a horse when picketed amongst them by a 40-foot rope would eat his fill and lie down without finishing his forage within the radius of his rope. Now one has often to travel miles before he will see a stalk of these grasses and then only when protected by rocks or brush. On this same trip we likewise found the then unpublished prickly Gooseberry, named RIBES AMBIGUUM by Watson, but later changed to RIBES WATSONIANUM. Here we camped for nearly a week, sometimes working together, and as often solitary on our tramps. Many of the plants were old friends I had seen in '78 and '79, but most of them, on account of later season, were new to both of us, and hundreds of plants were added to our collections. As Thomas Howell was supporting himself almost wholly by the sale of his collections, while I had my regular teacher's salary, I always allowed *him* to send off what collections went East for naming, as anything new, or even rare, brought the most money in the plant market.

The published papers of Howell were confined to two western periodicals, *Erythia,* published by Willis Jepson at the University of California, and the *Mazama,* published by the Mazamas, a Portland mountaineering club. In both instances, Howell had the honor of making a contribution to volume one of the respective publication. However, the paper which received the greatest attention was "The Flora of Mount Hood," appearing in volume one, number one (1896), of the *Mazama* magazine. This article was primarily a list of some 272 species of plants growing in the Mount Hood area above the altitude of 2000 feet. The article had long been forgotten when it appeared in Senate Document number sixteen of the second session of the Seventy-first United States Congress. This document, entitled "Public Values of the Mount Hood Area," was prepared by a special committee appointed by the Secretary of Agriculture to make a study of the features of major public importance in the Mount Hood area and was presented June 9, 1930, by the late Senator Charles McNary of Oregon. Howell's article was included in the appendix with the comment that the list, though incomplete, was the only one available.

Howell's main objective over the years was to compile all of the information regarding Northwest plants into a single, usable flora. The actual writing of this began in 1882 and was completed in 1896. As the time for publication arrived, a new obstacle presented itself; the printers in Portland were unable to set the type because of so many technical words; however, Howell was not to be discouraged. He carefully learned to set type with his own hands; then at his home, he laboriously set eight pages at a time, after which he took them to Portland to be printed. Both the writing and typesetting were difficult for one with only three months of formal schooling. Again, his good friend Martin Gorman helped by correcting copy and reading proofs. Extant letters of Howell's on file in libraries abound in errors, and several of his contemporaries

Brewers weeping spruce. Gold Basin Range, Siskiyou National Forest. (U. S. Forest Service.)

commented on his errors and poor handwriting. Martin Gorman commented that Howell improved considerably by writing his account of the Northwest flora.

On March 15, 1897, the first fascicle of 112 pages appeared under a paper cover, priced at fifty cents. After examining this first fascicle, Willis Jepson[4] wrote a rather lengthy review in his magazine *Erythia*. After noting some minor errors and discussing a few species, he praised the work by saying:

It is evident from a perusal of the pages that Mr. Howell's work is cyclopedic rather than critical. What he has done has been to bring together in usable form, in the light of his field knowledge (and no other botanist knows so well the plants of these states) all that has been published concerning the flora of the region. But it should, of course, be added that very many diagnoses are his in whole or in part, and that his personal observations color the completed product. Even so much is a task of no small magnitude. The author has not spoken of difficulties, but difficulties must have been many in a region in which library and herbarium facilities are meager. Mr. Howell, therefore, deserves no small meed of praise for the courage and resolution necessary in the face of such circumstances, and we trust that he may finish his volume within the limit of the period contemplated—that is, by the end of the present year.

However, the book was not to be completed during the year. Six and a half years after the first fascicle was completed, the work finally made its appearance under one cover, after seven separate fascicles had been published, each selling for fifty cents. In 1902 Howell wrote in a letter that he was spending eighteen hours a day in an effort to complete his book.

The completed book, *A Flora of Northwest America,* was described as an octavo volume of 792 pages, plus 24 pages of index. About a million words were required to describe 3150 different species, of which 89 were new to science. An original literature reference was included for almost every species. The volume was priced at five dollars but was a financial failure for the author. Today the book is so rare that to find one for sale at five dollars would be almost a miracle; one copy is reported as having sold for $37.50. While rare on the book market, the "Flora" is widely distributed in the libraries of the large universities, and many libraries in Oregon have one or more copies.

After publication, Howell's "Flora" drew attention in various quarters. The *Botanical Gazette*[5] commented:

Howell's Flora of Northwest America, which has been in course of publication since 1897 is now completed. The author has struggled against difficulties in producing this work, for which he himself has set the type. Those who have used the parts in the field have found it exceedingly useful.

At the meeting of the Torrey Botanical Club, April 12, 1904, Dr. P. A. Rydberg presented a paper on "The Flora of Northwest America," the review of which appeared in *Torreya*. Dr. Rydberg[6] commented on some errors and then, referring to the work, wrote:

Only those who have been actively engaged in writing manuals of systematic botany can imagine what such an undertaking means, what difficulties are met with and what an amount of work is needed. If the fact is taken into consideration that Mr. Howell had to work far away from libraries with scarcely any other facilities than those afforded by his private library and collection the excellence of the work is really surprising.

The Portland *Oregonian*[7] in a feature article finished concerning Howell's "Flora":

. . . [that it] is not only a monument to the scientific knowledge and patient industry of the author, but a credit

to the State of Oregon as well. . . . Mr. Howell had all the patience and perseverance necessary to sustain him through these long-continued and unremitting labors and hardships without once faltering in his design, and it must be remembered that it was purely a labor of love, without hope of remuneration at the end worth considering.

Some years later, in writing Howell's life for the *Dictionary of American Biography,* Jepson[8] wrote regarding the first Northwest flora:

Although thus handicapped, he had a sound and just comprehension of what was needed, and he organized diagnosis of genera and species scattered in the works of many writers into a pioneer flora, which considering the circumstances of its production is balanced, judicious, and highly useful. Even after more than a quarter of a century it remains the only flora for the three states which it covers.

After the writing of the "Flora" had been completed and the publication of the book was drawing to completion, Howell decided to give his large collection of plant specimens to the University of Oregon. In a report to the board of regents for January 20, 1903, P. L. Campbell, president of the University, requested that $500 be paid Howell for the job of arranging and classifying the collection. That fall, Thomas Howell was listed as a collector in the department of botany, among the faculty listings in the school catalog. R. S. Bean, president of the board of regents of the University, in his biennial report for 1903 and 1904 to the governor of Oregon and the twenty-third legislative assembly (1905), wrote concerning the collection:

The most notable acquisition by the University during the year has been the invaluable collection of type specimens of the flora of Oregon contributed by Thomas Howell. The great value of this collection, the best certainly in the Northwest, will become more and more fully recognized as time goes on.

Each year since 1904, as a new University of Oregon catalog appeared, the name of Howell has been perepetuated by recognizing his gift of 10,000 specimens to the University's herbarium.

Not only does the University of Oregon possess specimens by Howell, but large numbers are to be found in herbaria over the entire United States. Between 2200 and 2300 specimens are deposited in the Chicago Natural History Museum and large numbers are in such centers as the United States National Herbarium, the Gray Herbarium at Harvard University, The Britton Herbarium of the New York Botanical Garden, the Jepson Herbarium of the University of California, and undoubtedly others.

All who knew Howell spoke of his poverty. At least two botanists of note came to Portland to talk and visit with Howell. In 1906, Willis Jepson came to Portland and rode for forty minutes on the electric interurban trolley toward Oregon City to the place where Howell was building a house in a new clearing. Of his poverty, Jepson[9] wrote in his field book:

It is too bad to see him [Howell] so miserably poor. He came into Portland with me and I insisted on his taking lunch with me but he would allow only a few simple things to be ordered for him.

Martin Gorman reported to Jepson[9] that his poverty was due to:

. . . [his having] lost the money from the sale of his share of his father's estate in unfortunate investments. One man promised him an income of $300 a year, $25 a month, if he would put in $3000. Mr. Howell felt that he could live on $25 a month in a simple way and work on his flora. But he never got back a cent on either interest or principal. Another man who was looking for suckers got him into a laundry business scheme, and in addition got his signature to certain notes for machinery and then skipped out.

In October, 1910 Huron H. Smith, Dendrologist of the Field Museum of Natural History of Chicago, made a special trip to Portland in order to spend a day with Howell. At that time Howell was living on Hood Street in Portland where he had a small grocery-confectionary store, which was also his residence. Smith[10] reported on Howell's limited finances and said that Howell was spending his time making coarse teamsters' mittens on a sewing machine for seven cents a pair. However, in spite of his poverty he was "very cheerful at all times and betrayed no impatience with depressing external conditions."

During the early years of Howell's interest in botany he lived on the old homestead on Sauvies Island and engaged in farming with his two brothers. From February, 1873 to August, 1876 he served as postmaster of the Willamette Slough Post Office on Sauvies Island. The name of this post office was later changed to Arthur, Oregon, and this address appeared on Howell's catalogs until 1894. After 1895 his catalogs were issued from Clackamas, Oregon. From December, 1904 to March, 1906 he again served as postmaster, this time of the Creighton Post Office, which is today the Oak Grove Post Office. During the intervening years he operated grocery stores in Clackamas, Milwaukie, and finally on Hood Street in Portland.

Howell was married in 1892, at the age of 50, to Effie Hudson. They had two sons. Jepson,[9] after visiting with Howell, described him as "a man below medium height, his hair brown and gray, shortish full beard, reddish face, blue eyes, slightly Roman Nose."

Thomas Howell died December 12, 1912, after a long illness and only two months after the death of

his older brother Joseph, also a contributor to the knowledge of Northwestern botany. In writing his obituary, a contemporary botanist, Edward L. Greene,[11] who has also reported some of Howell's discoveries, extended him the highest tribute in these words:

> That which is most peculiar and noteworthy about Mr. Howell's career is, that he accomplished the greatest amount of meritorious and valuable scientific work that was ever done by any man of any epoch, on so very rudimentary an education in letters.

And Jepson[8] wrote of Howell:

> Few men leaving a durable contribution to American botany have led so obscure an existence as did Howell.

While the great names in American botany are memorialized by having botanical clubs, periodicals, and herbaria named after them, Thomas J. Howell, uneducated and not affiliated with a large university, is almost forgotten except by botanists and the botany students who find his "Flora" on a library shelf or who are impressed by the 30 Northwest species and varieties bearing the name *howellii.*

Thomas Howell's Publications:

1. Catalogue of the Flora of Oregon, Washington, and Idaho (1881).
2. A Catalogue of the Known Plants of Oregon, Washington, and Idaho (1887).
3. A Rearrangement of American Portluceae. *Erythia,* **1,** 29 (1893).
4. New Plants of Pacific Coast. *Erythia,* **1,** 109 (1893).
5. Note on Sedum Radiatum. *Erythia,* **1,** 144 (1893).
6. New Species of Pacific Coast Plants. *Erythia* **3,** 32 (1895).
7. Distribution of Darlingtonia in Oregon. *Erythia,* **3,** 179 (1895).
8. The Flora of Mt. Hood. *Mazama,* **1,** 28 (1895).
9. The Flora of Mt. Adams. (Co-author, William N. Suksdorf). *Mazama,* **1,** 68 (1895).
10. *A Flora of Northwest America.* Portland, Oregon, Aug. 10 (1903).

References

1. GRAY, A. *Proc. Am. Acad. Arts Sci.,* **15,** 43 (1879–1880).
2. WATSON, S. *Ibid.,* **20,** 378 (1884–1885).
3. HENDERSON, L. *Memoirs* (unpublished), p. 12. Eugene: Univ. of Oregon Library.
4. JEPSON, W. *Erythia,* **5,** 66 (1897).
5. *Botan. Gaz.,* **36,** 480 (1903).
6. RYDBERG, P. A. *Torreya,* **4,** 70 (1904).
7. *Work of a Lifetime.* In the Portland *Oregonian,* p. 12 (Jan. 25, 1904).
8. JEPSON, W. In *Dictionary of American Biography,* Dumas Malone, Ed., **9,** 305 (1932).
9. ———. *Field Book* (unpublished). Berkeley: Univ. of California, Jepson Herbarium, **7,** 108 (1902); **16,** 86 (1906).
10. SMITH, H. H. *Botan. Gaz.,* **55,** 458 (1913).
11. GREENE, E. L. *Am. Midland Naturalist,* **3,** 30 (1913).

Circulatory Adaptations to Birth

S. R. M. REYNOLDS

Dr. Reynolds received his A.B. and M.A. at Swarthmore College, and his Ph.D. at the University of Pennsylvania. He has taught physiology at Western Reserve University and at the Long Island College of Medicine. In 1941, he joined the Department of Embryology, Carnegie Institution of Washington. He has held two Guggenheim Fellowships. Much of the work on which the following article is based was done during his second fellowship, when he worked at the Nuffield Institute for Medical Research, Oxford University.

AT the moment of birth, a series of related and interdependent changes takes place in the circulatory system of the fetus. This is necessarily so, since the requirements of the fetus demand one set of specialized circulatory conditions and those of the newborn infant require another, very different, set of conditions. Nature usually contrives to achieve a normal and orderly change from one to the other. The mystery of the mechanism of vascular adjustment deepens and the wonder grows as one contemplates the complex but orderly changes that make possible the abrupt transition from a dependent life in a fluid medium within the womb to an independent life in an environment of air.

The basic anatomical differences between life in the two conditions have been known since the days of William Harvey in 1628, although anatomists of an earlier time, extending back to the days of Galen in the second century A.D., knew of one or another of the essential differences.[1] In terms of development, the fetus is an aquatic organism, deriving its oxygen and sustenance from maternal blood and giving off its waste products to it, across the tissues and structure of the placenta. The newborn infant, like the older adult, derives its oxygen through its lungs and gives off its waste carbon dioxide across the tissues of the pulmonary barrier. The change in mode of respiration depends upon extensive alteration in the course of blood flow throughout the organism. The nature of this change-over has been established only recently. What is the basis of these adjustments?

The heart receives blood through the large veins that enter it and distributes blood to the arterial system. In both the fetus and newborn, blood enters the heart by way of the right auricle (a word meaning "ear"), or the first chamber of the heart. After birth, blood passes from the right auricle into the right ventricle ("little belly") and thence into arteries that carry it to the lungs, where the respiratory exchange of gases is effected. The oxygenated blood then returns through veins to the left auricle of the heart and thence to the left ventricle, from which it is sent through the branching arterial system to all parts of the body.

In the fetus, the blood that flows in the veins coming from the lower parts of the body is in large part composed of oxygenated blood that has entered the body of the fetus through the umbilical vein. Hence it is the "best" blood in the fetal body. Reaching the heart, it passes in one of two directions. The blood that strikes one side of a recently

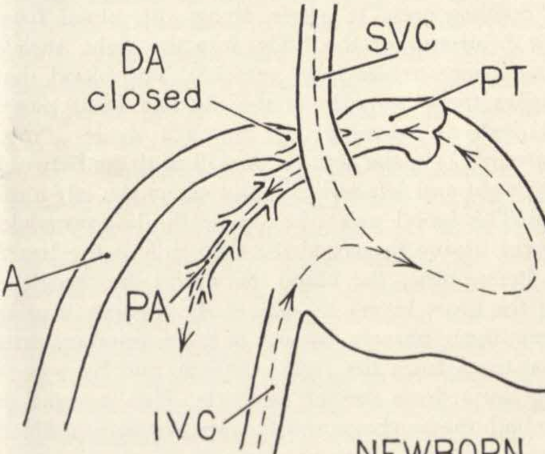


FIG. 1. Scheme of the course of blood flow through the postnatal heart. Venous blood from the superior vena cava (SVC) and inferior vena cava (IVC) enters the right side of the heart. The right ventricle ejects it into the pulmonary trunk (PT), from which it goes to the lungs by way of the pulmonary arteries. Not shown is the return of blood from the lungs to the left side of the heart and thence into the aorta.

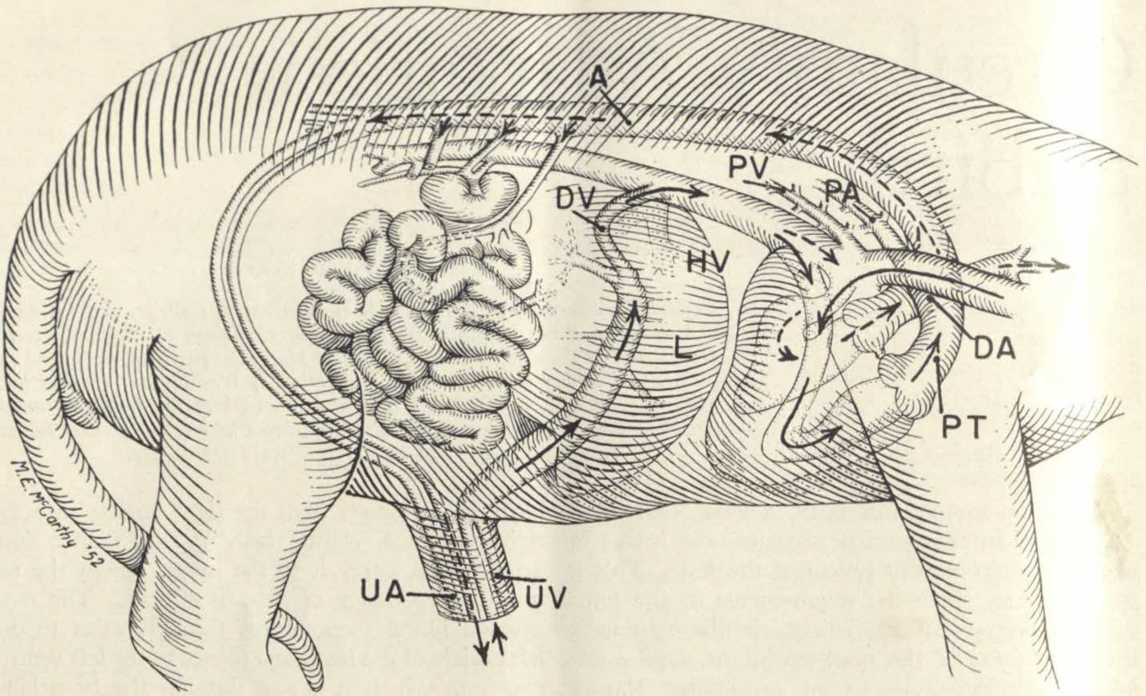


FIG. 2. Schematic diagrams of the course of the circulation of blood in the fetal lamb. Blood, oxygenated, enters the body in the umbilical vein (UV) and goes to the liver (L). It continues through the liver to the heart (shown enlarged at the right of the chest) by either of two channels: (a) by a direct connection, the ductus venosus (DV), or (b) an indirect route, through the hepatic veins (HV). At the heart, the blood stream divides. Part of it goes to the right side of the heart, to leave by way of the pulmonary arterial trunk (PT). The blood stream enters either the arteries going to the lungs or the ductus arteriosus (DA), a connection between the pulmonary vessel and the aorta (A), the large systemic vessel along the back. Thence the blood is carried to organs of the abdomen, the lower part of the body, and the placenta via the umbilical arteries (UA). The blood that goes to the left side of the heart leaves by way of the aorta and passes to the head as well as to the lower parts of the body. Pulmonary artery (PA) and pulmonary vein (PV) carry little blood in the fetus.

recognized promontory of tissue, the crista dividens ("dividing crest"), passes, along with blood from the head end of the body, into the right auricle and thence to the right ventricle. The blood that strikes the other side of the dividing crest passes through an opening, the foramen ovale ("oval opening"), in the septum or wall of tissue between the right and left auricles, and enters the left auricle. This blood passes in turn to the left ventricle, hence having by-passed the right side of the heart.

Before birth, the blood that enters the ventricles of the heart leaves by way of the arteries leading from them, namely, by way of the pulmonary arterial trunk from the right ventricle, and by way of the aorta, from the left ventricle. This is also true in both the newborn and the adult organism. However, a second difference between the two types of circulations lies just beyond this point. About an inch from where the main pulmonary arterial trunk begins, it divides into three branches. The two smaller branches go to the lungs, one to the right and one to the left. Lying more or less between these two branches, the main pathway continues, for a short distance, to join the large aorta arising from the left ventricle of the heart. Here, then, is a direct and major connection between the two great arterial systems of the fetus. This connection is known to anatomists as the ductus arteriosus. Only a relatively small quantity of blood finds its way into the nonfunctioning lungs of the fetus from the pulmonary artery, instead of going into the ductus arteriosus; this returns to the heart through the pulmonary veins. It enters the left auricle where, joined by the blood coming through the foramen ovale that connects the two auricles, as mentioned above, it passes into the left ventricle, to be discharged into the aorta.

In infants and adults, the two arterial systems, that arise from the right and left sides of the heart, respectively, are quite separate. Prior to, and until the moment of birth, however, they are connected by the ductus arteriosus; thus a large portion of the blood that leaves the right ventricle joins that coming from the left ventricle. This mixed stream passes to the lowermost regions of the body, where it supplies the abdominal organs and the legs. Mainly,

however, it goes to the placenta through two large arteries that pass from the lower part of the aorta into the umbilical cord. The main purpose of this part of the circulation is to assure the fetus an abundant and continuous circulation of its blood through the placenta where, in fetal life, the vital processes of respiration, assimilation, and excretion take place.

The cardinal structural differences between the fetal and newborn circulations are now clear. They are basically three in number: (1) the abundant and important placental circulation in the fetus; (2) the crossover in the fetus, by way of the foramen ovale, of blood between the two auricles or receiving chambers of the heart, and (3) the shunt by way of the ductus arteriosus between the two arterial systems, which arise respectively from the two sides of the heart. At the moment of birth, each of these structures—placenta, foramen ovale, ductus arteriosus—becomes nonfunctional; and from that time on the newborn organism normally breathes in the adult fashion, with all that this entails in readjustment of the vascular system to the altered conditions of life. These are structural and functional changes that must, and do, occur with remarkable rapidity.

Before consideration of the timing and cause of the change-over from the fetal to the newborn condition, two points should be mentioned in passing. First, for what purpose, the reader may ask, does some of the blood freshly charged with oxygen in the placenta return to the heart and pass through the foramen ovale into the left side of the heart? We saw above that all of the blood remaining on the right side of the heart goes to the lower part of the organism, particularly to the placenta. The blood from the left side of the heart enters the aorta on its way to meet the stream of blood coming through the ductus arteriosus from the right side of the heart. On its way, some of the blood leaves the aorta by appropriate arterial branches to go to the developing brain, the head, and the remaining forepart of the animal. Since these are the first branches to leave the aorta, above the point where it is joined by the ductus arteriosus, the crossover of blood between the auricles in the heart assures that the brain in particular will receive, almost directly, blood that is rich in oxygen and nutrient substances and low in the waste gas, carbon dioxide. A more efficient arrangement can hardly be imagined.

The second point to consider is concerned with the character of blood flow from the placenta to the heart. As the blood moves in great volume and at an appreciable velocity through the umbilical vein, it reaches the fetal liver. Here, the blood returns by two paths to the large vein, the inferior vena cava, in which it is carried to the heart: it may go by an indirect route through the blood channels of the liver and thence by way of veins draining the liver to the inferior vena cava, or it may go by means of a direct connection (another shunt, termed ductus venosus) from the umbilical vein to the inferior vena cava and so to the heart.[2]

The course that the blood takes in traversing this part of the vascular system is governed by a muscular mechanism that can open or close and which is located at the begining of the ductus venosus. When it is open, little blood goes by way of the liver; when it is closed, all the blood goes by way of the liver. The purpose served by such a mechanism of control remains to be discovered. As of today, one can say that the controlling sphincter muscle seems to be sensitive to fluctuations in blood pressure within the umbilical vein, and closes when a large volume of blood rushes back from the placenta as it does when the maternal uterus contracts. Closure of the ductus venosus diverts blood into the liver, which therefore acts temporarily as a reservoir to store the excess quantity of blood and so to prevent overloading, and perhaps embarrassment, of the fetal heart.

Whatever the purpose and exact character of this elaborate mechanism ultimately may be found to be, it ceases to exist after the organism is born. It, like the other three mechanisms mentioned above, has fulfilled its purpose at birth, and so it disappears with the beginning of that life which depends upon ventilation of the lungs. These are indispensable mechanisms during fetal life, but necessarily dispensable immediately afterward.

The nature of the anatomical change-over at birth that is associated with the circulation of blood, is now clear. The pathways of the blood flow in the fetus are also evident, and accepted by students of the subject. However, the acceptance of these facts had to await their clear demonstration in 1945 by a group of investigators at the Nuffield Institute for Medical Research, Oxford, England. In that year, A. E. Barclay, K. J. Franklin, and M. M. L. Prichard published their now classic monograph on the fetal circulation.[1] In it they presented the proof that established the pattern of blood flow which can now be stated with the certainty outlined above. This was a noteworthy advance in our understanding of life processes; it seemed then that little else could be added to our understanding of the subject. Nevertheless, crucial questions remained unanswered. Why, for example, do the ductus arteriosus and foramen ovale close? When do they close? At once, with onset of breathing, or after some delay? Together, or singly?

In 1951, in the course of other studies at the Nuffield Institute for Medical Research, using fetal

FIG. 3. Two pictures of the ductus arteriosus. Left, from the fetal (nonbreathing) lamb; right, from the newborn (breathing) lamb. The ductus arteriosus (DA) is open on the left, constricted on the right. Other labeled structures are the heart (H), the lung (L), the pulmonary artery (PA) and pulmonary arterial trunk (PT), and the aorta (A). The ductus arteriosus connects the two latter. (From *The Foetal Circulation,* by Barclay, Franklin, and Prichard.)

lambs, these and other theretofore unthought of questions were answered. The answers revealed a beautifully timed and integrated group of circulatory adaptive mechanisms associated with birth. These involve changes in blood pressure and the rerouting of blood flow throughout the organism. The trigger for the complex group of mechanisms is inflation of the lungs and a change in the amount and character of the blood flowing through them.

These new observations could be made in 1951 and not in 1945 because of a radical improvement in the method of photographing the fetal circulation. In the earlier work, cineradiographs, or x-ray movies, were taken at a frequency of three to six pictures a second. This was too slow to permit them to be viewed as regular moving pictures. In the later work, Dr. G. M. Ardran arranged to take pictures at a rate of twenty-five frames a second, a speed well-suited to studying the movements of blood throughout the organism. Blood pressure was also recorded.

In the fetus, as noted above, blood flows from the pulmonary arterial trunk to the aorta through the ductus arteriosus. One may assume, therefore, that the blood pressure in the pulmonary trunk is higher than in the aorta, into which the blood flows. In the adult organism, i.e., in the absence of the ductus arteriosus, pulmonary arterial blood pressure has long been known to be very much less than aortic blood pressure. Presumably this change takes place at birth or shortly afterwards.

Attempts to measure the changes associated with onset of respiration by W. F. Hamilton, R. A. Woodbury, and E. B. Woods at the University of Georgia in 1937, failed to reveal the course of such changes of blood pressure, as did earlier observations of the German workers, J. Cohnstein and Zuntz, in 1884 and 1888. One worker, B. S. Schulze, in 1871 suggested, without proof to support his contention, that an increased vascular capacity of the lungs with aeration, coupled with closure of the ductus arteriosus, would cause a fall in systemic blood pressure, prior to its subsequent rise. This, as we shall see, so clearly presaged the recently made observations that it is regrettable that Schulze cannot have the satisfaction of seeing how men whom he never knew are today "marching to the measure of this thought."

Throughout the intervening period, from 1871 until the present day, stress has been laid almost exclusively upon the cause and effect of closure of the ductus arteriosus in the circulatory adjust-

ments to postnatal life. That this adjustment would be secondary to another one seemed unlikely, since the position of the ductus arteriosus was so prominent and since it is so unique a structure in the fetus. We now can put closure of the ductus arteriosus in perspective against the total complex of changes taking place at birth. Let us see why this is so.

A new group of workers were associated during 1951 in a reinvestigation of this problem at the Nuffield Institute in Oxford, England. The radiologist, Dr. G. M. Ardran, a physicist, Mr. D. G. Wyatt, and physiologists, including Drs. G. S. Dawes, M. M. L. Prichard, and the present writer, combined to make a team.[3] Experiments were carried out on fetal lambs delivered from anesthetized ewes near term. Breathing was prevented by applying a fluid-filled rubber bag over the nose, and allowing the lamb, also anesthetized, to remain attached by its umbilical cord to the placenta which was still in place within the womb.

In this way, it was possible to work on the lamb as it remained in an essentially fetal condition. Blood pressure was recorded at each end of the ductus arteriosus, by means of small cannulas thrust through the pulmonary trunk and aorta and connected to capacitance manometers. The flow of blood was recorded by taking x-ray moving pictures of the entire fetus at a rate of twenty-five pictures

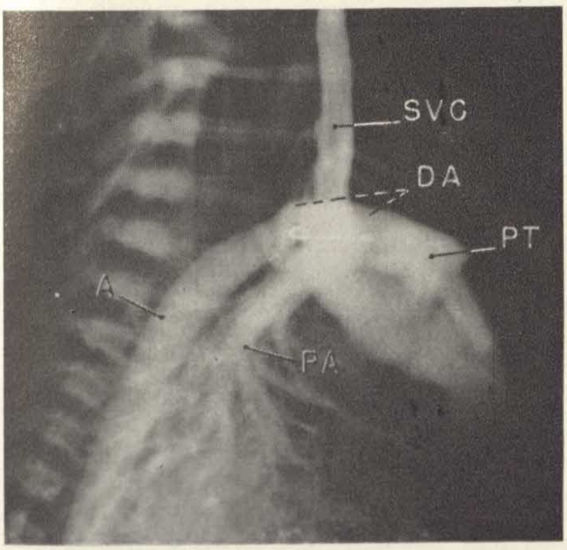


FIG. 4. Path of contrast medium injected from the head end of a fetus shown in a cineradiograph. A wire had been sewn on the ductus arteriosus. The contrast medium passed through the superior caval vein (SVC) entering, only the right side of the heart. Thence it went to the pulmonary trunk (PT), through the ductus arteriosus (DA) and into the descending aorta (A). Some also passed into the pulmonary artery (PA).

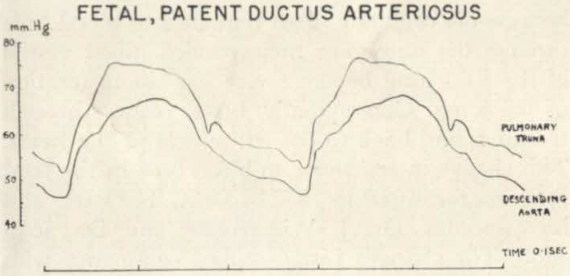


FIG. 5. Two pressure pulsations in the fetal condition, from the fetal heart in the pulmonary arterial trunk (top) and the descending aorta (bottom). The pressure in the former always exceeds that in the latter. (G. M. Ardran, *et al. J. Physiol. London,* 1952.)

per second. A suitable contrast medium was injected in such a way as to display the various parts of the vascular system requiring study, especially the pulmonary arteries and the ductus arteriosus. After observing the blood flow and blood pressure in the lamb in the fetal condition, aeration of the lungs was permitted. At suitable intervals afterward, the pressure and flows were determined, again and again. In this way, comparison of the state of the circulation with the passage of time after the onset of aeration of the lungs could be readily evaluated.

The results were dramatically clear. Prior to aeration of the lungs, the pressure at the beginning of the ductus arteriosus in the pulmonary artery exceeds that in the aorta, at the other end of the ductus arteriosus. The flow of blood through the lungs is very slow and exceedingly small in amount. Therefore, most of the blood from the right side of the heart enters the aorta through the ductus arteriosus, where, in moving pictures of it, one may see the two streams mixing turbulently as the blood rushes along the aorta toward the placenta. This is in the fetal condition.

When ventilation of the lungs begins, there is at once a profound fall in both the pulmonary arterial and aortic blood pressures, just as Schulze predicted in 1871. The fall in blood pressure coincides with and depends upon an enormous increase in the velocity and volume of blood flow into the lungs as the minute blood vessels of the lungs expand and fill with blood. The faster flow into the lungs at a lower pressure obviously means that the resistance to blood flow in the lungs diminishes upon their aeration. While the blood pours into the lungs, there is a reduction in the return of blood to the heart. This accounts for the fall in blood pressure in the systemic arterial system described above. Inasmuch as the lungs fill up after a few minutes, blood returns to the heart, and so the level of systemic blood pressure recovers. In the pulmonary

system, however, the blood pressure remains low because the originally high resistance to blood flow through the numerous nonexpanded blood vessels of the fetal lung becomes very low upon aeration of the lungs. Consequently, blood rushes through the lungs and back to the left auricle of the heart. This change in resistance to blood flow in the fetal lung was measured in 1952 by Dr. G. S. Dawes and his associates, Dr. J. Widdicombe and Dr. Joan Mott. The changes just described commence with the first filling of the lungs with air, and usually become stabilized within ten to twenty minutes.

What happens to the ductus arteriosus at the time when these crucial changes take place? At what time does it close? Cineradiographs show that it effectively closes within a minute or two after the onset of ventilation of the lungs. It closes when the blood pressures in both arterial systems are at their lowest points. Why should this be so? The answer to this question may be stated today only in hypothetical terms. The best explanation is as follows: normally, the ductus arteriosus, a distended structure containing elastic and muscular tissues, tries to close at all times, but it is held open by the high blood pressure within it. This results from blood being forced through the ductus arteriosus during fetal life, where it is deflected by the high resistance to flow in the lungs. With aeration of the lungs, resistance to blood flow decreases, as described above, and in consequence blood is diverted from the ductus arteriosus. Then, as the force of blood pressure restraining closure of the ductus arteriosus in the fetus lessens, the ductus arteriosus is able to close. This may be called a hemodynamic theory of closure of the ductus arteriosus.

In support of the above view is the fact that when a state of asphyxia is transiently induced while the lamb is in the fetal condition, this is accompanied by a generalized constriction of blood vessels and a rise of blood pressure.[3] Cineradiographs show that the ductus arteriosus closes, too, as the power of its contractile force overcomes the pressure of blood within it. Recently, the author has likewise observed this directly in anesthetized fetuses with opened chests. The ductus arteriosus visibly and powerfully constricts as asphyxia develops.

The concept of ductus-arteriosus closure just described is a mechanistic one. It is characterized by simplicity. As such, it stands in contrast to the two previous theories that have been propounded and found wanting. One of these is that the ductus arteriosus closes as the result of nervous reflex action associated with distention of the lungs upon

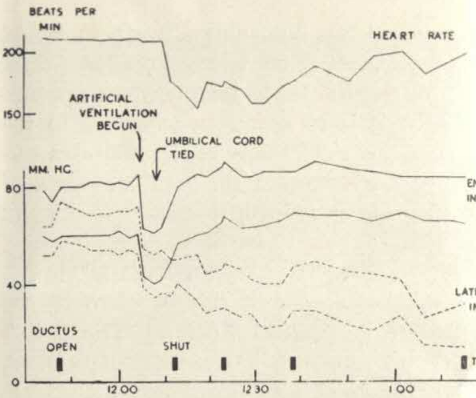


FIG. 6. Effect of breathing on the blood pressure of the fetus. Aortic pressure was recorded from the far end of the aorta, where the pressure reading was higher than a "lateral" pressure reading taken higher up. The effect of ventilation was to cause a transient fall in blood pressure, then a divergence into the condition characterizing the newborn. The ductus arteriosus was found to be closed at the times indicated by the black marks at the bottom, after ventilation of the lung had commenced. (*G. M. Ardran, et al. J. Physiol. London,* 1952.)

inflation. It has been observed, however, that after severing the nerves to the ductus arteriosus, it will close when the lungs are aerated. A nervous reflex mechanism is not essential, therefore. The second theory holds that when air fills the lungs, the oxygen in it acts, when transported by the blood stream, as a stimulus for the muscle of the ductus arteriosus to contract and shut. The fact that the ductus arteriosus closes during asphyxia, when oxygen is low, as described above, belies this. Moreover, physiologists know that oxygen does not act as a stimulant to muscular contraction. The hemodynamic theory recounted above has the virtue of simplicity, and it fits the known facts.

Nothing can as yet be said regarding precisely why, when, or how the foramen ovale closes. An anatomical peculiarity of this structure is that a loose, tough membrane overlies it in the left auricle, and acts as a flap valve. The supposition is that when the blood flow becomes well established through the lungs after aeration and returns at high velocity to the left auricle, it forces the membrane over the foramen ovale to close. In the case of both this structure and the ductus arteriosus, prolonged closure resulting from the kind of physiological conditions described above permits the growth of new tissue to bring about their permanent anatomical obliteration as structures concerned with the passage of blood. Recent measurements of pressures within the heart affirm the above ideas.

When the cineradiographs obtained in the study at the Nuffield Institute were reviewed, upon completion of the experiments in 1951, a new, dynamic

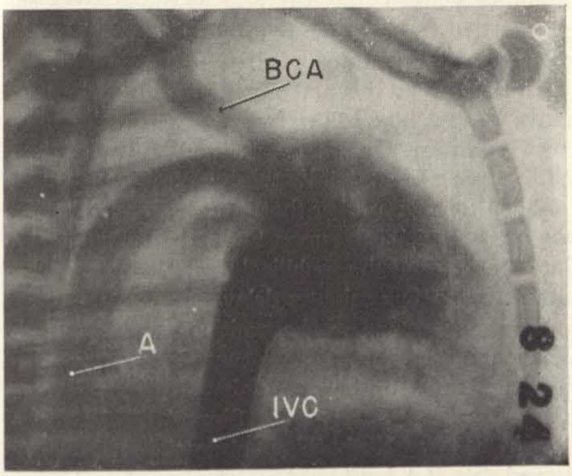
concept of the fetal circulation was developed. This evolved from the fact that it was possible to calculate the speed of travel of the injected opaque medium used in the x-ray procedures between different parts of the circulation, before and after the onset of breathing. Moreover, in the aorta and umbilical arteries, the velocities as well as the diameters of the vessels could be measured. It became possible, therefore, to compute the volume-flow of blood per minute in these vessels. A very general statement of the results of these observations is of interest, for it shows the beauty that is the wisdom of the body.

In the fetal condition, the circulation times for the flow of blood is quite slow in the forepart of the organism. This includes the lungs and head. In contrast, the fastest flows are found in the aorta and throughout the lowermost part of the animal. When pulmonary respiration is established, however, a reversal takes place. There is approximately a threefold speed-up in the rate of blood flow through the head, and a fivefold increase in flow along the pulmonary arteries. The rate of flow in the aorta diminishes appreciably. In short, in both the fetal and newborn conditions nature provides the fastest flow of blood to the organ that in each instance is concerned with exchange of oxygen and carbon dioxide—the placenta and the lungs, respectively.

It was noted above that the flow of blood to the head of a fetus is so arranged that freshly oxygenated blood returns from the placenta to the heart, where it takes the most direct route possible to the head. We now see that at this time the velocity of flow is slow. There is a compensating factor to the slower velocity, therefore, in the better quality of blood going to the head. This mitigates the fact that the primary organ of respiratory exchange lies at the other end of the body, away from the brain. The placenta demands a more rapid and more abundant circulation of blood.

Long and careful study of the cinefluorographs has made possible the determination of a number of other significant physiological facts concerning the fetal circulation. Of these, four may be mentioned. These are all concerned with the volume of blood flowing to the aorta and umbilical arteries of the fetus. Essential for this determination is the fact that the speed of movement of injected opaque contrast medium had to be clocked as it moved from the heart along the aorta and umbilical arteries. The diameter of the vessels was determinable from the pictures. With these data, the volume of blood flowing per minute could be calculated.[4] Let us look first at the rates of flow of blood in the aorta and umbilical arteries.

In fetal lambs weighing from five pounds to more than eleven pounds, the amount of blood flowing down the aorta varied from about 900 to 3000 cubic centimeters per minute. The largest volumes were in the largest animals. It was unexpected that values so large as this would be observed in such relatively small organisms. In an adult young man at rest, the output of the heart



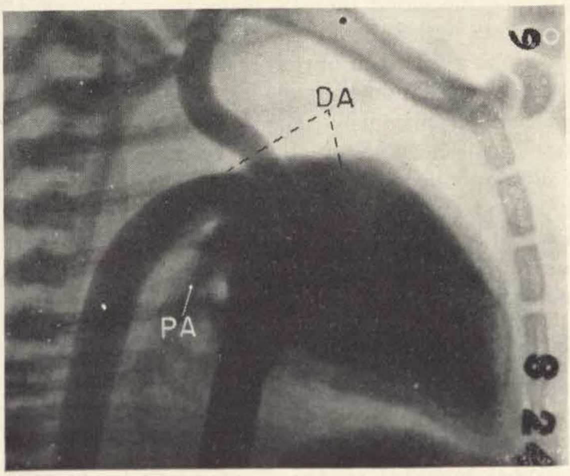


FIG. 7. Pictures showing a contrast medium passing in the blood from the umbilical vein to the heart by way of the large inferior caval vein (IVC). *A*: the prompt appearance of a good shadow in the aorta (A) and in the main branch going to the head (BCA) indicates a considerable flow of blood from the right side of the heart to the left through the foramen ovale, which lies in the septum between the two sides of the heart. *B*: 1 second later, there is a shadow in the pulmonary arteries (PA) and ductus arteriosus (DA). Hence blood flows from the right side of the heart into the pulmonary arterial trunk. Therefore, the stream must have divided in the fetal heart, part going to the right side and part to the left side of the heart.

per minute is less than double the top value, above. Consideration of the facts of the fetal circulation show why such high values are observed. In the fetus, most of the output of both ventricles of the heart goes down the descending aorta. Little blood flows through the lungs of a fetus, and the flow through the head is very slow and rather small in amount. In determinations of cardiac output in human subjects, only that blood which comes from one side of the heart, the right, is measured. Such data are not comparable, therefore, to those obtained in the fetus mentioned above.

How much of the blood flowing down the aorta ends up going to the placenta, instead of to the abdominal organs and other parts of the fetus? Calculations of flows in the umbilical arteries show that the volume of blood in the two arteries varied between 700 to 2000 cubic centimeters per minute. This, expressed as a proportion of the aortic blood flow, is about 63 per cent. The fact that nearly three-fourths of the blood put out by the heart goes down the aorta and approximately two-thirds of this blood goes to the placenta, testifies anew to the tremendous importance of the placenta for the welfare of the fetus.

What determines how much blood will go to the placenta? Is it the length of the umbilical cord, or the sizes of its vessels? Or is it governed by some other conditions? Earlier studies of umbilical cord blood flow[2] have shown that, as long as the diameter of the umbilical blood vessels remains above a critical point, very little energy is lost in the flow of blood from one end of the cord to the other. In fact, the blood literally pours from one end to the other under the impetus of a head of pressure fluctuating with the output of the heart. This suggests that the amount of blood passing to the placenta is primarily related to the volume put out by the heart. Such, in fact, is the case, as the following considerations show. When the volume of blood flowing to the placenta per minute is divided by the weight of the fetus, a fairly constant unit of flow is found. It is between 300 and 400 cubic centimeters of blood per minute per kilogram of body weight. Since the volume output of blood by the heart increases with the size of the organism, it is clear that the placental flow receives a fractional proportion of that output, the amount increasing as the fetus nears term.

What practical lessons, if any, may be learned from the fundamental facts of a physiological nature just described? It would be strange, indeed, if they were not reflected in more effective management of the baby at the time of childbirth. There is one major lesson that has been discovered and already applied. It relates to the inevitable redistribution of blood within the organism taking place when the lungs are first inflated.

At this time, the volume of blood necessary to fill the pulmonary blood vessels increases. This is at the expense of blood throughout the rest of the body, and it is associated with a transient but marked fall in blood pressure. Clearly, if more blood were available for the baby, both of these conditions would be minimized. There is a way to help this situation. This is to obtain blood for the baby from the placenta before the umbilical cord is cut. Such a practice is now a generally employed procedure among obstetricians, although the objective is different from that cited above. After the baby is born, it obtains no iron from its mother's milk, so it must depend upon its own stores in blood and muscle hemoglobin to satisfy its needs for growth until iron is available as a dietary supplement to milk.[5] The present practice of delayed clamping of the cord may add more than 60 cubic centimeters of blood from the placenta to the blood of the body. This is not an inconsiderable amount. In a baby weighing seven and one-half pounds, the quantity of blood in the body is about 300 cubic centimeters. Thus an increment of 60 cubic centimeters is an increase of twenty per cent. The immediate benefit of this volume of blood is to increase the volume of blood, and not the iron alone. This aids in restoring the blood pressure, and so helps to complete the circulatory adaptations to birth.

Among almost all animals, including certain primitive women, the transport of blood from the placenta to the baby prior to birth is aided by the relation of the position of the newborn relative to the placenta in the act of birth. It is below the latter, so that blood flows by gravity into the newly born organism. In many civilized cultures, it is clinical practice to nullify this by employing a birth position for the mother that is more convenient for the attendant. Then, upon birth, the baby is lifted into the air, and the umbilical cord clamped, sometimes after a few minutes delay.

The importance of the position of the baby in relation to the level of the placenta has been studied by Dr. Mary Gunther, of University College Hospital, London, and by Dr. Simon Duckman, of Brooklyn, New York. These obstetricians have observed that if a baby, still attached to the placenta before the latter is delivered, is placed upon a scale, the weight of the baby will vary according to the position of the baby relative to that of the placenta. If the baby is held below the placenta, the baby gains some 30 to 60 grams in weight in

less than a minute. This is enough blood to aid materially in the inevtiable and extensive hemodynamic alterations that take place at the time of birth.

Such conclusions as this concerning the basis of the adaptive mechanisms of the body to birth testify to the fitness of the organism which can be achieved through evolution. Taken in conjunction with those other circulatory mechanisms that cater to the needs of the organism as long as it is a fetus, but which disappear by a series of subtle adjustments when the newborn first inflates its lungs, they make the most complex inventions of the human mind seem small.

References

1. Barclay, A. E., Franklin, K. J., and Prichard, M. M. L. *The Foetal Circulation*. Springfield, Ill.: Thomas. (1945).
2. Reynolds, S. R. M. *Sci. American,* **187,** 70 (1952).
3. Ardran, G. M., *et al. J. Physiol. London,* **118,** 12 (1952).
4. Reynolds, S. R. M., *et al. Bull. Johns Hopkins Hosp.,* **91,** 83 (1952).
5. Windle, W. F. *Asphyxia Neonatorum*. Springfield, Ill.: Thomas. (1950).

Books Reviewed In SCIENCE

August 21

An Appraisal of Anthropology Today. International Symposium on Anthropology of the Wenner-Gren Foundation. Sol Tax, Loren C. Eiseley, Irving Rouse, and Carl F. Voegelin, Eds. Chicago: Univ. of Chicago Press; London: Cambridge Univ. Press, 1953. 395 pp. $6.00.

Reviewed by John W. Bennett.

International Tables for X-ray Crystallography: Symmetry Groups, Vol. I. Norman F. M. Henry and Kathleen Lonsdale, Eds. Birmingham, Eng.: Kynoch Press, 1952. (For the International Union of Crystallography.) 558 pp. Illus. 105s.

Reviewed by J. D. H. Donnay and Gabrielle Donnay.

Proceedings of the International Congress on Analytical Chemistry, Oxford, England, September 4–9, 1952. Under the patronage of the International Union of Pure and Applied Chemistry. Cambridge, Eng.: W. Heffer, 1953. 493 pp. Illus. + plates. 42s.

Reviewed by M. G. Mellon.

August 28

Crime Investigation: Physical Evidence and the Police Laboratory. Paul L. Kirk. New York-London: Interscience, 1953. 784 pp. Illus. $10.00.

Reviewed by William E. Kirwan.

Marine Fouling and Its Prevention. Prepared for the Bureau of Ships, Navy Department, by the Woods Hole Oceanographic Institution. Annapolis, Md.: U. S. Naval Institute, 1952. 388 pp. Illus. $10.00.

Reviewed by Joel W. Hedgpeth.

Wood Chemistry, Vols. 1 and 2. 2nd ed. American Chemical Society Monograph Series. Louis E. Wise and Edwin C. Jahn, Eds. New York: Reinhold, 1952. Vol. 1, 688 pp. Illus.; Vol. 2, 652 pp. Illus. $15.00 the volume.

Reviewed by Roy L. Whistler.

September 4

Introduction to Organic Chemistry. Leland A. Underkofler. New York: Van Nostrand, 1953. 352 pp. $4.25.

Reviewed by G. Bryant Bachman.

A Manual of the Chiggers. The biology, classification, distribution, and importance to man of the larvae of the family Trombiculidae (Acarina). Memoirs of the Entomological Society of Washington, No. 4. G. W. Wharton aided by H. S. Fuller. Washington, D. C.: Entomological Society, c/o U. S. National Museum, 1952. 185 pp. Illus. $6.00.

Reviewed by Charles D. Michener.

September 18

Applied Inorganic Analysis: With Special Reference to the Analysis of Metals, Minerals, and Rocks. (Hillebrand and Lundell.) 2nd ed. Revised by G. E. F. Lundell, H. A. Bright, and J. I. Hoffman. New York: Wiley; London: Chapman & Hall, 1953. 1034 pp. Illus. $15.00.

Reviewed by M. G. Mellon.

Fresh-water Invertebrates of the United States. Robert W. Pennak. New York: Ronald Press, 1953. 769 pp. Illus. $15.00.

Reviewed by Ralph W. Macy.

September 25

Dynamical Oceanography. J. Proudman. London: Methuen; New York: Wiley, 1953. 409 pp. Illus. $8.50.

Reviewed by Henry Stommel.

Fundamentals of Limnology. 2nd ed. Franz Ruttner; trans. from German by D. G. Frey and F. E. J. Fry. Toronto: Univ. Toronto Press, 1953. 242 pp. Illus. $6.50.

Reviewed by Joel W. Hedgpeth.

Cold Spring Harbor Symposia on Quantitative Biology: The Neuron, Vol. XVII. Cold Spring Harbor, L. I., N. Y.: Biological Laboratory, 1952. 323 pp. Illus. $8.00.

Reviewed by E. Gellhorn.

BOOK REVIEWS

AFRICA

Africa: A Study in Tropical Development. L. Dudley Stamp. vii + 568 pp. Illus. + maps. $8.50. Wiley, New York; Chapman & Hall, Ltd., London. 1953.

NO living geographer has seen more of the world than L. H. Dudley Stamp. To his masterful studies on Asia, *Land Use in Britain*, and his many other volumes on different parts of the world, he now adds an analysis of Africa. Since he has traveled widely in that continent, his book has the sure touch of one who has seen and understood. This volume at once takes its place as the definitive study of the continent. The author is interested in man's use of the land, so it is but natural that he stresses agriculture rather than minerals or political geography. At the same time, his geological background gives him a solid physical base. The emphasis is on tropical Africa.

Professor Stamp is uniquely equipped to compare Africa with the other continents. He finds it distinctive in its geography as it lies "athwart the equator," and "projects almost equally into both the northern and southern hemisphere. . . . The bulk of its 11,700,000 square miles lies within the tropics, so that Africa is essentially the world's problem continent where development under tropical conditions is concerned. Africa has other claims to urgent world attention. By Western standards much of the continent is underdeveloped, with resulting poverty and misery to its inhabitants, and with a further consequence that resources of interest to the outside world in minerals, agriculture, and forest products remain unused."

Stamp recognizes four great realms: the Mediterranean, which is part of the Moslem world; western Atlantic Africa, which is tropical Africa par excellence; eastern Africa, and southern or white man's Africa. The first nine chapters unroll the map and sketch the story of climate, soil, people, and transport. Eleven regional chapters follow, with a final survey of African problems and a statistical summary.

Most of the volume is properly filled with detail, but throughout the book the reader will find significant generalizations. Thus, "Morocco, Algeria, and Tunisia, though situated in Africa, form a region so apart from the remainder of the continent that they might almost have been excluded from a book dealing with Africa, the tropical continent." Or, "The development of the Gezira irrigation area is a fascinating story. It is the supreme example in Africa of a successful co-operative scheme between European management and African cultivators settled in an orderly way on the reclaimed land . . . It was in fact the largest peasant agricultural co-operative in the British Commonwealth."

The last chapter deals with the past, present, and future; and there both the general reader and the scholar will find much of interest. Several things have helped to transform Africa so rapidly. The first is the development of transport, a mobility that has disrupted the old tribal positions, made possible rapid economic development, and changed political administration. All this has brought a rise in the standard of living, with changes in food, clothing, and occupations. The export trade has greatly increased, with new products such as iron ore, bauxite, tin, chromium, manganese, and cobalt, in addition to the traditional diamonds, gold, and copper.

Out of a population of 198 million, those of European origin number 5 million. Fifty million live in the four independent countries, those under control of the United Kingdom number 57 million, 42 million are in the French Union, 15 million live under Belgian direction, while 11.5 million are under Portugese rule, and 1.6 million live in Spanish areas. United Nations Trust Territories account for 17.6 million.

This story of Africa is both professional and meaningful. It is the most interestingly written of the author's many books, and will be acclaimed by all who have any concern with the social, economic, and political development of this coming continent. The volume is handsomely illustrated by more than 200 photographs and maps, reproduced in excellent style.

GEORGE B. CRESSEY

Maxwell Professor of Geography
Syracuse University

THE POPULAR QUEST FOR SCIENCE

Explorations in Science. Waldemar Kaempffert. vii + 296 pp. $3.50. Viking Press, New York. 1953.

A healthy future for science depends on the dissemination of the results of research to a widening audience. Some scientists are willing and able to write for the layman, but many lack inclination or talent for communication beyond their esoteric circles. That function is now largely served by the science reporters, whose craft is increasingly important for scientist and layman alike. The reporting of science as spot news has problems that are absent in other fields of news gathering and that have not (from the point of view of the scientist, at least) been completely solved. It is in the longer review article that the best of the science reporters have achieved greatest success, and among the best is surely Waldemar Kaempffert of the *New York Times*.

In his most recent book the author has gathered and revised twenty of his periodical articles. They are not (as the blurb would have us believe) "a complete guided tour . . . of today's increasingly important scientific advances." They are a fascinating personal selection of some topics along the highways and some along the byways of science and technology. They range

from A-bombs to Zworykin (on weather prediction and control). Between A and Z the diet, if not balanced, is certainly varied: atomic power plants, solar engines, cycles, atmospheric stratification, supersonic planes, space flight, astronomy (from Palomar), mechanical substitutes for human organs, the nature of the human body, cancer, psychodrama, science and social change, H. G. Wells as social reformer, and others. The interest for the reader of SCIENTIFIC MONTHLY is less in the contents of the articles (although he, too, will surely learn something) than in the selection and presentation. The appeal of the chosen topics is evident. Standards of coverage and accuracy range from high to, at worst, adequate. The style is readable and attractive, journalistic in the best sense, and only occasionally so staccato as to obscure some of the links in thought.

The author has predilections and limitations consonant with his own aims and therefore meriting notice but not criticism. In the tradition of good science reporting, he does not ignore past failures and present uncertainties but he does stress future supposed benefits. He is more at home with engineering and invention than with science, strictly speaking. As a rule (and this is also evident in the choice of subjects) he has more grasp and insight for the physical sciences and their applications than for most other subjects. Examples, among many, of these (perfectly legitimate) tendencies are his stressing the putative use of synthetic proteins to make fur coats or his inadequate and sometimes erroneous understanding of the history and present status of evolutionary theory. Many professional scientists will question some of his ideas on the scientific mind and on the processes and philosophy of research.

The chapter on "luck" is hardly up to the author's usual standard, and that on ciphers and codes can only be called mediocre, in addition to the fact that it certainly is not an "exploration in science." It is unfortunate, too, that the book should close with a chapter on sunken treasure which is excellent in its field but is decidedly outside the field of the book as a whole.

By the most stringent standards Mr. Kaempffert has a batting average of at least .750, which is good in any league. This book includes fine examples of science reporting, and scientists are fortunate to have such an interpreter.

G. G. SIMPSON

American Museum of Natural History
New York City

THE PURSUIT OF THE STARS

The History of Astronomy. Giorgio Abetti, translated from the Italian by Betty Burr Abetti. 338 pp. Illus. $6.00. Schuman, New York. 1952.

Astronomy for Everyman. Martin Davidson, Ed. 494 pp. Illus. $5.00. Dutton, New York. 1953.

ALTHOUGH astronomy is the oldest of the sciences, it is also one of the most active. This is shown by such recent advances as the development of radio techniques for studying the heavens and the revision of the astronomical measuring scale which gives a new set of values for the distances of the outer galaxies, double those previously accepted.

Doubtless an astronomer can get along without knowing much about the history of his science, but he can view these new developments in better perspective if he has some knowledge of the steps that led to them. This excellent book, *The History of Astronomy,* will give him that background, starting as it does with the ideas of primitive man and ending with the 200-inch Hale telescope at Mt. Palomar.

Dr. Abetti, director of the Astrophysical Observatory of Arcetri, near Florence, Italy, is a leading authority on the Sun and also on the history of astronomy, particularly Galileo, whose collected works and letters he has edited. This is peculiarly appropriate, since Galileo spent the last years of his life at Arcetri, and did his early work at Padua, where Abetti was born.

Fortunately the author's career has not been confined to his native Italy, but has been international. He has worked at Berlin and Heidelberg, as well as at the Yerkes and Mt. Wilson Observatories in the United States. This has helped him to write a book that is remarkably free from nationalistic bias, in which ample credit is given to the work of Americans, Englishmen, Germans, Frenchmen, and others, as well as Italians.

The first part of the book, 63 pages, takes us from the Chaldeans, Greeks, Romans, Hindus, Chinese, and others of ancient times, through the Middle Ages, when the Arabs made the principal contributions. The next 86 pages deal with the reformation of astronomy in the 16th and 17th centuries by Copernicus, Tycho Brahe, Galileo, Kepler, Huygens, Cassini, Newton, and their contemporaries. The remainder of the book, more than half, is devoted to the modern era, coming up to the 20th century concepts for which men such as George Ellery Hale, Sir Arthur Eddington, and others were responsible.

The final chapter is an account of the International Astronomical Union and its important activities, as well as the author's views of the future of astronomy. He foresees continued new discoveries and results, made possible by increased numbers of astronomers with ever more powerful means of investigating the sky, and by ". . . the assistance which the related sciences in the theoretical and experimental fields lend to astronomy in increasing measure."

The illustrations, for which the author is probably not responsible, are grouped together in one 32-page section. While they are well reproduced, they seem to have been chosen with little thought, since many relate to matters not discussed in the text. It seems strange, for example, to see among all the serious subjects a reproduction of an old print of "the inhabitants of the Moon, as seen through the telescope of Sir John Herschel," without any indication that this was a famous hoax, perpetrated by an American journalist in 1835. Certainly there should be a picture of the 200-inch telescope; actually there are three, two of the instrument itself and one of a model. This seems too much of

a good thing. Some pictures taken with it, of which there are none, might well have been substituted for two of these.

Astronomy for Everyman, of British origin, is of quite different character, although one chapter does give a concise history of the subject. In fact the various chapters cover almost every phase of astronomy, including the aurora and zodiacal light, and navigation. There is a short chapter on the possibilities of rocket flight to other planets by that prolific writer on such subjects, Arthur C. Clarke, chairman of the British Interplanetary Society.

Special emphasis is given the nearer objects, perhaps with good reason, since these are most familiar to the lay reader to whom the book is addressed. Certainly the Sun and Moon deserve the chapters they have been given, but it seems somewhat provincial to limit the account of the external galaxies, the largest and most distant objects studied by astronomers, to less than seven pages at the end of the chapter on the stars.

However, the book does give a good summary of many of the most interesting and significant aspects of modern astronomy. The authors, most of whom are officers, or former officers, of the British Astronomical Association, which is mainly an amateur group, know their subjects well and write about them in a manner that is both interesting and authoritative.

JAMES STOKLEY

Schenectady, New York

CULTURAL CHEMISTRY

Man and the Chemical Elements. J. Newton Friend. ix + 354 pp. Illus. + plates. $6.00. Scribner, New York. (Printed in England.) 1953.

"A truly human document." These are the author's own words (p. 261), used in commenting on the Egyptian Papyrus, Anastasia I, but no phrase of mine could more aptly characterize *Man and the Chemical Elements.* It is thoroughly delightful throughout. There are twenty-three chapters, each devoted to a group of chemical elements arranged according to Mendelejeff (or perhaps we should say Newlands out of deference to the pro-British enthusiasm of the author), historical anecdotes collected by a genuine scholar who is careful to distinguish fact from fiction, for instance (p. 21): "The Priestley statue in Birmingham represents Priestley heating the oxide in a tube with the sun's heat concentrated by a lens held between his thumb and second finger. Poetic licence! The actual lens was twelve inches across! Legend hath it that Priestley discovered the gas on 1st August 1774. . . . His first public announcement was at the Royal Society on 23rd March 1775, and in most cases that would now be taken as the date of the discovery." Or (pp. 102 and 144): "Before long a large battery was installed for research purposes in the Royal Institution under the direction of Humphry Davy. It had 2000 pairs of plates—copper and zinc—with a total surface of 890 square feet. With its aid in 1807 Davy was able to isolate for the first time the alkali metals sodium and potassium. . . . Napoleon was extremely angry that the honour of discovering the alkali metals should have fallen to the English . . . (He commanded an electric battery) . . . made at once and when it arrived he called at the Academy to see it. Before any one could stop him he placed the terminals in his mouth to try the strength of the current. The shock on his tongue must have been terrific; he left the Academy without a word!"

Thorough treatment is given diamonds (pp. 55–67), thermometers (pp. 221–227), nails, from the ancient to the modern threaded nail (pp. 285–289), and standards of length and mass, including monochromatic light from Hg-198 (pp. 306–310). One wishes that the account of the H-bomb were up-to-date: tritium is not mentioned, and the reaction $4H \rightarrow He$ which is fully treated is unsuited to terrestrial use.

Thousands of interesting facts catch our attention, for instance: the Monarch of Moscow bell cast in 1735 and weighing 200 tons, but cracked in casting and therefore inarticulate (just as the Russian vacuum cleaners made two centuries later wouldn't vac) . . . the famous Delhi pillar of wrought iron, perhaps unconsciously kept from corroding these 1700 years by the ceremonial butter with which it is annointed at religious festivals . . . the name plutonium, suggested over 100 years ago by Clarke of Cambridge for an element that was later called barium . . . and the meteor that fell in Eastern Siberia in 1947, largest in the memory of man—1000 tons, compared with the famous Rowton meteorite of 1876 that weighed a mere 7 pounds.

Any teacher of general chemistry should forever have a copy of this book within his grasp as an unending source of human-interest stories about the chemical elements—enticing scientific bait for the beginning student who is still alien to science but native to history. Other scientists who read this in their lighter hours will derive continuing pleasure from it.

HUBERT N. ALYEA

Frick Chemical Laboratory
Princeton University

POPULATION PRESSURE

Hungry People and Empty Lands. S. Chandrasekhar. 306 pp. Illus. $3.50. Indian Institute for Population Studies, Baroda, India. 1952.

THIS is a book dealing with the relation of international tensions to the growth of population and to the economic development of the "underdeveloped" areas of the world by an Indian student. His thesis is that international tensions are at least aggravated, if not caused, by differentials in level of living due in large measure to the unequal pressure of numbers of people on the resources available for their maintenance in different political areas. He cites the now well-known facts regarding differentials in population growth and resources in different countries, but he quite naturally

draws heavily on the data relating to Asiatic countries having small resources in comparison with their large and growing populations.

Dr. Chandrasekhar, while believing that there are still considerable unused resources in the world that could be made available to the needy peoples through emigration, argues that the universal control of population growth must be one of the essential elements of any world policy calculated to improve living conditions in the underdeveloped areas and thus to ease the tensions between such nations and the more favored areas of the West. He also urges the need for the complete liquidation of the colonial system, for large scale industrialization, and for the improvement of agriculture as bases for a better level of living, but repeatedly he comes back to the point that without the control of population growth these other parts of a broad program of economic improvement will be of small avail.

The book is written with conviction and vigor and the reviewer finds himself in basic agreement with Dr. Chandrasekhar on all important points, although he has, perhaps, less faith in the substantial alleviation of the hard lot of the Indians, Chinese, Japanese, and other hard pressed people through migration to the less densely settled areas of the earth. He would also be inclined to emphasize somewhat more than the author the need to show these underdeveloped peoples how their poverty has arisen in the past and will continue to harass them, in large part, because of their too great numbers. However, the author has done a good job in surveying the world population situation and in presenting it as it appears to a man in, perhaps, the most important of the underdeveloped areas.

The reviewer regrets that an otherwise excellent book is occasionally marred by a lack of precision in presenting small details of fact that, while not resulting in actual distortion, may give some readers an impression of careless treatment and thus prejudice them unduly against the conclusions of the author. The reviewer greatly hopes that this book will secure wide circulation among thoughtful people not only in the underdeveloped areas of the world but in the West that, willy-nilly, must accept much of the responsibility for the population and economic changes and the tensions resulting therefrom in these less favored areas.

WARREN S. THOMPSON
Scripps Foundation for Research in Population Problems
Miami University
Oxford, Ohio

FOOD LORE

Man's Foods. J. B. Jensen. x + 278 pp. $4.50. Garrard Press, Champaign, Ill. 1953.

AS stated in the subtitle of this book, it is concerned with nutrition and environments in food gathering times and food producing items. Dr. Jensen has developed a fascinating subject, tracing some of the nutritional influences on man's development. The book has been divided into two sections: the food gathering times which begin with the foods of the pre-Chellean-Chellean to the Mesolithic periods of Europe, and the food producing times. This second part includes chapters on cereals, foods of orchards and gardens, animal foods, climatic stress and nutrition, and social aspects of nutrition. This illustrates the broad fields that the author has covered. In fact, what may be one of the chief criticisms of the book is that the author has so much information that the book seems poorly organized. Because of the tendency to skip from subject to subject somewhat rapidly, the book is not easily read. The book is devoid of illustrations which could have brightened it considerably. The author has gathered his information from the anthropologist, the explorer, the chemist, the physiologist, and the nutritionist and, as with any book dealing with so many aspects of food and its contribution to progress, there will be parts with which some may not agree. For example, the nutritionist surely will take exception to the table of nutrient content of wheat in the chapter entitled cereals. The table is not in agreement with the U. S. Department of Agriculture Handbook No. 8. The table is not documented and it is poorly arranged.

Nevertheless, the reader will find the book interesting. The author has a way of inserting interesting quotes in the right places. For example his little quote from a Sumerian milker (5000 years ago) which he says cannot be vouched for, "that if the gods wanted man to have clean milk they would have placed the udders on the forepart of the animal," makes the reader want to read on to find out what more modern people think of the practicality of milk and milk sanitation.

There are about 600 references listed. These include the works of explorers, anthropologists, and historians, as well as chemists and nutritionists. The author has made wide use of data from the many fields that contribute to the very interesting subject of food and man's physical and social growth.

MARTHA F. TRULSTON
Department of Nutrition
School of Public Health
Harvard University

MAPS

Elements of Cartography. A. H. Robinson. vii + 254 pp. Illus. $7.00. Wiley, New York. 1953.

THE past decade may well mark a renaissance in the ancient science of cartography. For centuries cartography has been considered a manipulative subject consisting mainly of map drafting. Dr. Robinson brings us a fresh approach with his stimulating theme that cartography is an "intellectual art and science."

The author, a professionally trained geographer and cartographer with several years wartime experience in governmental mapping activities, is eminently qualified to present this new approach. In addition, he has had experience in teaching cartography and has done out-

standing research in methods of cartographic presentation and the analysis of cartographic technique.

Robinson has competently selected the subject matter of this book in accordance with accepted philosophy of geo-cartographic training. All of the conventional aspects of modern cartography are presented in a balanced and well organized manner. Two chapters (The Employment of Projections, and Map Design) stand out as highlights, not because they are superior in presentation, but because much of the material covered in them is usually lacking in cartography texts.

The chapter on The Employment of Projections, focuses attention on the qualities of projected earth grids, the characteristics of a variety of map projections, and the criterion for selecting the proper projection to suit the map purpose. The excellent illustrations together with well organized textual matter bring the reader a visual as well as a written understanding of map deformation. This presentation of an oft considered cartographic bugaboo should dispel apprehension of the uninitiated.

It is in the chapter on Map Design (VII) that Robinson's theme of cartography as an "intellectual art and science" is most fully developed. Particularly noteworthy are the new concepts in sections on visual outline, visual significance, clarity and legibility, color in cartography, contrast of value, contrast of pattern, choice of colors, balance and layout, titles and legends, and effects of reduction. Again, excellent illustrations lend significance to these new departures. Here as elsewhere throughout the book Robinson has applied the findings of research done in allied fields of design, typography, and visual psychology. If it were necessary to single out the most important contribution of the book it would have to be Chapter VII. Nowhere else is as complete and competent a discussion of map design available in the English language.

Elements of Cartography will be a welcome addition to the libraries of traditional makers of maps, such as geographers, geologists, engineers, and military men. It should also prove to be very useful to authors in other fields who find it necessary to compile and draft maps to accompany their own publications.

George F. Jenks
Department of Geography
University of Kansas

THE AMATEUR WEATHER PROPHET

Weather Inference for Beginners. D. J. Holland. xiii + 196 pp. Illus. $6.00. Cambridge University Press, New York. (Printed in England.) 1953.

PUBLICATION of books like *Weather Inference for Beginners* (made clear in a series of actual examples), indicates the presence of a sizable market in Great Britain for nonfiction bordering on the professional level. Although the author points out in his preface that "this book is for all who are interested in their own daily weather," by page 2 he is discussing *total* versus *local* changes in a fluid's properties. Similarly, various standard meteorological symbols are introduced and used briefly, such as φ, ρ, λ, ∇p, θ, α and Φ, as well as ψ, the stream function. Even a vector cross product appears on page 85. Thus, this book actually provides the novice with a brief but comparatively high-level introduction to practically all the major concepts of modern meteorology, including blocking action, jet streams, thermal winds, potential temperature, extinction coefficient, solenoids, ridge and trough theory, convergence and divergence, Richardson number, virtual temperature, entropy, geopotential, isentropic surfaces, thickness lines, isallobaric winds, and thermodynamic charts, particularly the tephigram.

Contrasting sharply with this quasi-professional approach are long lists of consecutive weather observations throughout the book illustrating weather changes on which the technical explanation is based. Although these observations were made near London, England, between September and December 1936 in a mixture of official and nonofficial codes, the observations do not achieve their purpose in providing a framework for the technical discussion, in the reviewer's opinion. Instead, the listings require the perusal of tedious weather notations long before the reader thoroughly understands the meteorological mechanisms involved. Moreover, the weather inferences based on single and multiple station observations, though intersecting and well described, probably are applicable in North America only in the Pacific Coast region between Oregon and Alaska where the air-mass sources and storm tracks are somewhat comparable to those affecting London, England.

The professional meteorologist is likely to find the section on the balance of atmospheric radiation helpful in preparing classroom lectures. The section on wind scales contains a new universal scale which the author considers one of the easiest to use when deducing gradient wind from a single diagram. In conclusion, it is noted that in contrast to many of the postwar books printed in Great Britain, this one is printed on semigloss paper, in large type, and with wide margins that combine to provide a pleasing typographic product.

Charles C. Bates
7810 Gateway Boulevard, S.E.
Washington, D.C.

ARTIFICIAL RESPIRATION

Adventures in Artificial Respiration. Peter V. Karpovich. xvi + 303 pp. Illus. $7.50. Association Press, New York. 1953.

THIS book presents an extensive historical account of the development of methods for artificial respiration, well and fully illustrated with photographs of many of the leaders in the field. The reader is surprised to learn of the large number of techniques introduced over the last century. The changes in practice over the years (from Silvester to Schafer, and from Schafer to Nielsen, as the most widely accepted authorities) are described in great detail, with some account of many other methods. A table is included, 18 pages long, in

which 105 different procedures are briefly described, including both single and double phase methods. The text is written in rather colloquial style, with many witticisms, some rather forced, but the total impression is stimulating and informative.

There is a good account of recent studies of artificial respiration in normal people, ending with Gordon's observations on 26 heroic anesthetized and curarized subjects, totally unable to breathe on their own, in which the superiority of the Nielsen arm-lift back-pressure method over the older Schafer prone-pressure technique was conclusively demonstrated. The evidence suggests that the Schafer method is inadequate, not only because it moves too small a volume of air, but because this volume is not properly distributed in the lungs.

The normal physiology of respiration is discussed in several chapters, at a rather elementary level. The treatment of the Hering-Breuer reflexes leaves much to be desired. It omits recent work that shows that sensory nerve impulses from lung receptors are almost certainly not responsible for the onset of a new inspiration, however important they may be in bringing it to an end. Sufficient attention is not given to the recent revival of the concept of intrinsic rhythmicity of the respiratory centers.

The author has made many contributions to the subject, and has been closely associated with the research sponsored by the Armed Forces, under the direction of Dr. D. B. Dill. In this program he has carried through metabolic studies that have shown that, in obtaining the more favorable results of two-phase methods, the operators exercise much more vigorously than in the older techniques, thus greatly increasing their oxygen consumption. At the same time their subjects show no change in metabolic rate, if the methods be properly used. Injuries and distress, in both operators and subjects, are discussed, and suggestions are made for minimizing such incidents. The book closes with a summary of the most important material, intended as a guide for instructors in life-saving.

WILLIAM R. AMBERSON
Department of Physiology
University of Maryland Medical School
Baltimore, Maryland

THE WORLD AROUND US

Our Physical Environment: A Problem Approach. Leonard W. Gaddum and Harold L. Knowles. ix + 625 pp. Illus. $5.50. Houghton Mifflin, Boston. 1953.

THE authors of this textbook for an integrated course in the physical sciences voice two principal objectives: a comprehensive picture of man's physical environment, and the encouragement of reasoning ability. In choosing material, those factors that have been most important in influencing civilization are given first consideration. An excellent introduction on environmental factors and the nature and applicability of scientific method is followed by sections on space and time, weather and climate, native vegetation, topography, and material resources and energy resources. The fundamentals of physics and chemistry appear in the last two sections of the book—an inversion of the customary approach. Each of the sections is prefaced by a discussion of the relative significance of the environmental factor that is to be developed. Included at the end of the book is a bibliography of seventy-three suggested readings on basic knowledge, scientific method, and social and environmental problems.

A distinctive feature of the book is the extensive problem lists at the end of the chapters. These provide great variety, and include drill exercises, problem situations requiring application of principles and methods discussed in the text, and others based upon excerpts from literature and the diaries of travelers and explorers. As the book progresses, review questions are prominently featured in the lists. It is clear that the authors have given much time and effort to these lists, and if as much time is devoted to them in the classroom and study as they deserve, the authors are well justified in calling the work a "problem approach."

The book is attractively printed and illustrated. Important ideas and principles are set off in black-face type. The style is conventional. Difficulties are not sidestepped and important concepts (such as pressure and humidity in the section on weather and climate) are adequately developed as they are needed, a feature that contributes to the integration. This book should be a valuable aid in reaching the objectives set by the authors. It is an excellent addition to the list of textbooks on the physical sciences.

W. PAUL GILBERT
Department of Physics
Lawrence College
Appleton, Wisconsin

Man and His Physical Universe. Richard Wistar. xv + 488 pp. Illus. $4.75. Wiley, New York. 1953.

THIS is a provocative book that might be misjudged if cursorily examined. Written as an integrated approach to the study of physical science, materials are drawn from the fields of physics, astronomy, meteorology, geology, and chemistry. In the choice of material, the assumption is made that student interest and retention is greater when attention is focussed on phenomena that are an intimate part of the student's experience. For example, photography serves to introduce the principles of optics and wave motion, and this leads naturally into sound and the physical basis of music. Weather lore points up the need for a scientific basis of forecasting. Pressure and its measurement, gas laws and kinetic theory, work and heat, are then studied as a rational basis for the analysis of weather and climate. Communication by telegraph, telephone, and radio, and the electrical system of the motor car, lead to the exposition of the major principles of electricity. An account, in historical sequence, of the major developments in atomic structure and energy ends the book. Succinct chapter summaries are provided. The study questions

are thought provoking and many of them require numerical computation. Scientific method is stressed and is correlated with the text; the limitations of the method are not considered.

The style is simple and direct and might be observed with profit by other writers of physical science textbooks. The terse, crisp sentences make easy reading. To one familiar with the subject matter, the author's sentences are often striking because of the range of implication. To the unitiated, the simple direct statements may lead to inaccurate impressions and a false simplicity if they are not accompanied by qualifying comment. It is a little surprising, for example, to learn that Bach "suggested" the fundamental basis for equal temperament, or to find that "the period between 1700 B.C. and A.D. 1700 was nearly barren of significant industrial and scientific achievements." Topics are frequently introduced by a barrage of suggestive questions. Mathematical formulae seldom appear and in those instances where they are needed, the relations are written out in words rather than in symbols. The author has done much toward making physical science interesting and exciting, and the book should appeal to the layman as well as to the student of physical science.

W. PAUL GILBERT

Department of Physics
Lawrence College
Appleton, Wisconsin

DRUG ADDICTION

Conferences on Drug Addiction Among Adolescents. xvi + 320 pp. $4.00. Blakiston, New York. 1953.

THIS is the verbatim report of two conferences on drug addiction among adolescents held at the New York Academy of Medicine in November, 1951 and in March, 1952. The meetings were sponsored by the Committee on Public Health Relations of the Academy and subsidized by the Josiah Macy, Jr. Foundation. Fifty professional workers interested primarily in either the social problems of adolescence or in drug addiction participated—most of them from New York. There are a few short reports, or perhaps one might better term them memoranda, to focus the discussions. The bulk of the book consists of spontaneous verbal interchanges between participants. There is in consequence a good deal of repetition. The volume has a three page glossary of terms from drug-addiction argot, and a very extensive index.

Dr. Frank Fremont Smith, the Medical Director of the Macy Foundation, keynoted the proceedings when he said, "The most important thing that this preliminary conference can possibly do is to light up the areas of our ignorance." In justifying such meetings he makes use of Professor Cannon's very apt phrase, "the fertility of aggregation." Confessions from many of the participants of our great areas of ignorance of the myriad of problems connected with drug addiction punctuate the report.

There seemed to be general agreement that there existed in 1951 in New York City, and elsewhere in this country, a marked increase in the use of narcotics. In 1951, six hundred and sixty-two drug users between sixteen and twenty years of age were arrested in New York City—a great increase over the usual number. According to Dr. Haven Emerson, New York had a similar epidemic of drug addiction in 1911 that was not brought under control for five years.

Dr. Harris Isbell, the Director of Research at the United States Public Health Service Hospital for drug addicts in Lexington, Kentucky, has a short but comprehensive outline of the many subjects for psychiatric research connected with addictions. Professor Stanley Bigman presents the sociologist's point of view, emphasizing the importance of group pressures. Professor Maurice Seevers briefly reviews the animal experimental work and mentions his own experiences with his sixty addicted monkeys.

Here and there one can find points of clinical interest, such as the rarity of withdrawal symptoms in juvenile addicts, the reduction of the intensity of withdrawal symptoms by hypnosis, and the fact that heroin capsules commonly peddled generally consist of ninety-eight per cent sugar.

But, for those seeking specific information the report will prove disappointing. For those who are interested in learning the direction that the thinking of a group of competent and dedicated professional workers is taking, in dealing with this important social problem, it will prove rewarding.

MANFRED S. GUTTMACHER

Court House
Baltimore, Maryland

SPADE WORK

Spadework in Archaeology. Sir Leonard Woolley. 124 pp. Illus. + plates. $4.75. Philosophical Library, New York. (Printed in Great Britain.) 1953.

IN this brief and easily read book a man who can be called the greatest living archaeologist gives us the benefit of his wisdom, acquired in fifty years of digging. The book is dedicated to Mohammed ibn Sheikh Ibrahim, now a member of the Syrian Parliament and head of an Arabian tribe. Hamoudi, as the sheikh is less formally called, served as Sir Leonard's foreman until after World War II, when he was succeeded at the excavations by his son Yahya. The book contains a photograph of a bust of Hamoudi by Lady Woolley and several anecdotes about his skill at handling workmen, protecting finds, and directing the most delicate kinds of excavation.

Sir Leonard's appreciation of Hamoudi is the key to his success in the field. Honesty and sincerity combined with mental alertness and native ingenuity are qualities that anyone needs to succeed in any kind of enterprise in the Middle East. The Arabs are keen students of human nature, treating each man according to his merits, and Sir Leonard did well in the test. He and Hamoudi,

if one may judge by the wealth of amusing anecdotes with which this book is studded, are two of a kind.

His first dig was at a Roman site in the north of England in 1907. He then went to Nubia for five years; during this time he unearthed a vast number of curious specimens of a mixed African culture which have lately been the downfall of our experts on the University Museum (Philadelphia) television program. In 1922 he dug at Akhenaten's capital, Tell el Amarna. Then came work in Italy, and in 1911 he opened a huge site, the Hittite city of Carchemish in Syria, with T. E. Lawrence as his assistant. After time out for World War I, he returned there in 1919. His greatest work was at Ur, where he spent twelve years in the desert, and unearthed the magnificent specimens now on exhibition in Philadelphia, London, and Baghdad. It is characteristic of him that he waited five years before opening the royal tombs, in order to train himself and his men for this delicate and intricate task. For seven years before and after World War II he dug at another city-site near Hatay, on the Turko-Syrian border. In all of these digs he and Lady Woolley lived in the close quarters of expedition houses, with expedition personnel. No mention is made of the strain on the human nervous system this manner of life imposes.

This is more than a manual of how to dig, to handle workmen, and to deal with government officials. It is more than a collection of amusing anecdotes. It is first hand history and not the usual product of compilers. Sir Leonard, for example, thinks less of the godly qualities of the Pharaoh Akhenaten than do the standard historians, for it was Sir Leonard and not they who excavated that monotheistic king's wine cellar and harem, even finding the body of one of the ladies amid the jars. The city itself, he remarks, was shoddy, made of bright paint over cheap stone. No other subject could be represented by the royal sculptors except the monarch and his family. "At his death," says Sir Leonard, "his gaudy capital was deserted like a theater stage after the last curtain falls."

Even in his most formal reports Sir Leonard's writing is never tedious. In *Spadework* it is delightful.

CARLETON S. COON

University Museum
University of Pennsylvania

SOCIAL INSECTS

The Social Insects. O. W. Richards. 219 pp. Illus. + plates. $4.75. Philosophical Library, New York; Macdonald, London. 1953.

THIS book provides an excellent, readable account of the behavior of the truly social insects: that is, the termites, the ants, some bees, and some wasps. It is illustrated with several diagrammatic drawings and and numerous photographs.

The emphasis is on British (or at least European) types of insects, so that, for example, the stingless bees which abound only in the tropics are treated in but six pages of text. On the other hand, in many of the discussions, information on non-European forms is included, especially if such material contributes to an understanding of other social insects.

If one were to select a particular section for special comment, it would be the section on the wasps. This is no surprise since Dr. Richards is a specialist on these insects. His treatment of wasp behavior is the best that has been assembled in the relatively brief, easily read style of a semipopular book. Here, fortunately, the author has not hesitated to draw on his personal knowledge of tropical as well as temperate wasp biology, and on the recent studies of wasps made by French and Italian authors.

The rather meager treatment of subsocial forms whose behavior may provide clues to the origin of social behavior may be a disappointment to some. However, the last chapter, entitled "Insect Societies," makes up for some of the lack in this connection by discussing the principles involved in social behavior, prerequisites for social life, the origin of societies, and the like. On reading this book one is impressed not so much by items which might have been included, but by the great amount of material that Dr. Richards has been able to include in a short and thoroughly interesting book. We can recommend it not only to entomologists, but to the general reader as well.

CHARLES D. MICHENER

Department of Entomology
University of Kansas

LETTERS

MODERN CULT OF RACISM AND THE BLACK MADONNA

THERE is an ironic sidelight on The Black Madonna which is the subject of an article by Moss and Cappannari [THE SCIENTIFIC MONTHLY, **76**, 319 (1953)] (not "*so-called* Black Madonna," as she is so often described, but truly Negroid black and with all of the other accouterments to go with her blackness). We learn from the authors that she is the *miracle-working* madonna ("We wish to emphasize that all the black madonnas mentioned above are 'miracle-working madonnas' ").

According to the article by Moss and Cappannari, these madonnas were descendants of one of the Demeters; not "the sorrowful Eleusinian mother" but the more *powerful* Demeter Melaina, the *black* Demeter associated with the earth and fertility. The Greeks built temples to her and the direct line from Demeter (or the Egyptian black Isis or the Roman black Ceres) and the Jesus and Resurrection story has long been noted. These black and Negroid figures get not "*dulia* and *hyperdulia*" (veneration and adoration) but "*latria*" (worship). ". . . The Madonna is worshipped for its power," observe the authors. In fact, it is accorded the greatest and most basic power, that of life itself. Even though religious sources deny the relationship between the black madonnas and the pagan Roman Ceres, they have at least one thing in common. After centuries, eons, of the cult of white worship, after waves of conscious effort to deny, obliterate, deface, paint, and destroy, the black madonnas remain, still believed to be more powerful for the help of suppliants.

The authors conclude that the black madonnas (Christian borrowings from an earlier art form) are equated with power ("They are implored for intercession").

The relationship between power and fear is undeniable. A fear that antedates recorded history is still upon us. In its modern form, it is the cult of racism. For the basic component of prejudices against darker human beings is fear. While it is true that "changes in the culture of any society are always influenced by the pre-existing customs and institutions of that society," also true is the observation that, for the most part, mankind is certainly not easily detached from the patterned customs and beliefs, which, in this instance, appear to have attached themselves to humanity—or at least part of it—right down to the present day.

MARGUERITE CARTWRIGHT
Hunter College of the City of New York

WE find Miss Cartwright's observations interesting. However, we make specific mention in our article that the madonnas under examination are *not* Negroid. Rather, the Negroid madonnas can be explained by an alternative hypothesis, as an attempt to anthropomorphize the Virgin. The extension of our remarks in the statement by Miss Cartwright is unwarranted by the data presented in our article. A careful examination of the data reveals that the evidence does not lend itself to the conclusions made by Miss Cartwright.

LEONARD W. MOSS
STEPHEN C. CAPPANNARI
Department of Sociology and Anthropology
Wayne University

THEOLOGICAL OR CULTURAL BLACKNESS

MAY I, after reading Moss and Cappannari's article, "The Black Madonna: An Example of Culture Borrowing" [THE SCIENTIFIC MONTHLY, **76**, 319 (1953)], express a perhaps critical opinion of it?

I found the article extremely suggestive, and I have no doubt that the authors have a workable thesis in their contention that some of the Black Virgins in Europe, more especially in the Mediterranean area, represent survivals of the cult of a black-faced goddess. However, after reading their discussion of said hypothesis, one is still left wishing that they had come closer to the point where a clean bill of proof could be granted them.

To come down to particulars, one cannot help wondering on what basic criteria their initial classification is made to rest. In tackling a problem of this kind, if I may suggest, it might have been advisable to proceed first to as complete a survey as possible of the cases of the Black Virgin in the area under study. This done, the material on hand could have been divided into those cases where blackness was clearly due to physical agencies, and the others where cultural or religious influences were to be suspected. Then, sharpening the focus, there might have been a discussion of blackness in earth-goddesses in the same area. After which—*hoc opus, hic labor est*—the crucial part of the problem could have been entered, that of confirming "adhesions" (E. B. Tylor's terminology) between representations of certain black pagan goddesses and those of the Black Madonnas today. For the Madonna della Spiga mentioned in the article, this seems to obtain most convincingly; not so, I am afraid, in several of the other cases.

Another insufficiently analyzed criterion is the power of working miracles as an attribute of the Black Madonna. Theologically, of course, the Madonna is everywhere, black or otherwise, capable of working miracles. Historically, however, certain of her representations have acquired a reputation for "working miracles," becoming the *raison d'être* of various popular shrines. This distinction is not to be dismissed as purely verbal since the authors' thesis rests on the possible identifica-

tion of some extant Black Virgins with shrines of the Earth-Goddess where the image itself was supposedly endowed with miracle-working powers.

The article, I am constrained to observe, labors almost throughout under an abundance of extremely luxuriant, but apparently not always relevant, information. The authors' thesis would certainly have come off to greater advantage if it had been disencumbered of such an overgrowth of sometimes rather elementary material. Too, it seems to me that evidence derived from literary sources such as St. Augustine or Guillaume de Digulleville could have formed the object of a separate section.

From the point of view of material presentation, the article is somewhat defaced, so to speak, by a number of "unidiomacies," but in most cases the meaning can be made out. One may query, however, the use of the term "cultural borrowing" which, I take it, is given here its common ethnological sense, where perhaps "survival" might have done better.

Coming down to typography, it may not unkindly be pointed out that "Nuestra Senora di Guadaloupe" (page 319) is distressing to the reader on three counts: first, the *n* of Senora should have a tilde; second, *di* is Italian; the preposition here should be *de*; and third, *Guadaloupe* is French, not Spanish.

I rather blush at the pedantry with which these remarks may appear to be redolent, but first and last, my complaint is that, possessed of a rather interesting hypothesis, Moss and Cappannari should seem satisfied, even at the end of their article, still to call it an hypothesis. One might be disappointed at less.

PAUL-LOUIS FAYE
Department of Modern Languages and Literatures
University of Colorado

WE should like to thank Dr. Faye for his helpful comment on our paper and his suggestions for the improvement in our style. There are, however, several thoughtful points in Dr. Faye's letter that demand answers.

The intent of the article was that of suggesting an hypothesis and, as such, our exposition, it goes without saying, would rest on partial, fragmentary, and incomplete evidence. Anything beyond this we feel with Hamlet would be "a consummation devoutly to be wished."

The writers are well aware of the theological view that God granted the Virgin special powers to perform miracles. However, our interpretation is not an orthodox theological one, but rather an anthropological or functional conception. We have simply suggested that the black madonnas are a special type of phenomena in that they have been regarded *as if* this supernatural power were intrinsic to the images. We are not here at all concerned with the grave theological question of the miraculous powers of the Virgin per se. Instead, we have simply suggested an *hypothesis* that might illuminate one *class* of black madonnas.

Dr. Faye's concern over our use of the term "cultural borrowing" seems unwarranted. We employed it as synonymous with "diffusion." Current anthropological usage calls for the employment of the term "retention," rather than "survival," which smacks too denotatively of Tylor's construct.

Since the writers had not expected to advance their thesis beyond the stage of developing a verifiable hypothesis, we warmly appreciate the constructive criticism advanced by Dr. Faye.

LEONARD W. MOSS
STEPHEN C. CAPPANNARI

SONG OF SONGS

IN a footnote, page 324, to their article "The Black Madonna: An Example of Culture Borrowing" [THE SCIENTIFIC MONTHLY, **76**, 319 (1953)], Moss and Cappannari failed to identify correctly the passage from the *Song of Songs* which they quote as an introduction to the subject matter. They hold it to be a "conversation between King Solomon and his Black Queen." They add ambiguously: "The intent of the original Hebrew version clearly sets forth the relationship between the two lovers." However, for every serious student of the *Song of Songs,* these two lovers are not the royal couple but a shepherdess, tanned by the sun of her native Palestine, and her bridegroom, also of lowly origin. By rabbis as well as by Church Fathers, the *Song of Songs* has been allegorically reinterpreted in order to justify its inclusion in the Holy Scriptures; but never has it been misunderstood as a dialogue between Solomon and a black spouse of his.

Is it really necessary to tell scientists that there is no other "version" of the *Song of Songs* but the "original Hebrew" one? A translation, whether the King James Bible or any other, never becomes another "version."

May I close with the appeal to treat references to the Bible in the future with the same seriousness and thoroughness as any other source material.

L. SELIGSBERGER
1603 Harrison Avenue
Wlimington 3, Delaware

SINCE Mr. Seligsberger has leveled the charges of ambiguity and misinterpretation at the footnote passage in our paper, we feel it necessary to elaborate on an area which is merely tangential to our thesis.

An examination of the literature will reveal a plethora of interpretations regarding the *Song of Songs.* There have been many theological controversies waged on the passages of this Book. Both Jewish rabbinical synods and Roman Catholic scholars have attached a mystical interpretation, treating the Song as an allegory of God and His people (or Church). It is on the basis of this mystical view that the Song has been included in the Jewish Passover liturgy and in the various versions of the Holy Scriptures. However, this is but one approach to the interpretation of the Song.

In the third century A.D., one of the Church Fathers, Origen, described the Song as an epithalamium sung at the marriage of Solomon with the daughter of an Egyptian Pharaoh. Ample evidence has been amassed to

warrant the discard of this hypothesis. More recent interpretations hold that the story is a collection of rustic folksongs used in connection with wedding festivities in the North of Palestine and Lebanon.

The authorship of the Song is commonly credited to Solomon. However, serious objection to this claim has been raised by many students of the Bible. The style and language of the passages are those of an age much later than the time of Solomon. Evidence of Greek culture diffusion is present in the Hebrew text. It is entirely possible that the Song was written some four hundred years after the time of Solomon. Be that as it may, the name of Solomon was attached to the work.

Hebrew scholars describe the story as one concerning the trials of a Shulammite maiden who is in love with a shepherd. Her brothers, disapproving the union, put her to work tending the vineyards where she is blackened by the sun. Courtiers of Solomon chanced to pass the vineyards and were impressed by the rare beauty of the Shulammite. They attempted to persuade her to accompany them to Solomon's Court. After her refusal, they led her away as a captive. Solomon tried to win her love and sang of her beauty. But the maiden remained true to her shepherd lover.* This then is the explanation of our interpretation in the footnote.

Mr. Seligsberger is, no doubt, unaware of the specific use of the word "version." Webster's Dictionary indicates ". . . a translation or rendering of the Bible or a part of it; as, the Authorized and Douay *versions*."

LEONARD W. MOSS
STEPHEN C. CAPPANNARI

* An interesting commentary and translation of the Song by Rabbi Dr. S. M. Lehrman may be found in: *The Five Megilloth*. A. Cohen, Ed. x – xiii, 1–32. Hindhead, Surrey: Soncino Press. (1946). *Megilloth*: Hebrew, meaning *scrolls*.

CURRICULA AND EDUCABILITY

PROFESSOR CAIRNS portrays a problem of the utmost seriousness in his article entitled "Mathematics and the Educational Octopus" [THE SCIENTIFIC MONTHLY, 76, 231 (1953)]. It is inevitable that, as the base of education is broadened to include a larger and larger proportion of a population, a corresponding dilution of content must occur. No one can deny that every citizen must have the right to be educated, but it is equally true that persons and segments of the population vary in their educability.

The objectives to be attained by the "life adjustment" movement are in themselves fine and worthy, but surely should not represent the whole of the goals of education. In themselves they form only the base of good citizenship common to all our people. However, if they are construed to be the end point as well as the common base, then serious damage results. This is extremely well pointed out in Cairns' article in the words—". . . the false argument that the studies in question are good only for the college-bound minority. Hence, by a gross misinterpretation of democracy, these subjects are crowded out, to the detriment of everyone. . . ." This is exactly the misinterpretation always associated with the Lincolnian "All men are created equal." This, of course, means equality in opportunity and not in size, strength, or mental ability. Must we proceed to use the same false arguments to govern the future pattern of our educational processes?

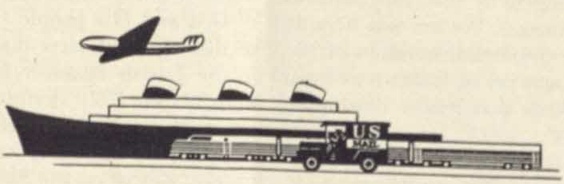
The "life adjusters" exhibit themselves as gallant champions in a great controversy between liberalism and traditionalism. The application of cold common sense shows the problem really to be one resulting from our increasing social and technological complexity. What is needed is to strip off certain deadwood details of accumulated results of man's inquiries of the past in order to prepare for a still more complex future. I would call this an endeavor to bring about modernization of traditional content while securing and maintaining rigorous standards. This is true within any given discipline and especially mathematics. This attitude is distinctly that of Professor Cairns.

Why has the Mathematical Association of America appointed a committee to appraise the present undergraduate mathematical curricula and to make recommendations concerning their betterment? Obviously, to meet this very need. This slackening of standards and this attrition of content, begun by persons trained in college themselves, must now be corrected by a movement beginning in the colleges.

In order to insure that our total educational system of the future is a healthy one, we must modernize content, develop and maintain high standards of performance on the part of the learner (and obviously the teacher), and offer to everyone, based upon his ability to learn and to profit, access to all areas of man's knowledge by appropriate curriculum offerings at the various levels of our educational segments.

PAUL H. KRATZ

Washington, D. C.



ASSOCIATION AFFAIRS

SANTA BARBARA MEETING OF THE PACIFIC DIVISION

THE thirty-fourth annual meeting of the Pacific Division, AAAS, was held in Santa Barbara, California, June 15–20, 1953, under the sponsorship of the California Conservation Council, Montecito School for Girls, Santa Barbara Botanic Garden, Santa Barbara Chamber of Commerce and Convention Bureau, Santa Barbara Museum of Natural History, Santa Barbara College of the University of California, and Westmont College. A well organized committee made up of representatives of these institutions functioned effectively under the chairmanship of John E. Cushing.

Registration headquarters were in the auditorium of the Civic Recreation Center, which was large enough to afford space for exhibits of scientific books and instruments, as well as special exhibits provided by the Botanic Garden, Conservation Council, Natural History Museum, and others. Immediately adjacent, Baylor Lounge of the Recreation Center was available for informal meeting and conversation.

Scientific sessions were held at the Recreation Center, in near-by hotels and theatres, in the Carillo Auditorium, at the Museum of Natural History, and at the Art Museum.

General sessions included an afternoon tour and social hour at the Santa Barbara Botanic Garden on the opening day of the meeting, a general reception the following afternoon sponsored by the California Conservation Council and cooperating organizations, and two evening addresses. The first evening lecture was the presidential address of C. D. Shane, Director of the Lick Observatory and President of the Pacific Division, AAAS, who spoke on "The Distribution of Galaxies in Space." The following evening W. F. Libby of the Institute for Nuclear Physics, University of Chicago, spoke on "Radiocarbon Dating." Each of these addresses drew a large and attentive audience.

Meetings of the Pacific Division differ from national meetings of the AAAS in that no sectional meetings are held as such; the scientific sessions are organized entirely by the affiliated and associated societies. Twenty organizations participated in the Santa Barbara meeting, as follows: American Meteorological Society (123rd National Meeting), American Nature Study Society (Western Division), American Phytopathological Society (Pacific Division), American Society for Horticultural Science (Western Section), American Society of Ichthyologists and Herpetologists (Western Division), American Society of Limnology and Oceanography (Pacific Section), American Society of Plant Physiologists (Western Section), Association of Pacific Coast Geographers, Astronomical Society of the Pacific, Botanical Society of America (Pacific Section), Cooper Ornithological Society, Ecological Society of America (Western Section), Herpetologists League, Nature Conservancy, Pacific Science Board, Society of American Bacteriologists (Southern California Branch), Society for Experimental Biology and Medicine (Southern California Section), Society for Systematic Zoology (Pacific Section), Western Society of Naturalists, and Western Society of Soil Science. Some 400 papers appeared on the programs of these societies.

At the meeting of the Council on Wednesday afternoon, Edwin R. Guthrie of the University of Washington was elected President-Elect of the Pacific Division. The President of the Division for the coming year is A. H. Sturtevant of the California Institute of Technology. C. D. Shane, the Retiring President, becomes Chairman of the Executive Committee.

Ralph Emerson of the Universiy of California at Berkeley, and Moyer D. Thomas of the American Smelting and Refining Company, Salt Lake City, were elected members of the Executive Committee. Richard Van Cleve of the University of Washington and Jerzy Neyman of the University of California at Berkeley were elected to membership-at-large on the Council.

Action was taken to amend the constitution of the Pacific Division to include "that portion of Montana west of the Continental Divide" in the territory of the Division. This action was taken on the basis of a poll of the Montana membership by the Administrative Secretary of the AAAS, and was approved by the national Board of Directors.

Action was also taken to invite the National Science Teachers Association and the American Association of Physics Teachers to become affiliated societies of the Pacific Division.

The next meeting of the Pacific Division will be held at the State College of Washington in June, 1954, the exact dates to be decided by the Executive Committee at its October meeting. It was decided in response to invitations pending to hold subsequent meetings as follows: June, 1955, California Institute of Technology, Pasadena; June, 1956, University of California at Davis; and June, 1957, University of Washington, Seattle.

Raymond L. Taylor and John A. Behnke of the Administrative Staff of the Washington office spoke briefly of the forthcoming national meeting of the AAAS in Berkeley in December, 1954, and related matters. While desiring the widest possible participation in the Berkeley meeting, they urged that societies associated with the Pacific Division hold their June meetings as usual. Such procedure is encouraged by the fact that meetings of the Pacific Division scheduled for June, 1954 and June, 1955 are at considerable distances from the San Francisco Bay Area, so that a national meeting on the Pacific Coast should not affect the usual regional activities.

TABLE 1

GEOGRAPHICAL DISTRIBUTION OF REGISTRANTS*

Arizona	28	New Jersey	1
California	**766**	New Mexico	5
Colorado	4	New York	7
District of Columbia	5	**Nevada**	**6**
Florida	1	Ohio	4
Idaho	**4**	**Oregon**	**50**
Illinois	5	Texas	3
Maryland	3	**Utah**	**32**
Massachusetts	5	Virginia	1
Minnesota	1	**Washington**	**48**
Missouri	3	Wisconsin	1
Montana	**2**	Wyoming	2
Total, continental United States			987
British Columbia	**19**	Japan	2
Hawaii	**9**	Norway	1
India	2	Pakistan	1
Italy	1		
Total, foreign or overseas			35
Grand Total			1022

* Boldface type indicates the seven states, the territory of Hawaii, and the Canadian province of British Columbia that comprise the Pacific Division of the AAAS. Their combined registration was 936 or 92% of the total.

In addition to the luncheons, dinners, and breakfasts held by various societies, there was a well-attended Association Dinner, and a highly successful outdoor barbecue held on the new campus of Santa Barbara College of the University of California.

The total registered attendance at the meeting was 1022 drawn from a wide geographical area, as shown in Table 1. Although predominantly a divisional meeting, all sections of the United States were represented, and there was a gratifying attendance from British Columbia and Hawaii. There were registrants, principally exchange professors and students in American universities, from five foreign countries.

Despite final examinations and the beginning of summer schools, which prevented some teachers from attending, nearly every center of learning in the region was represented. There were registrants from 122 communities in California: *e.g.*, Los Angeles, 144; Berkeley, 80; Santa Barbara, 73; Pasadena, 45; Riverside, 44; Davis, 43; Palo Alto-Stanford, 22; San Francisco, 22; San Diego, 18; Santa Monica, 15; and La Jolla and Sacramento, 14 each.

ROBERT C. MILLER

Pacific Division Secretary, AAAS

MONTANA AND WYOMING JOIN THE WESTERN DIVISIONS

As a result of requests from members in the states of Montana and Wyoming, the question of the incorporation of these states in the Southwestern and Pacific Divisions of the Association was given careful study by the administrative office. A poll of the members was taken to determine their preferences. Of the 76 replies received, 69 favored affiliation with one of the Divisions. Wyoming voted 24 to 1 for the Southwestern Division. The Montana vote was a tie with a majority in the eastern part of the state favoring the Southwestern Division and a majority of those in the west expressing a preference for the Pacific Division.

The Executive Committee of the AAAS at its meeting December 26–29, 1952, authorized the administrative officers to work out an acceptable distribution of Montana between the Divisions and approved the incorporation of Wyoming into the Southwestern Division. By action of the Executive Committees and Councils of the two Divisions (the Southwestern Division at Tempe, Arizona, April 22, 1953, and the Pacific Division at Santa Barbara, California, June 19, 1953), Wyoming and Montana east of the Continental Divide were made a part of the Southwestern Division and Montana west of the Divide was formally accepted as part of the Pacific Division.

Bozeman, Billings, Great Falls, and Helena are the major Montana membership centers which now become part of the Southwestern Division. Missoula, Hamilton, and Butte are now in the territory of the Pacific Division.

CORRECTION

In the article "The Boston Meetings of the Association: A Bit of Background," (THE SCIENTIFIC MONTHLY, 77, 118) it was stated that of the 15 past presidents of the Association now living five are residents of New England. It should have been *six*: The name of Albert F. Blakeslee, for the past ten years Visiting Professor of Botany and Director of the Smith College Genetics Experiment Station, Northampton, Massachusetts, instead of being listed in a footnote, should have been included with Karl T. Compton, James B. Conant, Harlow Shapley, Edmund W. Sinnott, and Kirtley F. Mather. Apologies are offered to all concerned. *R. L. T.*

THE
SCIENTIFIC MONTHLY

NOVEMBER 1953

A Visit to Barro Colorado Island

WARREN ANDREW and NANCY ANDREW

The authors have worked together on a number of research projects and have published the results jointly. Dr. Andrew is professor of anatomy and director of the department at the Bowman Gray School of Medicine of Wake Forest College. He has done graduate work in the zoological sciences at Brown University and Yale University, and received his Ph.D. from the University of Illinois in 1936. He later received his M.D. from the Baylor University College of Medicine. He has occupied teaching positions at the University of Georgia, Baylor University, Southwestern School of Medicine, and The George Washington University. In 1945–46 he was visiting professor at the University of Montevideo, Uruguay. He is the author of a monograph, a translation from the Spanish of a general cytology by De Robertis, Nowinski, and Saez, and a number of scientific articles. Mrs. Andrew is the former Nancy Valerie Miellmier of Akron, Ohio.

WE had been a long time thinking and talking about the projected visit. Since some forgotten day in the past when a biologist friend had mentioned Barro Colorado we had sensed something of the great attraction which the very name of the place and the knowledge of its biological marvels have for students of the science of living things.

The idea took real substance when Dr. Alexander Wetmore, the Secretary of the Smithsonian Institution, spoke with fine enthusiasm of his own days in Panama, on oft-repeated expeditions.

Barro Colorado is a part of the Canal Zone Biological Area of the Smithsonian Institution, and our request to spend some time on the Island was granted with a minimum of formality. We had read of the richness of the molluscan land fauna and wished to collect specimens of the huge tree snails, both for general and histological study of these forms.

The flight from Miami was pleasant. We had chosen to go by LACSA (Lines Aereas de Costa Rica), and so we found ourselves in a Latin-American atmosphere from the time we boarded the plane. The rather dreary terrain of the southern tip of Florida soon fell behind us, then the long chain of islets to Key West, and soon we were over the beautiful, palm-decked countryside of Cuba. At the airport of Havana we made a brief stop, then were off again, this time soaring into heavy clouds and a driving rain. After what seemed a long time, we sighted the waters again below us. It was not too surprising to us to hear the steward fervently say, "Gracias a Dios," as he pointed to the cape be-

low. We soon recalled that this actually is the name of the jutting headland over which we were flying. Now the green hills of the little country of Costa Rica appeared, and soon, in a lovely valley, the Capital City, San José, with its colorful red-roofed buildings. As we deplaned, we were led by a courteous official through the busy terminus and into a large dining room. Here, instead of a customs examination, we were simply asked, as guests of the Republic of Costa Rica, to choose between beefsteak and chicken for our lunch.

Beyond San José we had wonderful views of the long beaches, of the jungle-covered hills, and the many rivers and streams flowing into the Caribbean. We were impressed by these vast stretches of wild scenery with no signs of habitation nor of any of the works of man for mile after mile.

The works of man, however, became very evident as we soared over the Pacific coast and obtained a fine view of Panama Bay and of the great Canal itself. The Miraflores Locks seemed surprisingly close, and higher up the Canal we could see the Miguel Locks clearly. It is a ride of some fifteen miles from the airport at Tocumen to the Tivoli Hotel in Panama. We were there in time for late dinner and early retiring.

Early next morning we learned that we would have three days in the city and that on Tuesday Dr. James Zetek, the director of the Laboratory, would meet us on the train to Frijoles and the Island. Although we were eager to get on to our real destination, we welcomed the opportunity to learn a little more of Panama City. Especially we wished to visit the famed Gorgas Memorial Institute of Tropical Medicine.

Here we met Dr. Herbert Clark, director of the Institute. His robust physique and hearty manners, as well as his quick intellect, seemed to belong to a man much younger than his seventy-odd years. We knew that he still could put many a younger man to shame in tramping through the rugged jungle country, so often necessitated by the research projects of the Institute. Dr. Clark told us a great deal about the early days of Barro Colorado and the struggle to maintain the ideal of conserving it in its present state, an almost untouched tropical wilderness. He told us also many details of the work of the Gorgas Memorial Institute. It is not nearly as easy as it may sound to effect a program of anti-yellow fever vaccination among scattered communities and family groups of Indians—not even with the aid of the modern helicopter for penetrating the brush—especially when many of the natives may be of another mind.

The laboratories and animal quarters are fascinating places. Sheep graze in the walled-in enclosure about the main building, and in the rear are large cages with monkeys of several types. Here are the big red and black spider monkeys, which have interbred in cages, with good healthy baby monkeys resulting. Here are howler monkeys, vicious macaques from Asia, and delicate, big-eyed night-monkeys.

The insectary presented us with a novel way of raising mosquitoes. Glass microscope slides covered with a growth of nutritious yeast served as the "milk bottles" for the larvae, which eagerly cluster about them.

It was in the second-floor laboratory of Dr. Alexander Fairchild that we first were shown specimens of some of the molluscs from Barro Colorado. They were large and beautiful shells of species which we had not encountered.

Two days later, on Tuesday, we were up at dawn and off on the train for Frijoles. Here for the first time we met our host, the curator and resident manager of the Barro Colorado Island Laboratory, Dr. James Zetek, an internationally famous entomologist. He has done fundamental studies on tropical diseases and is an authority of many years standing on the insects and molluscs of Panama. His work on behalf of establishing and maintaining Barro Colorado Island Laboratory has made his name familiar to all those who have seen the biological treasures of this region. Dr. Zetek introduced us to Dr. Dunn, professor of zoology at Haverford College, Pennsylvania, and to Mrs. Dunn. Both are herpetologists, particularly well acquainted with reptiles and amphibians of tropical America.

At Frijoles we boarded a launch to carry us to the Island. We had not realized that the route of the ships passing through the Canal and Gatun Lake brings them so very close to the Island shores, but now we saw the buoys and shore lights marking their course.

Barro Colorado Island lies between 9 degrees 8 minutes to 9 degrees 11 minutes North latitude, and between 79 degrees 49 minutes and 79 degrees 53 minutes West longitude. The island is almost in the center of the Isthmus of Panama in the Canal Zone. The damming of the historic Chagres River formed the waters of Gatun Lake, and made islands of many hills. Barro Colorado Island is the largest of these. It has a shore line of 42.85 miles and an area of 3840 acres. It is approximately 3 miles in diameter and has a very irregular coastline. It is separated from the mainland at the nearest point by about 300 yards of deep water which includes the channel of the Canal.

Barro Colorado Island became a natural reserva-

tion in 1923 by order of Governor Jay J. Morros, and through the efforts and initiative of Dr. Zetek and Dr. Barbour. The Institute of Research in Tropical America was organized for the development and management of this reservation. Launch service connects the Island with the railroad to Colon and Panama City, and thus supplies are brought to the Laboratory. A network of trails has been cut through the forest and these are kept cleared. Each route is named. Numbers are posted at 100-yard intervals, thus making orientation on the Island and location of points relatively easy. About 1259 species of plants and trees have been identified.[1] Chapman[2] has described over 230 species of birds. The Island has almost a complete sampling of the animals found on the mainland of Panama.[3] Four species of primates are found there and represent four genera—marmoset, night monkey, cebus (capucinis), and howling monkey.[4] The howling monkeys are much less bold than formerly, and their presence generally is shown only by the plaintive howling from the farther parts of the Island. Spider monkeys, indigenous to Panama, are not represented on the Island.

The approach to the Island is very impressive. It rises steeply like a jungle-covered hill from the waters of Lake Gatun, and in fact before the lake was here Barro Colorado was just that—a jungle-covered hill, part of the immense tract of tropical wilderness that stretches from southern Mexico through Central America down onto the great continent to the south.

The only sign of man's activities we saw was a long narrow clearing up which led a tremendous flight of stone stairs, and about which were scattered the buildings of the laboratories and the homes of the few native caretakers. After climbing the stairs, we were shown the roomy well-screened main laboratory building, which serves also as a common dining room and meeting place for the scientific workers in residence. Then we went to our own cabin, a clean and comfortable two-room cottage with shower and bath.

We did not have long to wait for our introduction to the jungle. That morning we set out with a group of "Jungle Survival" airmen who were examining possible sites and methods for training pilots who might have to parachute into just such green fastnesses. We were not without a native guide, for Boca Negra of the Laboratory accompanied us on the trail.

The first trip set before us a whole new world of biological interest. Here a strange, sluggish upside-down sloth hung suspended from a branch. There a great toucan fluttered up as we approached. Giant ants, fully an inch in length and clad in shining black armor, promenaded before us.

Suddenly Boca Negra raised his hand for us to halt and pointed upward silently. There on a high branch was a beautiful yet savage sight. A great white bird, the davilon, perched majestically, a long blood-stained serpent dangling from its beak.

Monkeys somehow seem to typify the jungle. We had heard the strange, distant calls of the howlers several times during our first tramp on the trail. We were introduced to the monkeys in person by the appearance of a troop of cebus monkeys who peered at us curiously and followed us for some distance along the trail. We were back in the Laboratory for a late lunch, and Dr. Zetek regaled all of us with scientific anecdotes and incidents from the history of the Island.

That first evening brought us a minor adventure, but one which we shall long remember. We had stayed in the library, looking over the interesting collections of books and reprints, many of them concerning the fauna and flora of Barro Colorado, until the darkness of the tropical evening had wrapped the little clearing and its buildings in a cloak of obscurity. As we left the library and walked through the quiet and dimly lit main building, we realized two things simultaneously: first, that it was very dark outside, and second, that we had no flashlight or other means of illumination to guide us down the steep and winding stairway which led to our cabin, a hundred or so feet below.

What neophytes we were. True, we had thought about a flashlight in Panama City and usually had one with us on our travels, but somehow or other we had not anticipated such a real need. We decided to light out way by matches—part of the way guided by the dim glow of a match, part of the time feeling our way in darkness. More than once one or the other of us stepped suddenly off the stone stairs and into the grass, only to beat a hurried retreat. Finally we reached the cabin and with the turn of a switch brought back the light and security of the twentieth century. It was not until the next evening, however, that the real climax of that journey through darkness came, when we turned a laboratory flashlight on that hillside and found it literally covered with the burrows of the great hairy tarantulas, the owner and inhabitant of each burrow crouched in waiting close to its entrance.

The nights on Barro Colorado, as no doubt elsewhere in jungle-covered terrain, are not hours of calm peacefulness such as we enjoy in the countryside of temperate climates. They are roistering, clamorous hours of excitement and ceaseless activity. That first night we lay awake for a long time

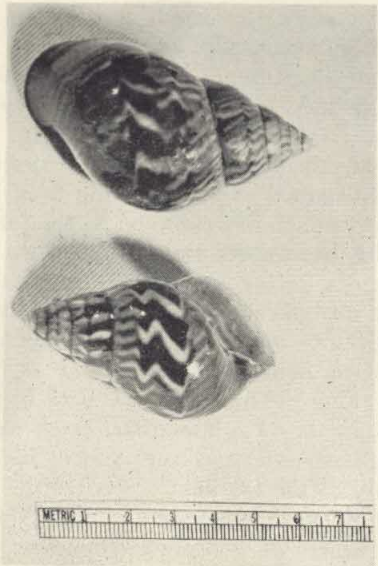


FIG. 1. Shells of the giant tree snail, *Oxystyla princeps* (Brod.). The larger one is almost 3 in. in length. Both shells have slight defects.

listening to the marvelous concatenation of sounds. Just as we were falling asleep a new sound intruded itself among the others, a sweet, plaintive, liquid whistle which has a strangely human characteristic. This was to be the most impressive of all the sounds of the night. We learned later that it is the call of the tinamou, an odd bob-tailed jungle bird, with some relation to ostrich and to fowl, which inhabits the forest floor and lays beautiful eggs that have blended colors of jade and turquoise.

Workers at Barro Colorado must be early risers. Breakfast is at seven and is announced by a bell that also serves as an alarm clock for the late sleeper. We struck out that morning with the Dunns on the Thomas Barbour Trail. Barbour, of the Museum of Comparative Zoology at Harvard, had been very active along with Dr. Zetek in the establishment of the Laboratory at Barro Colorado and was a naturalist in the grand tradition. Emmett Dunn had studied with him, and so we were made to feel personally acquainted with the scientist after whom this trail had been named.

Our first real collecting came on this day. It consisted in gathering a number of fragmented, and a few whole and perfect, shells of the large tree-snail *Pleurodonte otis otorhinus,* which appears somewhat like a mammoth edition of the flattened planorbids found in our own ponds and streams. The next day we added shells of the second large tree-snail, *Oxystyla princeps* (Brod.), to our collection. This species resembles somewhat a tremendous version of the *Lymnea* of our own acquaintance, with a conical pointed shell (Fig. 1).

The real thrill of our collecting for histological study began only on the fourth day. We were tramping along the Snyder Molino Trail, keeping an alert eye on the trunks of the larger trees as we passed. Suddenly, just at eye-level, there it was—a big beautiful, living specimen of *Oxystyla*. We picked him off quickly, before he could even sense the presence of alien beings. He was only the first, and before noon a half-dozen were in our collecting jars. Emmett Dunn found and contributed a fine living specimen of *Pleurodonte.*

It was on this same trip that I had the honor of finding what was said, unofficially at least, to be the largest serpent ever discovered on the Island. I had been poking about under a large fallen tree when I suddenly saw the glint of a very regular pattern of rhomboidal shapes. They were the scales of a giant boa constrictor, coiled tranquilly in this comfortable spot, no doubt to digest a recent meal. The great fat coils of its body were surmounted by a tiny flattened head, in which the eyes were closed. Dr. Dunn, always the eager herpetologist, was all for waking the creature up to see it in action and view its full length. After a number of attempts to stir the great creature out of its somnolence, including gentle pokes with sticks and movement of the fallen tree for a few inches, we contented ourselves with a few photographs and continued on our way.

The days began to pass more rapidly, but each one was filled with a variety of new adventures and

FIG. 2. Mrs. Andrew feeding a deer just outside of the Laboratory.

experiences. The objects with which we became acquainted early in our stay always had some new aspect of interest. A constant source of entertainment after meals was to sit and watch the Gato Solos playing and quarreling or seeking to walk the tight-rope to secure bits of food strung from it.

A graceful deer took a special attachment to Nancy and often visited our cabin for a tidbit (Fig. 2).

The enormous village of leaf-cutting ants with the thousands of inhabitants coming and going on well-marked trails was always worthy of a stop for observation. Along the paths in the jungle less innocent ants were to be found. Among these was the great black stinging ant, *Paraponera clavata* (Fab.). This ant is fully an inch long (Fig. 3). Army ants in long files were seen frequently and were definitely to be avoided.

The heavy-beaked toucans (*Rhamphastus sulphuratus*) flying across the clearing from jungle edge to jungle edge could be followed easily with binoculars. With these instruments, too, we became acquainted with a huge iguana whose favorite haunt was one of the topmost boughs of a giant tree near the edge of the clearing.

The ponderous nests of the termites ornament many of the jungle trees. A slight scratching away of the surface of any nest reveals myriads of swarming white workers. We were hardly surprised to learn that over forty species of termites have been described from the Island. The total termite population is certainly impressive.

At night myriads of strange insects were to be found near the lights of the Laboratory and cabins. One evening we saw a brilliant green, thin body flutter onto our porch. It was a leaf-like mantis, *Choeradodis rhombicollis* (Latr.), as we learned later (Fig. 4). The mantis of the States is a deadly

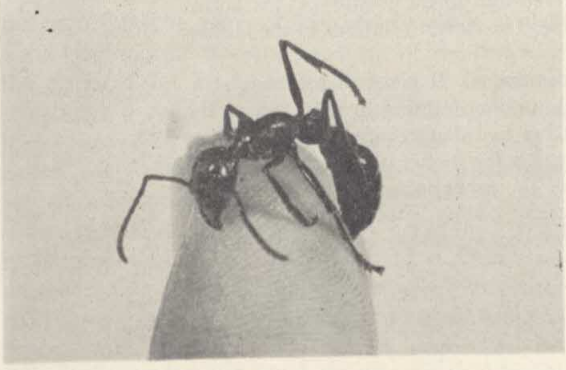


Fig. 3. The large black ant, *Paraponera clavata* (Fab.). This insect is formidable in dimensions and stinging ability.

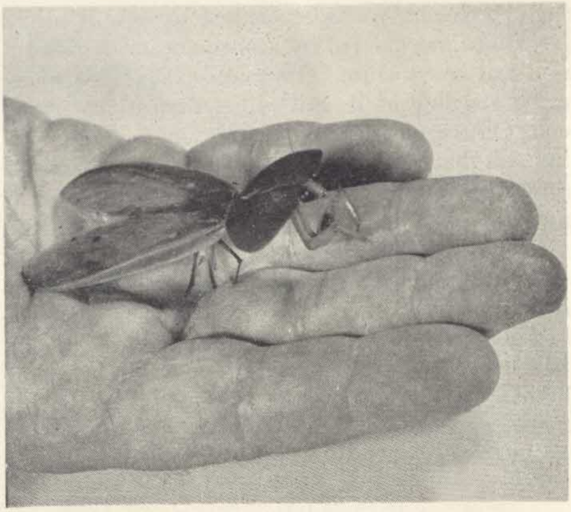


Fig. 4. Specimen of a large, leaf-like mantis, *Choeradodis rhombicollis* (Latr.).

enough looking creature, but here is one which carries his own ambush.

Our work was going well. There was plenty of rainfall and the great snails were present in fair numbers. We had learned how to kill the animal without injury to the soft body and to remove it intact from the shell. The posterior portion of the fleshy body is coiled in perfect conformity to the coils of the shell. In *Oxystyla* the coils are close and tight, in *Pleurodonte* well separated and loose.

One day near the end of our stay we went out with the Dunns to visit the "Big Tree" on the Van Tyne Trail. We had expected something huge, but we were not prepared for the overwhelming size of the great ceiba when we came upon it. There is no way in which to view this tree from a distance, as the growth of smaller trees and tangled vines is dense all about except for a distance of perhaps fifty feet. Its mighty trunk rears directly upward supported by a number of great buttresses. In the dark recesses between these buttresses, where light does not penetrate on even the brightest of days, there are clusters of bats hanging in sleep.

I caught a view of a tiny tree frog climbing nimbly up a vine. Dr. Dunn, who was seeking amphibian eggs, ascended the tree on a ladder of spikes which Boca Negra had prepared. There was a tree hole with water about fifteen feet up, but no eggs were found.

Suddenly Mrs. Dunn called out from the other side of the tree. She had found a fine specimen of the highly poisonous coral snake, its vivid pink coloration standing out against the greens and grays. We were all very interested in seeing it, but as the reptile collection in the Laboratory did not lack

such specimens, we were content to view it alive. Dr. Dunn was excited by a phenomenon which he said was new to him. The snake exhibited a menacing wiggling of its tail rather than of its head-end. On the return trip the only unusual happening was that we ran into a swarm of mites among some bushes and had to assist each other to remove them.

The day of our departure was upon us all too soon. Still, our collections were as complete as we had envisioned them, and while we knew that we had hardly begun to explore the mysteries of the Island, we had come to feel a sense of being at home on the trails and of some familiarity with the many and fascinating living things around us.

Dr. Zetek kindly presented us with two specimens of a large slug (*Veronicella*). We in turn gave him back the "snake kit" which had been lent to us, as it is to other visitors on the Island, "just in case." As we watched Barro Colorado fade into the distance, we felt that here is truly a place which enriches the visitor in biological and personal experience, a place to which some day we surely must return.

References

1. Standley, Paul C. *Contribs. Arnold Arboretum, Harvard Univ.*, No. 5, 21 (1933).
2. Chapman, Frank M. *My Tropical Air Castle.* New York: Appleton. 1929.
3. Enders, Robert K. *Bull. Museum Comp. Zool.*, 78, No. 4, 385 (1935).
4. Carpenter, C. R. *Comp. Psychol. Monog.*, 10, No. 2 (1934).

ANTILOCAPRA AMERICANA, THE PRONGHORN

Sole survivor of a once-great family of North American ruminants, the pronghorn differs from the Old World antelopes in having horns with a bony core and a horny sheath that is forked. This horny sheath is shed and renewed annually, whereas the horns of the true antelopes, like those of cattle, sheep, and goats, are never shed. When Europeans first settled this continent, the pronghorn was widespread and abundant, locally outnumbering even the bison. There were not less, it is estimated, than 30 to 40 millions of them on the western plains, ranging from southern Canada to northern Mexico. By 1924, less than 30,000 remained, and the species would soon have become extinct had not adequate protection started. In 1939, a census indicated that the numbers had increased to over 180,000. In 1945 there were over a quarter of a million, and the pronghorn is no longer in serious danger of extinction.

The pronghorn family traces back to the Upper Miocene, perhaps 10 or 12 million years ago, to an ancestral type, *Merycodus*. In the fossil types the horns were generally much more forked than now. The closest relations of the family are with cattle, the family Bovidae.

The pronghorn is a shy but very curious animal. It can run for miles at a speed of 40 miles an hour, and may reach a mile a minute in short spurts. Its habit of erecting its flaring white tail patches as a danger signal makes it very conspicuous on the plains. Even baby fawns a few hours old demonstrate the instinct to do this. The chief predators of the pronghorn are the coyote and wolf, but much more serious is the danger of disease, often carried by parasites such as ticks, lice, and mites. Few American mammals are as graceful and attractive as the pronghorn.

Philosophy of Science in Scientific Education*

LUDWIG VON BERTALANFFY

The author was formerly a professor at the University of Vienna, where he received his Ph.D. in 1926. He is now professor of biology at the Faculty of Medicine, University of Ottawa. He is the author of Modern Theories of Development *and* Theoretische Biologie. *A summary of his work is given in his recent book,* Problems of Life. *He was one of the founders of the "organismic conception" in biology. His research work is along the lines of comparative and cellular metabolism, physiology of growth, and biophysics.*

MODERN scientific education is presented with a well-known dilemma. The amount of facts in modern science, and in any of its smallest branches, is enormous. Life in general, and the academic curriculum in particular, is short. The abundance of factual data, as well as the intricacy of modern scientific techniques, experimental and theoretical, necessitates utmost specialization. This specialization, unavoidable though it is, involves serious danger for both the education of the scientist and the social function of science.

In so far as the advancement of science is concerned, history shows that interconnection of different fields and problems is a most important basis of progress. Many of the paramount achievements of science arose on borderlines, and from the synthesis of formerly separate fields. This is true of many fields, such as high-molecular, physical, and biochemistry, and virus and cancer research. It applies also to problems that are still under consideration, such as the relation of quantum physics and the theory of the gene, of servomechanisms in electrical engineering and the function of the brain, or the applicability of ecological concepts to human societies. Of course, such integrative fields can be taken care of by making them new fields of specialization with their own departments or subdepartments, and by bringing together, through teamwork, specialists of the branches concerned. This is the obvious thing to do if the field is already established by some clever outsider who, as experience shows, originally was thwarted by all full professors and heads of departments concerned because his newfangled ideas would not fit into the sacred scheme of academic curricula and budgets. But the departmentalization of modern science does little to foster, and much to discourage students to go for adventures in new lines.

Something similar is true for the place of science in general education. It is a platitude that science is one of the driving forces in our civilization. It is equally obvious that science cannot be grasped from the harangues of an inspired lecturer, but only from practical work: making physical experiments, carrying out chemical analyses, looking at cells through the microscope, or dissecting animals. It is, however, less certain whether learning by heart the solution of some differentials, the formulas of some dozens of chemical compounds, or the names of the twenty-nine bones in the human skull, does much for what is called a liberal education.

It is here, I believe, where the philosophy of science comes in. We shall not venture to give a strict definition. Philosophers have debated for more than two thousand years the exact meaning of philosophy without coming to an agreement. Rather I propose to discuss, in a pragmatic attitude, some pertinent topics which seem to be important for the education of the scientist as well as for the general education in our time.

It appears that the main objectives of philosophy of science and, correspondingly, the objectives of instruction in this field, fall under three headings. First, how can the factual data of the individual sciences be integrated into a unitary, although necessarily tentative and fragmentary, world-picture? Second, what are the characteristics and principles of scientific method and thought? Third, what is the impact of science on our civilization?

The need for integration in science is generally

* Address delivered at the Canadian Hazen Conference, June 10, 1953.

understood by scientists as well as educators. For example, a group of scientists, comprising the physicist Bode, the sociologist Mosteller, the mathematician Tuckey, and the biologist Winsor,[1] has urged "The education of a scientific generalist":

> The complexities of modern science and modern society have created the need for *scientific generalists*. . . . We often hear that "one man can no longer cover a broad enough field" and that "there is too much narrow specialization". . . . At all levels, decisions must be made which involve considerations of more than a single field. These difficulties are most pressing in the borderline fields like physical chemistry, chemical physics, biophysics, high polymers, and the application of chemistry, physics, and mathematics to medicine. . . . We need a simpler, more unified approach to scientific problems, we need men who can practice science—not a particular science—in a word, we need scientific generalists. . . . The generalist would naturally be concerned with system problems. These problems arise whenever parts are made into an integrated whole.

Mather,[2] in a symposium on Integrative Studies for General Education under the auspices of the AAAS, discusses the present disintegration of education:

> One of the criticisms of general education is based upon the fact that it may easily degenerate into the mere presentation of information picked up in as many fields of enquiry as there is time to survey during a semester or a year. . . . If you were to overhear several senior students talking, you might hear one of them say "our professors have stuffed us full, but what does it all mean?" . . . More important is the search of basic concepts and underlying principles that may be valid throughout the entire body of knowledge.

In answer to what these basic concepts may be, Mather states:

> Very similar general concepts have been independently developed by investigators who have been working in widely separated fields. These correspondences are all the more significant because they are based upon totally different facts. The men who developed them were largely unaware of each other's work. They started with conflicting philosophies and yet have reached remarkably similar conclusions.

These statements are almost identical with viewpoints that I have advanced myself[3–5] and which have led to the postulate of a general superstructure of science called General System Theory.

There are a number of organizations devoted to integration of science and scientific education, such as the logical positivists with the *Encyclopedia of Unified Science* as their organ, the General Semantics group founded by Korzybski, and the Foundation for Integrated Education. There is a general cry for scientific integration, but it seems that no American or Canadian university does anything about it.

It appears an important task of academic education to show how problems and individual fields are connected in modern science, and how they fit into the modern scientific world-picture. In the following, a program for such a course is given.

Outline of a Course on The Philosophy of Science

The Structure of Nature. A survey of the basic aspects of the levels of organization: elementary units of physics; inorganic, organic, and high molecular compounds; colloidal units; biological elementary units (viruses, genes); organization of the cell; multicellular organization; supraindividual units of life (biocoenoses, animal and human populations in their biological aspects).

The relation of the laws at different levels: physics, biology, psychology, and sociology; application and limitation of physical concepts in biology: mechanistic, vitalistic, and organismic conceptions; application and limitation of biological concepts in the social sciences: body, mind, and society.

Principles of Scientific Knowledge. Observation, experiment, and theory. Model conceptions in science. Induction and hypothetico-deductive systems. The role of mathematics in empirical science. The logical status of laws of nature. Statistical character of scientific laws. Science as a hierarchy of statistics. Attitudes in biological fields: description, comparison, typological concepts. Attitudes in the sociological fields: "nomothetic" and "ideographic" procedures. Laws and individualities. The question of laws in history.

The implications of modern science with respect to the notions of time, space, causality, and finality.

The unity of science. Parallelism in the modern development of the different branches: modern conceptions in physics, organismic conceptions in biology and medicine, Gestalt theory and related developments in psychology, system conceptions in the social sciences. Attempts to create interscientific synthesis. The problem of wholeness. Isomorphic laws in different fields. General System Theory as a way towards unity of science, and as an expedient for a controlled transfer of models and laws from one field to others.

The Place of Man in Nature. Biological foundations: the animal and human body. Neurological foundations: the animal and human brain. Behavioristic foundations: trial and error, instinct, learning, insight.

The uniqueness of man. Language, conceptual thinking, symbolic attitude, tradition, history. The Subconscious and Conscious. General Semantics. The triumph of man's symbolism in his cultural activities. Its dangers and pitfalls. The meaning of civilization.

Of course, this program is only one possible way of organizing instruction in this field, whose aims may also be accomplished in entirely different ways. Fortunately, not even fields like biology, psychology, or economics are standardized, but instruction varies from one teacher and one university to the other. Philosophy of science asks even more for nonuniformity, and full freedom for the teacher's individuality.

There are, however, three general considerations applying, irrespective of the particular program followed. First, with all due respect to the philoso-

phers, a course in the philosophy of science should be given by a practicing scientist, for only he who works practically in a certain field has the necessary immediate experience of scientific methods and problems. Second, a course in the philosophy of science is not a survey course in the way of Introductory or General Science. Rather, it presupposes that the student has already gained knowledge of facts and scientific methods in one or several fields. Therefore, philosophy of science can only be taught in the senior year of college, and, in more elaborate form, in postgraduate education. Third, and perhaps most important: the program outlined is not Utopian, and it should not be objected that it is too high-brow to be practical. I myself have given similar courses for many years at the University of Vienna, and in the form of university-extension courses. The audience comprised people from the most different walks of life. Since, as usual in Europe, such courses are not taken for credits or degrees, their mere existence showed the interest that there was in them.

Foregoing further elaboration of practical matters, let me discuss a few central problems of the philosophy of science which may help to understand better its role in scientific education. The first is the question of the unity of science.

Probably the most systematic attempt to justify the unity of science is logical positivism, often called physicalism. According to the physicalistic thesis,[6] the unity of science is based upon the unity of scientific language. All terms in physics, biology, and eventually also psychology, are, by means of a chain of "reduction sentences," reducible to the "thing-language" that is common to prescientific and physical language and comprises such terms as stone, water, fire, and rain. These eventually can be reduced to "observable thing-predicates," such as hot, heavy, red, large, and thick. Biological terms, like ox or cell, are either obviously reducible to thing-predicates, or can be reduced to them via appropriate physical terms. So the thesis of physicalism is that all terms in science eventually can be reduced to physical terms and observation-predicates. This is the basis for the unity of science. As far as scientific laws are concerned, there is, of course, at present no possible reduction of biological, psychological, and sociological laws to the laws of physics; but, it may be hoped that such reduction will take place in some future.

Physicalism is correct in contending that there is no place in empirical science for any term or statement that cannot eventually be referred to observation, expressible in thing-language and observation-predicates. However, this is no startling discovery. But it is an absolutely different matter whether or not scientific terms and laws can be constituted in this way. Most emphatically not. Reduction statements or so-called operational definitions do not lead to the establishment of any scientific term. We cannot build up the concept of an atom in physics, or of a gene in biology, starting from observation-predicates, for the simple reason that such predicates are not applicable. An atom is neither hard nor soft, warm nor cold, blue nor red. On the contrary, physics progressively eliminates all predicates that are connected with direct observation and that are, therefore, determined by the specific psychophysical organization of man.

The general trend of science is what may be called its progressive de-anthropomorphization, the continuous elimination of characteristics originating in human sensory experience. For example, optics starts from visible colors; then, all sorts of radiations are discovered that are beyond visual experience, and eventually the theory of light and of electricity are united into electromagnetic theory. The theory of heat starts from hot and cold and their quantitative determination in thermometry. Soon one comes to unvisualizable concepts like that of entropy, and eventually the theory of heat merges with mechanics into an abstract field called statistical mechanics. Not only observable thing-predicates, or what in older terminology are called secondary qualities, are progressively eliminated; the same is true of the so-called categories of space, time, and causality which were considered by Kant as being *a priori,* but which prove, in the evolution of physics, to be all-too-human and are consequently de-anthropomorphized. The Euclidean space of classical physics is in no way identical with the space of direct experience; it is already a conceptual construct. This, of course, applies even more to the space-time of the theory of relativity and quantum physics. In this way, everything that is specific of human experience is progressively eliminated. It has often been considered an argument against modern physics that it is reduced to a mathematical system detached from sensory experience, and hence non-visualizable. But this is a proof that physics liberates itself from our specifically human observation, and is a pledge that physics, in its completed form, if this ever could be attained, does not belong to the human world anymore, but is, rather, of universal validity. Let me make this clear by way of a fiction. Suppose there are rational beings on some planet of the star Sirius who perceive not light but x-rays that are invisible to us. The world in which they observe and live would be completely different from ours. But, in a manner

corresponding to that by which we have detected x-rays and other radiations, such beings would detect those undulations which, for us, are visible light. Probably those beings on the Sirian planet would also calculate and theorize in systems of symbols that are totally different from ours. However, since the system of physics, in its completed state, is a system of purely mathematical relations containing nothing that is human, the same would apply to any other physics. Hence, our physics and the Sirian physics, although different in their expressions and symbols, would have the same content. By an appropriate dictionary, it would be possible to translate the mathematical relations contained in one system into those of the other.

Operationalism and physicalism interpret symptoms of scientific concepts as these concepts themselves. What are called operational definitions or reduction statements, that is, the sequence of statements by which scientific terms and laws are ultimately connected with experience, are only directions for possible ways of testing those concepts. For this reason, different methods are applicable; the symptoms can be interchanged, but science is interested in the invariant expressing itself in different ways.

Science is the representation of certain aspects of the universe in symbolic systems. Scientific terms and laws are constructs, chosen in such a way that consequences which eventually can be tested in experience can be derived. In this sense, these constructs are freely created and provable in, but not determinable by, observation-predicates. In the invention of such constructs lies the creativity of the scientific mind.

Behind the logical thesis that all concepts in science are reducible to physical concepts, there lies a metaphysical motive, although this would be sternly denied by the representatives of logical positivism. This motive is that the world, as pictured in physics, is the ultimate reality. The world consists of those elementary particles called atoms, electrons, protons, neutrons, and the like; and the things observed, whether stars and crystals, plants and animals, or brains and mental life, are aggregates or the outcome of those ultimate realities.

In my opinion, things are different. Suppose we have an antique table. From the standpoint of everyday experience, it is a piece of furniture, brown with some gold ornaments, with four legs, and an oval top. From the standpoint of biology, and examined under a microscope, the table consists of tracheal cells of walnut wood. From the standpoint of chemistry, it consists mainly of a rather complicated compound, called lignin, composed, in turn, of carbon, hydrogen, and oxygen atoms. The atoms themselves are composed of electrons, protons, neutrons, and perhaps a couple of other sorts of elementary particles. We make scientific statements about the table as a thing of physics, that is, we indicate certain relations, called laws of nature, applying to this entity. There is, however, another entity, the table as an economic object. Again, the table may be what is called a baroque piece of furniture, built, let us say, in Vienna around 1770. To speak with Humpty Dumpty, the things in physics are "not realler" than those of everyday life, or those of another empirical science. As a Kantian thing in itself, the table is a baroque object as well as a heap of atoms and elementary particles. The only meaning that can be given to this statement is that, if the thing in question enters the experience of an observer, and if it is agreed what the terms "atom" and "baroque" designate, each observer will make the same statements. We can safely say only that there is a "something" that, according to the frame of reference we apply, is an atomic structure, a mass of cells, and an objet d'art. All these viewpoints are constructs helping us to bring order into experience, irrespective of whether this order belongs to the physical, biological, sociological, or historical aspect. The physical laws governing the play of elementary particles and their union into chemical compounds are not more real than the economic laws governing the manufacture and sale of tables, new and antique.

Thus, as opposed to physicalism, we arrive at a standpoint which, if it has to have a name, may be called perspectivism. Science, as well as everyday reasoning, consists of an array of conceptual schemes that help us to find our ways in the world. Naturally, physics is a better science than biology or the social sciences because its theoretical system is much more elaborate and it allows better predictions. Furthermore, the effort towards explanation of ever more processes in living things by means of physics and chemistry is a most fruitful heuristic attitude. Finally, it is a general tendency in science to integrate formerly separate fields, which amounts to deriving from a minimum of primary hypotheses a maximum of consequences that can be tested in experience. However, any symbolic system that we apply represents a certain facet or aspect of reality. The world picture of physics has no singularity or metaphysical pre-eminence. It does not pertain more to the core of reality than do the others.

I cannot claim originality for this viewpoint. It was expressed by Aldous Huxley much better than I can. Remember those wonderful meditations of

Calamy in *Those Barren Leaves*[7] from which I cannot refrain from giving a short quotation:

> Your hand exists as electrical charges; as chemical molecules; as living cells; as part of a moral being, the instrument of good and evil. . . . Universe lies on the top of universe, layer after layer, distinct and separate . . . like a Neapolitan ice. What's true in the chocolate layer, at the bottom of the ice, doesn't hold in the vanilla at the top. And a lemon truth is different from a strawberry truth. And each one has just as much right to exist and to call itself real as every other. And you can't explain one in terms of the other. . . . The only hope is that perhaps, if you went on thinking long enough and hard enough you might arrive at an explanation of the chocolate and the lemon by the vanilla. Perhaps it's really all vanilla, all mind, all spirit.

As we have seen, the unity of science cannot be based upon the reduction of scientific terms to physicalistic language. The unity of science by way of a reduction of all scientific laws to the laws of physics is, at best, a pious hope. Where, then, is unity of science to be sought? From our viewpoint, the answer is to be expected in some uniformity of the conceptual schemes, the symbolic constructs in science.

If we survey the evolution of modern science, we are witnessing a remarkable phenomenon. Similar concepts and principles have arisen in quite different realms, although this parallelism is the result of independent developments, and the workers in the individual fields are hardly aware of the common trend.[3] For example, principles of wholeness, of organization, of a dynamic conception of nature have appeared in all fields of science. We see them active in modern physics, as opposed to the "blind play of atoms" in classical theory. They appear in biology in the form of organismic conceptions as opposed to the analytical, summative, and machine-theoretical conceptions of the biology of yesterday. They are manifest in the replacement, in psychology, of the older conceptions of classical association psychology by Gestalt theory and related theoretical conceptions. They are found equally in modern developments of the social sciences. Civilizations appear, if not as supra-organisms, as maintained by Spengler, at least as supra-individual units or systems, as expressed in Toynbee's conception of history. Thus, similar general viewpoints are present in all branches of science, irrespective of whether inanimate things, organisms, or mental or social phenomena are concerned.

This parallelism goes even farther. In many instances, we find a formal correspondence or isomorphy of laws in different sciences, in different fields or strata of reality. The same exponential law, for example, applies to the decay of radium, the death of bacteria under the action of a disinfectant, the extinction of a population where death rate is greater than birth rate, and many other phenomena. Systems of chemical reactions are governed by the same kind of equations that apply to the struggle for existence within animal populations, and to economic problems. There are hardly phenomena more different than the development of whole animals from divided animal germs, the regulations in the nervous system after injuries or resection of parts, and the phenomena of Gestalt perception. Nevertheless, the principles governing all these phenomena are strikingly similar. To take an example from the social sciences: in the history of Germanic languages, it is found that, starting from a certain parent language, phonetic changes have taken place in a parallel way in different nations inhabiting widely separated areas. A similar principle of independent but parallel changes is well known in many evolutionary series in paleontology.

In this way, there is a uniformity or isomorphy of laws in different fields. One of the roots of this isomorphy is obviously that the number of conceptual schemes or models we possess is not infinite but rather limited, so that similar schemes will appear in quite different fields.

This fact has led to the postulate of a new basic discipline called General System Theory.[4, 5] General System Theory is a logico-mathematical field concerned with the principles applying to systems in general, irrespective of the nature of their component elements and the relations or "forces" between them. Such theory leads to an exact formulation of many terms which have, as yet, been conceived in a vague, anthropomorphic, or metaphysical way. The fact that all sciences mentioned are concerned with systems, leads to formal correspondences in their general principles, and even in their special laws if the conditions correspond in the phenomena under consideration. Methodologically, a general theory of systems can develop model conceptions applicable to different fields, and so make unnecessary the duplication or triplication of the discovery of identical principles in different fields, as it has often occurred in the history of science.

This has considerable implications for the question of the unity of science. So far, the unification of science has been seen in the reduction of all sciences to physics. We have seen, however, that, in so far as scientific terms are concerned, they are hardly reducible to physicalistic language. If unification of science is seen in the reduction of all scientific laws to physical laws, this could be achieved, if ever, only in an inscrutable future. From our

point of view, unity of science gains a much more concrete aspect. A unitary conception of the world will be based not upon the possibly futile and certainly farfetched hope finally to reduce all levels of reality to the lowest level, but on the structural isomorphy of laws in the different fields of science. In particular, the gap between the natural and the social sciences will be diminished, not in the sense of a reduction of the latter to biological conceptions, but in the sense of structural uniformities.

It seems that the viewpoint that I have called perspectivism goes a long way in understanding the dignity as well as the limitations of science. Science is not a mere accumulation and catalogue of facts. It is a conceptual order we bring into facts. Science is not primarily a cookery book for inventing new gadgets. Applied science is possible only on the basis of detached fundamental research, technology being able to invent new applications exactly so long as the prerequisites are given by pure science; and there is a considerable danger of exhaustion. The present way of premium given to applied science, and of making basic science an ill-paying job for incorrigible idealists may, in not too remote a future, frustrate new theoretical developments and, as a necessary consequence, even technological progress. Science is a creative activity, and its fundamental advances are in the adventure of bold constructs, always checked by, but never simply resulting from, a mere increase of data. Science is not a materialistic or mechanistic metaphysics, reducing physical things, life and mind to a senseless play of atoms. It is one of the ways man has created to portray certain traits of reality and to control his destiny.

It seems to be one of the tasks of scientific education and of the philosophy of science to make clear this austere but unavoidable conception of science and to destroy popular illusions that are ingrained and common. Judging from the popular magazines, science means, first, new gadgets: the most brilliant television set, the fastest jet plane, the most devastating atomic bomb, and the new wonder-drug curing tuberculosis or cancer or arthritis or, preferably, all of them. Further, it means scoops or freaks: the galaxies farthest away, the quintuplets, the conversion of a G. I. into a girl with a subsequent Hollywood contract. Above all, it means progress, faster cars, more labor-saving devices which give more leisure to study comics, to hear advertisements of washing powders via the radio, and to watch soap operas on the television screen.

In our days, we can no longer conceive of science as being the superhighway to Utopia. Or perhaps it is, but Utopia is not so good a place to live in. It is obvious that telephones, refrigerators, and antibiotics are good things. It is less obvious whether movies, television, and other forms of mechanized entertainment are exactly cultural achievements; and machine guns, atomic bombs, and bacteriological warfare are definitely unpleasant. Whether, in the overall picture, the benefits of science compensate for its disadvantages, nobody is able to tell. It is disconcerting to proclaim science as being the motive force in our civilization if, in the next breath, it must be confessed that civilization may be smashed by a few atomic bombs. What a world governed by a strictly scientific and, for that matter, benevolent tyranny would look like, we know quite well since Aldous Huxley wrote *Brave New World* some twenty years ago. We are not so advanced as to breed human beings *in vitro,* and so to guarantee genetical homogeneity to the castes of society, but modern governments are pretty good already in standardizing public opinion by way of conditioned reflexes. Science cannot be justified by utilitarian reasons, nor be explained as arising from economic causes.

Science as the Sacred Cow is another popular illusion. The prestige of science and the domination of the modern world by science are fairy tales invented by college professors in order to compensate for their inferiority complexes. Scientific achievements are employed to precisely the extent politicians and businessmen see fit—that is all. Other equally important and far-reaching results are disregarded. Thus, science can predict very well the consequences of the devastation of our natural resources, of deforestation and of the erosion of the top soil; ways are known to increase yields in underdeveloped areas to a multiple of the present standard. But deforestation goes on, to print pulp magazines; and crops are controlled, to keep prices on the world-market stable. If a scientist is needed to prove that a chlorophyll toothpaste is the best against halitosis or if he is needed to make atomic bombs, he is paid a high salary indeed—that is, a fraction of the wages of a fashion model, a television starlet, or a junior executive in a firm selling brushes. The highest scientific award, the Nobel Prize, amounts to somewhat less than the purse in a minor boxing championship. Since the best authorities agree that economic values are determined by the law of supply and demand, it is obvious that the discovery of penicillin or of quantum mechanics has less "societal value" than knocking out the other tough fellow.

What, then, are we going to offer if it is one of the tasks of the philosophy of science to render an evaluation of science? The answer, I believe, was

given by G. B. Shaw's *Don Juan*. Like civilization, science is "an excellent peg to hang cynical commonplaces on; but before all, it is an attempt on Man's part to make himself more than the bare instrument of Women's purpose."

It remains to be seen whether the march of science is triumphal or macabre. One thing, however, is certain. Science is one of those symbolic worlds man has created for mastering the great enigma of the universe, the creation of which is part of his uniqueness. All reasoning consists in the substitution of symbols—concepts, words, mathematical signs—in lieu of reality. Through its immanent logic, this system of symbols gains, as it were, an existence independent of and surpassing the individual life and psychology of its builders. It is in this way that science, in theory, leads to prediction, and, in practice, to the control of nature. The technological mastery over natural events demonstrates that the theoretical system is more than a castle in the air, that it corresponds to certain traits of reality.

However, none of the worlds of symbols, the sum total of which is called human culture, is a full presentation of reality. Remember those precious etchings by Whistler: the lagoons of Venice, the bridges, San Marco and its doves—just a few scratches of the needle, giving, nevertheless, an unfathomable perspective of Italian serenity. Whistler called painting the art of omission representing things by a few characteristic traces. Something similar is true of science. It is not concerned with the innermost core of reality. But it is one of the perspectives of reality, representing, by means of interconnected symbols, certain traces of reality, namely, the orderliness in the relations of things. This, however, is sufficient to allow for theoretical as well as practical mastery of nature.

It is said, and it is true in a way, that the prelude of Wagner's "Rheingold" gives a picture of creation: moving upwards from the keynotes of the flowing element to ever higher and more organized, life-imaging melodies, till finally the voice of the elfish daughters of the Rhine emerges. It will be difficult, however, to learn much about the evolution of the vertebrates from Wagner. Da Vinci's "Gioconda" fortunately reveals little of human anatomy as represented by that admirable lady, although, incidentally, Leonardo was a keen and quite modern anatomist. What science can do is to symbolize reality in its way, knowing, as its great masters always did, that this is but a humble way to redraw a few traces of the great blueprint of Creation.

References

1. Bode, H., *et al. Science*, **109**, 553 (1949).
2. Mather, K. F. *Main Currents of Modern Thought*, **8**, 4 (1951).
3. Bertalanffy, L. von *Problems of Life. An Evaluation of Modern Biological Thought*. New York: Wiley; London: Watts (1952).
4. ———. *Brit. J. Phil. Sci.*, **1**, 139 (1950).
5. Bertalanffy, L. von, *et al. Human Biol.*, **23**, 302 (1951).
6. Carnap, R. *Intern. Encyclopedia of Unified Science*, **1**, 42 (1938).
7. Huxley, A. *Those Barren Leaves*. London: Chatto & Windus (1925).

Small-Town Fluoridation Fight

JOHN A. HUTCHISON

The author, a graduate of Lafayette College and Union Seminary, received his Ph.D. from Columbia University. He has taught philosophy and religion at the College of Wooster, and at present is the Cluett Professor of Religion at Williams College. He was a participant in the fluoridation fight described in the present article.

THE question of fluoridating the local water supply first came to the attention of our town when it was placed on the warrant or agenda for the 1952 town meeting. At the time most people gave fluoridation little chance for our town is traditionally conservative, particularly on public health issues. However, at the town meeting there were persuasive speeches citing the experience of other towns and cities in greatly reducing dental decay among children by adding minute amounts of fluoride to the water supply. Two local dentists testified that the American Dental Association had endorsed fluoridation as a safe and effective way to reduce dental decay. So, unexpectedly, the measure passed by the overwhelming vote of 302 to 2.

But this was only the beginning of what was to prove a long story—a story by turns tragic, comic, and frightening—and a story with many ramifications. The issue of fluoridation itself, while surely not of as great magnitude as many which come to public attention, is by no means unimportant. A state public health official recently characterized it as one of the best gifts which any town might give its children. But beyond the immediate issue, the controversy that it raised in our town has presented, in capsule form, many of the urgent and unsolved problems of public discussion in a democracy.

The scene of the story is a small picturesque and rather isolated New England town. Of its approximately 5000 population a few hundred are connected with the local college; another several hundred are professional people, many of them connected with local and nearby industries; the rest are laborers (local industries are nonunionized), small businessmen, farmers, and retired people. Long-time residents look with scorn and suspicion on newcomers. It is sometimes said that a man must have lived here for twenty-five years before he applies for naturalization.

Soon after the town meeting there were rumblings of opposition. As a consequence, the selectmen acted slowly in carrying out the voters' mandate. But when the opposition failed to materialize by midsummer, they investigated and placed an order for equipment. It did not arrive until January, 1953. By that time opposition was vocal and vigorous. An item was placed on the warrant for the forthcoming 1953 town meeting to rescind the action of the previous year and to sell the recently purchased equipment. In the face of such developments the selectmen decided not to operate the new equipment until after the 1953 town meeting, scheduled for early in February. Advocates of fluoridation asked what authority the town fathers possessed to stall in carrying out the people's vote. Some charged that the selectmen had dragged their feet throughout the year. Meanwhile, those who claimed to have a finger on the public pulse, predicted that flouridation would be overwhelmingly rejected.

The opposition expressed its views in a series of letters to the local paper. One letter charged that fluoridation was a dangerous practice, foisted upon the town by an evil alliance of misguided doctors and the companies that sell fluoridation equipment and supplies. Another said that fluorides were rat poison, and that if an enemy were to gain control of the town water supply our whole population could be silently and efficiently exterminated. Several charged mass medication and socialized medicine. Another letter asserted that too little is known about the subject and asked for an unbiased and expert committee to study it. Others cited data alleging to prove fluoridation harmful to the sick, aged, and to pregnant women. Cities which had adopted fluoridation were claimed to show a marked increase in heart disease, kidney disease, bone fluorosis, mottled teeth, stillbirths, and a variety of other maladies. Some added bluntly that they just didn't want any more chemicals in the drinking water.

Opposing attitudes ranged the whole spectrum of belief from the thoughtful conservatism which argued that we ought to wait a few years until more is known, to the dogmatic and frantic assertion that fluoridation is a satanic (or communist) plot. Individuals known throughout the region for their opposition to such measures as pasteurization, compul-

sory vaccination, and chlorination of water, entered the discussion and made their views heard.

Advocates of fluoridation sought to meet the issues. They read scientific articles and wrote letters to the paper, stating facts and figures from the Newburgh experiment, giving statistics from areas of the country where fluorides occur naturally or are added to drinking water, showing that dental health is greatly improved and also that no harmful results follow. The opposition countered with the distinction between fluorides which occur naturally, "as God made them," and "man-made," or artificial fluoridation, the former being beneficial and the latter harmful. They also quoted the Report of the Delaney Congressional Committee with its counsels of caution and its suggestions of possible long-time harmful results. A *Harper's Magazine* article entitled "Fluoridation—Go Slow," was widely read. Opponents spoke darkly of local doctors who were "agents" of sinister but unspecified powers and of dentists who were promised a cut in the sale of fluoridation equipment and supplies. But above all, they bore down on the argument that the long-time effects of fluorides are unknown. Who knows their effects? Who knows? Who knows? They repeated this with almost hypnotic effect.

Advocates sought arguments to stem the tide of misgiving and fear. They pointed out the high rate of dental decay in our region. They read and analyzed the Delaney Report, with its inaccuracies and its innuendoes, and also the American Dental Association's devastating analysis of the report. They mimeographed and distributed figures for death rates in regions having fluoridation, showing no divergence from the rates of the country as a whole. They argued that the issue came down in the end to a question of whom the people are to believe, of what authority they are to trust. On the one hand were the American Dental Association, the American Medical Association, the Federal Public Health Service, the state health board, the American Waterworks Association, and many other reputable and competent organizations which have endorsed fluoridation. On the other hand were the few individual doctors, dentists, scientists, and self-constituted local experts who questioned or opposed it.

Feeling ran high at two preliminary public meetings where the issue was discussed. At one meeting, called by the League of Women Voters for the purpose of discussing the warrant for the town meeting, there were speeches pro and con. One opponent raised the issue of constitutionality, arguing that fluoridation is an infringement of individual liberty and citing legal decisions purporting to prove the point. Another spoke of an alliance between local "so-called doctors" and the Aluminum Company of America, which would presumably supply fluorides. She also stated knowingly that Oscar Ewing, recent Federal Security Administrator, had formerly been an official of the same company.

At another meeting called by advocates of fluoridation, there were speeches by a representative of the United States Public Health Service and by a medical man with years of research experience in fluoridation and its effects. Opponents came to heckle and filibuster. The net outcome of the meeting was aptly expressed in the newspaper headline, FLUORIDATION DISCUSSION LEAVES 125 MINDS UNCHANGED.

A climax was reached at the town meeting. Of a total of approximately 3200 registered voters some 600 turned out, a fairly good number and about all the local school gym will hold. Many had come to vote only on this issue. Feeling ran so high that when this item on the agenda was reached, the moderator called a ten-minute recess. He introduced the issue with an appeal "not to let angry passions rise," saying that he would alternately recognize pro and con speakers.

The first speaker, an advocate, urged the meeting to defeat the motion to rescind, emphasizing the benefit of fluoridation to children's teeth and the recognized safety of the procedure. The first opposition speaker, a man well-known in town meetings for his practice of making speeches in opposition to almost everything, said that in his observation our children's teeth are good, but that few people over fifty have stomachs that are worth anything. He did not spell out the implication that fluoridation was harmful, but the point seemed widely understood.

A doctor who is a member of the town health board, and who had studied the issue carefully and extensively, explained the experimental work which had led up to its approval by the dental and medical associations. He also described the scientific investigations supporting the conclusion that fluorides in the amount contemplated are harmless to everyone, old and young, sick and well. An opposing speaker dwelt upon the possibility of poisoning the whole population if excessive amounts were dumped into the water supply at one time. Scientists attending the meeting writhed in silent anguish while the speaker confused and misconstrued elementary scientific facts. Another doctor and two dentists added their testimony that fluoridation was a safe and a desperately needed step toward better health.

An opposition speaker said that her ancestors had fought for their lives in this region against the Indians and that the time had come to fight with

equal vigor against fluoridation. She said that it had been proven useless and harmful in many cities and she defied the proponents to deny this. A local lawyer stated that he had checked the legal issues and that in his opinion fluoridation was legal in our state—an opinion in which the state's attorney general had recently concurred. A final opposition speaker, sensing that the tide of argument was running against her side, made an impassioned plea for the "little people" to rise against authority. They had done so in the American revolution, she argued, and should do so in the present emergency, defending the right of the individual "to control what goes into his own body."

The vote was a victory, 291 to 249 for fluoridation. Proponents heaved a sigh of relief, thinking that the cause of science and public health had been vindicated in our town. The result was headlined in the newspapers of many surrounding towns and cities. However, the relief was premature. Feeling was still intense and bitter; one person who had spoken for fluoridation in town meeting was accosted on the main street of town as a "stinker" and a "communist." One of the leading opponents of fluoridation said to a friend as they left town meeting that their work was now cut out for them. Meanwhile, instructed by two successive years' town meetings, the selectmen proceeded to put the fluoridation equipment into operation. Newspaper articles and pictures heralded the event and state health officials described inspection procedures and safeguards. One lady telephoned the water department complaining of stomach pains which she attributed to the fluorides, only to be told that she lived in a part of town supplied from a reservoir not then being fluoridated.

For a few days the town wondered if the issue would be dropped. This uncertainty was soon removed. After a series of discussions an Anti-Fluoridation Association was organized, officers elected, and a statement formulated, charging the moderator of the town meeting with bias, alleging that illegal votes had been cast, and raising the issue that the whole meeting had been illegal since it had been held in a hall too small for all the town's 3200 registered voters. The last charge, which is technically correct, would invalidate all of our town meetings for many years past. They made plans and raised funds for a hard campaign. They circulated petitions asking for a special town meeting at which there would be a secret ballot on fluoridation. The work was done thoroughly, and as a result 932 signatures were secured for the petition. The committee reported mysteriously that several other pages of names had been stolen from their headquarters.

The campaign against fluoridation gained momentum. Flushed with success in the petition, some members of the Anti-Fluoridation Association boasted that once they had won this fight they would turn their attention to other issues of good government. They did not specify what they meant. They continued to fan the flames of fear. Fluoridation is harmful to the kidneys, to the heart, and to the bones. Who knows what results may follow from its use? Who knows?

This phase of the campaign came to a climax in a public meeting, billed as The Facts on Fluoridation, and addressed by three dentists opposed to it. The leading speaker was billed as senior dental consultant of a well-known hospital in our region. A phone call revealed that the position is purely honorary, that he is not at present a member of the hospital staff. He claimed to have the signatures of 119 dentists in his city who are opposed to fluoridation. The signatures turned out to be several years old, a large percentage of the signers having subsequently changed their minds and endorsed fluoridation. At the meeting a package of sodium fluoride labeled "Poison" was plainly displayed at the front of the room. At the opposite side of the platform was a large container of nonfluoridated water from which the speakers drank freely during the meeting. The speeches rehashed the half-truths, the discredited arguments, and the misinformation. A few supporters of fluoridation who attempted to ask questions were heckled. The meeting ended with a spirited diatribe from the floor against the current addition (without popular mandate) of a water softener in our water supply.

The proponents of fluoridation were puzzled to know what course to take. If a large number of people were frantically opposed to fluoridation, however invalid their reasons, was it wise to press the issue? It was felt that misstatements of fact should be publicly corrected. But in limited time and without an expense account, it was impossible to trace and check all statements. Furthermore, a correction, however fully and accurately documented, did not in this case, as so often it does not, undo the effect of an original false statement. It seemed to them that many people were not open to facts, often rejecting carefully stated factual evidence from competent and reliable authority in favor of gossip and hearsay based upon the flimsiest evidence, and full of loopholes and contradictions. But most of all, proponents of fluoridation were puzzled to know why this issue had generated so much heat that long-time residents of the town can remember nothing comparable.

Seeking answers to such questions they recog-

nized a basic error in strategy in bringing the issue to a vote without a long and thorough campaign of education. Failing this, they found that they had a tiger by the tail.

They also noted that the issue was made to order for exploitation by fear and demagoguery. It is a highly technical question on which only a few trained experts have any first-hand right to an opinion. The public, knowing little or nothing of the technical questions of chemistry and biology, and hearing claims and counter claims bandied about by self-styled experts, is justifiably puzzled as to whom to believe. While all major medical and health organizations have endorsed fluoridation, there is a small minority of dentists, doctors, and scientists who have misgivings or are opposed. The public asks, "Since the experts disagree, should we not wait awhile?"

No science, except perhaps pure mathematics, ever achieves complete certainty in its results. What it offers is an impressive body of overwhelmingly probable facts—facts which gain in probability with each successful experiment but which never achieve complete certainty. Moreover, scientists, as men of intellectual integrity, do not want to claim too much for their results. Thus, when a scientist is asked to give absolute assurance for what will take place in the distant future he hesitates or demurs. But this limitation of scientific knowledge provides the opening wedge for deep-seated fear. So the unanswerable question recurs, "Who knows what results will follow?" Who knows? Such fears are also accentuated by the new and unfamiliar nature of the scientific processes involved in fluoridation.

Deeper than any specific fears or issues lies a free-floating fear or anxiety in the subterranean depths of men's minds. This unreasoning anxiety attaches itself to different and often unrelated issues. Thus, questions as far apart as Senator McCarthy's investigations and the fluoridation of a small town's water supply show a similar emotional pattern. It is this attitude, expressed in so many ways, which has led some observers to charge that American public opinion is in the grip of a fear psychosis. In our town this fear psychosis found a vehicle in the fluoridation issue. Health issues impinging directly upon individual lives have been effective in dredging up such fears, from the first days of compulsory vaccination to the present. The fruits of this fear, in bitterness, dogmatism, charges of guilt by association, misrepresentation of facts—as often to oneself as to others—suspicion, and intolerance of any disagreement, were all illustrated in our controversy.

But perhaps the most fundamental attitude was a deliberate rejection of reason and rational authority by the antifluoridation forces. In an age when storms of unreason have swept like devastating floods over many nations it is significant to trace such attitudes to their source in fear. Truly, big lies from little misstatements grow when they are fed by fear.

A further observation is the ineptitude of the time-honored New England system of direct democracy in the face of attitudes and problems of this sort. Any sharply controversial issue tends to disfranchise those citizens who are dependent upon public goodwill. For this reason many merchants in our town have stopped coming to town meetings. Systems of indirect or representative democracy provide means for giving weight to expert opinion on technical matters. No such means exists in the town meeting system.

As the time for the secret ballot drew near, the Anti-Fluoridation Association bought advertising space in the local paper, repeating and underscoring its charges. A crescendo was achieved the day before the election in the warning that fluoridation would produce a "third generation, degenerated to crippled Pygmies." The only organized effort of the profluoridation forces was a letter to the newspaper, signed by six of the seven local doctors, stating that they had faith in the reliability of federal and state health agencies, and that in their opinion fluoridation would be both beneficial to dental health and harmless to the population at large. A college professor of science pled in newspaper letters for reason against emotion. The result was a vote, 1114 to 415 against fluoridation. It was an authentic social revolt on the proto-fascist pattern. Advocates suggest that in a few years after emotions have cooled and public judgment is restored, the issue can be raised again and can receive a fair hearing. Opponents reply that fluoridation will take place in our town over their dead bodies.

Responsibility of the Scientist to Society*

AARON J. IHDE

The author is associate professor of chemistry and integrated liberal studies at the University of Wisconsin, where he received his doctorate in 1941. In addition to his courses in general chemistry and physical science, he is carrying on a program of investigations in the history of chemistry and is interested in the relationships between scientific and cultural developments. He is the author of "The Inevitability of Scientific Discovery," which appeared in THE SCIENTIFIC MONTHLY, *December, 1948.*

SCIENCE has developed in the last three centuries from an intellectual pursuit of a small minority of curious and sometimes gifted amateurs to a professional activity of thousands of highly trained members of our population. It has changed from an activity in which the masses had essentially no interest to one that has become front page news. It originally gained many of its answers from practical men—miners, navigators, arms makers, barber-surgeons, herb collectors—and contributed little in return. It now draws little from technology and contributes heavily in return.

The sudden rise of science as an intellectual activity has naturally created social problems of first magnitude. Science has not only had a physical effect in changing man's material way of life but has resulted in mental upheaval as well, the latter a result of changes in man's understanding of nature and in his attitude toward knowledge. Bringing about such changes in the culturally short period of three centuries—and much of it in the past century—could not help but create problems defying solution.

We may well raise the question, "What is the responsibility of the scientist to society?" The question has been the result of much discussion on the part of both scientists and non-scientists. The answers range from one extreme to another. On the the one hand we find those who advocate a moratorium on scientific activity as did the Bishop of Ripon when he declared in 1927, ". . . the sum of human happiness outside scientific circles would not necessarily be reduced if for ten years every physical and chemical laboratory were closed and the patient and resourceful energy in them transferred to recovering the lost art of getting on together and finding the formula of making both ends meet in the scale of human life. . . ."† Contrasting with this negative approach to dealing with the impact of science on society there is the irresponsible exploiter of applied science who holds that man is by nature a competitive creature so he must be left free to do as he wishes. This person insists that the scientist has no responsibility to society—or may even insist that the scientist's responsibility is to keep out of social affairs so that commercial success can be left solely to, "the play of the market place," to quote a currently well-known Secretary of Commerce.

To me it seems that neither of these solutions is realistic. The Bishop of Ripon and those of like mind wish to ignore the whole problem by returning to the *status quo pro scientia,* or failing to attain that problemless golden age they will settle for the *status quo pro 1927.* They would give up the lengthened life span that can be traced back to the studies in optics leading to the compound microscope (and through the compound microscope the knowledge of the living cell, the germ theory of disease, and chemotherapy). They would give up the material comforts of living in an age of diversified diet, varied clothing, rapid transportation (even with its attendant evils) and rapid communication. They would give up a system of production which, in the more advanced countries at least, has made possible a degree of leisure permitting educational opportunities to be extended from the aristocratic classes to the masses.

The second critic of science, who would permit the scientist merrily to develop practical things as long as he took no responsibility beyond develop-

* Originally presented before the First Unitarian Society, Madison, Wis., on July 27, 1953. Broadcast over the Wisconsin State Radio Network July 31, 1953.

† Bishop of Ripon, (London) *Times,* September 5, 1927, p. 15.

ment, strikes me as equally dangerous. He is the advocate of a return to the law of the jungle. He closes his eyes to the fact that human civilization is a cooperative enterprise—that civilization rose not because of competition but in spite of it. Whenever men have banded together to accomplish a feat not possible for single men there has been progress in the control of the environment. Whenever control of the environment was uniquely successful there has resulted a period of cultural development. It was then that some men had the leisure and the freedom for intellectual pursuits—philosophical, artistic, social, scientific, technological. It has been then that understanding has developed, and with understanding—particularly scientific understanding—greater measures of control of the environment. Let us beware of any plea which would have us abandon the cooperative way in favor of unbridled competition.

What then should be the position of the scientist regarding his obligations to the social order? Should he proceed with his chosen work oblivious to the consequences of his investigations? Should he drop his work in order to apply his talents to the social problems that have been created by science? Or is there a rational middle course to be followed? It seems to me that there is no one answer that suits all situations, though I lean toward a middle course.

It would seem desirable that the investigator be given a large degree of freedom to pursue his researches without thought of possible effects. I feel this way because so much research is without social significance, at least at the moment. In fact, the future significance of much investigation is highly problematical. This is particularly true of what is called "pure research," or by some "worthless research." We cannot hold the investigator responsible for the impact of discoveries whose effect may only make itself felt several generations later—and felt in unforeseen ways.

Take the researches of Michael Faraday for example. Among the many things studied by Faraday at the Royal Institution was the relation of electricity and magnetism. The experiments of Oersted had revealed that electricity flowing through a wire set up a magnetic field—at least a compass needle nearby was deflected. Faraday became interested in these experiments, not because he had a burning desire to create the electric power industry but because he had a simple faith in the unity of nature. Here was a hitherto undetected relationship between electricity and magnetism. Greater understanding of the relationship might lead to better understanding of all of nature and Faraday carefully pursued electromagnetic researches for two decades. He sought to convert magnetism into electricity since, he reasoned, if an electric current was accompanied by a magnetic field, should not a magnet be capable of creating an electric current? The question was not answered at once. Such questions seldom are in science. The most obvious experiments were failures. But in time the phenomenon of electromagnetic induction was discovered.

These researches were fundamental to the generation of electric current by mechanical means and led ultimately to the electric power industry. It is possible to say, in a rather loose sort of way, that Faraday was responsible for commercial electricity in all its manifestations. We may conceivably honor Faraday for the comforts he provided whenever we turn on a reading lamp, an electric stove, or a vacuum cleaner. Or we may curse him for creating a monster when we lose our savings in an Insull debacle or when a promising young man is electrocuted while repairing a power line.

Such attitudes, however, are not only unrealistic but dangerous. They oversimplify the whole story by ignoring nearly all the facets in the development of electrical power. They ignore the fact that Faraday had no intention of creating a new source of power and had no conception of the future impact of his experiments on electromagnetic induction. They ignore the fact that Faraday never attempted to develop a commercially practical dynamo. It was nearly four decades after Faraday's fundamental researches that the Belgian engineer, Gramme, developed such a dynamo. They ignore the fact that Faraday dropped these experiments in favor of researches on the influence of magnetic fields on light. They ignore the fact that the time was ripe for the discovery of electromagnetic induction; that is, the previous fundamental steps had been taken so that someone was due to make the discovery. In fact, Faraday was actually anticipated in some of his discoveries by the American Joseph Henry.

It is clear from my example that it is absurd to insist that Faraday is responsible for the good, or the evils, of the electrical industry. All that we can honestly say is that he played a role in the development. Beyond that, he can neither deserve praise nor blame. His motive in carrying out his researches was not one of commercial development but the more important one of gaining a slightly better understanding of nature. He was investigating pure science or, as some would say, he was doing impractical or worthless research.

Let me make a plea for the freedom to do worth-

less research. It is the worthless research of today which stimulates the technological progress of tomorrow. True, much of the worthless research remains buried in the scholarly journals unused. One may argue that the time spent on such investigations has been truly wasted. This waste is inevitable and is not actually too serious a waste since positive knowledge, even if nonutilitarian, represents a gain in understanding. But the main point we must consider is that no one is so all wise that he can know which piece of worthless research will remain a mere addition to man's knowledge and which will become basic to practical developments. Therefore, it is necessary and desirable freely to probe into the unknown solely for the sake of advancing knowledge. In doing so the scientist must not be held responsible for the long-range effects of his discoveries.

In view of the foregoing statements it must not be supposed that I feel that the scientist is without responsibility to society. As a citizen, he has the same responsibilities of good citizenship that are expected of any other member of society. As a highly trained person, society has a right to expect more of him than it expects of most. This is particularly true of the area of his competence. Here the scientist is in a position to speak with authority and he has an obligation to give society the benefits of his position. At the same time he has an obligation to bow to superior wisdom when he is outside his field of competence. All of us, I am sure, have seen respected members of a given field, say medicine or banking, speak out loud and long on problems of economics, politics, and religion. We are familiar with books on religion written by scientists who speak with a lack of objectivity sometimes equaling that of the most bigoted fundamentalists.

What, in particular, are the areas where the scientists are best fitted to make a contribution to society? As I see it the scientist has the right, even the obligation, to speak and act in the following areas which I shall indicate as the governmental, the sociological, and the educational. Let us first consider the *governmental area.* In this area the scientist has the obligation to preserve the freedom of science. This implies a number of things.

First, there must be a reasonable freedom to investigate problems of one's choice. The scientist who is not free to undertake or reject a proposed research problem is a slave. His acceptance or rejection may be based on degree of interest, competence, or conscience. He must be allowed reasonable freedom to make a choice on any of these bases. I do not mean to imply that an employer—be he governmental, industrial, or institutional—has no right to establish research problems and ask his employees to work on them. I do want the employee to have reasonable freedom to change employment if his assignments are personally unsatisfactory.

Second, there must be freedom to make one's own decisions on conceptual matters. When the scientist is told which theories he must accept and which are to be rejected, then science is placed in a straight-jacket and progress cannot help but be stifled. We have seen this sort of thing happen too often in our own generation—in a Nazi Germany where the "Jewish physics" of Einstein and Planck was *verboten*; in Soviet Russia where by state compulsion biologists have been forced to abandon the genetically fruitful concepts of Mendel and Morgan to pursue the decreed genetics of Lysenko, and where the chemists have been ordered to abandon the resonance theories of Linus Pauling. Every true scientist has an obligation not only to science but also to society to protest long and vigorously against such science by decree, whether it be in a foreign country or his own. It has been gratifying to notice how our own scientists rose up en masse to protest Secretary of Commerce Weeks' interference with the operation and integrity of the Bureau of Standards in the AD-X2 battery additive case. It is likely that the administration will proceed with greater caution in the future as a result of learning the temper of America's scientific men.

Third is the obligation to insist on the free dissemination of new scientific findings. Progress in science has always been due to the sharing of new information. We can observe this for instance in chemistry. The study of the nature of matter was carried on for a millenium by the alchemists with little semblance of progress. The thought of the alchemists was dominated by the Aristotelian concept of the elements. This concept erroneously suggested the possibility of the transmutation of the elements (i.e., making gold out of lead). In following this will-o'-the-wisp the alchemists had excellent opportunities to gain an understanding of matter. Instead of transmitting their knowledge freely they disguised their discoveries in allegorical mysticism. This thousand-year period of chemical stagnation is in marked contrast with the past two centuries when chemistry has made remarkable strides. In part, the recent progress must be attributed to the discard of ancient philosophical concepts. However, the progress could not have been rapid without the development of a new outlook regarding the sharing of new discoveries. In contrast to vague symbolism there developed among natural philosophers in the seventeenth century a feeling of responsibility for the dissemination of new knowledge. The *Accade-*

mia del Cimento in Florence, the Royal Society in London, the *Académie des Sciences* in Paris all included among their activities the publication of new findings.

This tradition of early and complete publication must never be given up. If the world's scientists cannot benefit from the new discoveries of their colleagues science will begin to stagnate. Instead of working on the frontiers of knowledge scientists will spend their time duplicating one another's experiments.

These remarks naturally raise the problem of national security and scientific censorship. It can be argued with some reason that here is a place where censorship is justified. What shall be the proper attitude of the scientist? Clearly this must be an area for compromise. It is reasonable to withhold information that is of value to an enemy intent upon destroying one's way of life. But we must be careful not to stifle science by a rash policy of thoughtless censorship. The real problem lies in the decision of what to censor. Just what is valuable to one's enemy and what is useless? Not too much damage will be done to science if technological details are withheld, but if fundamental discoveries are withheld, one may handicap one's friends as much as one's enemies and do long-standing damage to science. Further, it is the fundamental discoveries which are hard to recognize since they often bear no apparent relationship to later developments. It would be easy to withhold the inconsequential and reveal the vital discoveries. The only real solution is to strive for international relations where censorship is unnecessary.

The difficulty is revealed in the activities leading up to the development of nuclear energy. All the fundamental discoveries in this field had been made by the time publication was brought to a close early in World War II. Without access to these findings no nation could have developed the atom bomb. With this knowledge, any nation willing to expend sufficient human and financial resources could have created the weapon. Now suppose that a group of pacifist scientists had anticipated the development of nuclear energy. Just where would they have instituted censorship in order to block effectively the development? Let us examine the sequence of discoveries.

The discovery of radioactivity was made in the laboratory of Henri Becquerel, a Frenchman, in 1896. He was extending into the area of mineralogy the fluorescence phenomena observed by Röntgen, the German who discovered x-rays just the year before. Röntgen's experiments were the outgrowth of developments made in cathode ray tubes, apparatus used in studying the conductance of electricity by gases. Such experiments had been pursued by the German Plücker from 1859 and extended by Hittorf and Crookes. Experiments with such tubes also resulted in the discovery of positive rays (or gaseous positive ions) by the Austrian Wien and of the electron by J. J. Thomson, an Englishman.

Each of these discoveries was fundamental to the work of the Manhattan Project. Which one would our pacifist scientists censor and who would accomplish the censorship?

Let us look further at the fundamental discoveries which followed. Marja Sklodowska Curie, a Polish graduate student, and Pierre Curie, her French husband, discovered the elements polonium and radium. Ernest Rutherford, a New Zealander working in Canada and later in England, unearthed a mass of information about radioactive elements and their decay in the first two decades of the present century. In 1919 he achieved the first successful transmutation—that of nitrogen into oxygen, discovered the proton, and set off a barrage of transmutation experiments in laboratories around the world.

Frederick Soddy of England introduced the isotope concept in 1911. Youthful Henry Moseley established the physical basis for atomic numbers before being sacrificed to military exigencies in the storming of Gallipoli in 1915.

In Germany Max Planck, studying heat radiation by a black body in 1900, abandoned classical ideas and advanced the quantum theory. Einstein bolstered the new theory in applying it to photoelectric effect and worked out the famous $E = mc^2$ formula for the equivalence of mass and energy at about the same time.

Niels Bohr, Danish student of Rutherford, in 1913 developed the theory of the structural atom, breaking sharply with classical theory to relate the spectral behavior of hydrogen to quantum considerations. The twenties saw further theoretical progress in the understanding of the atom through the concepts of the Englishman Dirac, the Frenchman de Broglie, and the Teutons Pauli, Schrödinger, and Heisenberg. The thirties saw the discovery of heavy hydrogen in America by Urey, the neutron in England by Chadwick, the positron in America by Anderson, artificial radioactivity in France by Madame Curie's daughter Irène and her husband Frédéric Joliot. They saw Lawrence develop the cyclotron at Berkeley. They saw Fermi of Italy bombard uranium with neutrons—Hahn and Strassman and Lise Meitner of Germany interpreting his experiments as the nuclear fission of U^{235}. Yes, the fundamental discoveries were in print by

1940 for the scientific world to read. Now where, may we ask, would the proponents of national security have stopped publication? One can see that science knows no national boundaries. Censorship by a single nation might safeguard for private use an occasional discovery, but it would not prevent the discovery from being soon made elsewhere. Only an international agreement among scientists could stop the flow of information. But what would be gained? The discoveries just enumerated led to the atom bomb with all its horrors and its fears and suspicions. But these discoveries also led to our understanding of modern chemistry and physics with their commercial applications. The availability of isotopes has led to the tagging of molecules which can then be followed through industrial processes or through metabolic processes. Through this the role of soil constituents in plant growth is better understood. The fate of fats, carbohydrates, proteins, salts, vitamins, hormones, and drugs is being revealed in the body through the use of tagged compounds. The radioactive atoms themselves offer hope of more effective treatment of devastating diseases. Atomic fission itself need not be an unmixed horror for it offers real possibilities as a useful energy source. No, we must not expect a momentary security if we stop publication of our discoveries. We do not have anywhere the omnipotence to decide when not to publish, where not to publish, what not to publish. Lacking this omnipotence, aren't we better off working in other directions to solve our problems without stifling future progress?

I turn next to what may be termed the *sociological area.* In the area of social relations the scientist is again in a position where he can make a contribution. Within his field of competence he has an obligation to work for the social good. I should like to discuss several specific instances that come to mind.

There is the problem of inter-group relationships. Anton J. Carlson, the renowned physiologist long at the University of Chicago has recently stated the case particularly well so I quote: "Factual, that is, scientific education on the nature of man seems necessary for the best future of man. Such education includes the scientific evidence of the unity of the human race. Despite so-called racial differences in such minor details as skin color, language, and religions, science has proved that the people now living on our earth are one species. This fact, understood and accepted by all sane citizens, should gradually eliminate racial prejudice, fear, and hate. It should promote cooperation in place of violence. Basic to the achievement of freedom from fear, want, and violence is the freedom to know."*

* *Science,* **117**, 701 (1953).

Then there is the conservation area. The scientist is in a good position to understand the cycles of nature and the effect of maladjustments in these cycles brought about by the improper use of resources. The proper functioning of nature requires a proper balance of animal and plant species, water, soil, and weather. Modern civilization has seriously strained this balance. We owe it to future generations to leave them a world where the strains have not been so severe that a decent civilization cannot continue.

Closely allied to the problem of resources is the problem of an optimum population. Within our own lifetimes, improvements in sanitation and medicine in the more advanced countries have brought about a significant increase in the life span. Coincident with this we can observe large areas of the world where the population pressures are such that whole populations are living in a state of chronic malnutrition. Shall the natural scientist close his eyes to this and say that it is the problem of the social scientist? Shall the social scientist insist that he must not be bothered since he is a pure scientist searching for natural laws? Or shall they join hands in grappling with the problem of the world's cultural unfortunates?

Speaking for myself, I feel that we must do the latter. Admittedly the problem is a sensitive one. Much can be accomplished by extending a knowledge of sanitation and agriculture as has been done in some countries by the Point 4 Program. However, the best efforts along medical lines and improvement of the food supply can come to nought if the populations of the undernourished areas continue to multiply like rabbits. Here we face religious and cultural taboos but they had better be faced honestly rather than trying to accomplish the impossible. As A. V. Hill recently said, "If they (representatives of religions) now claim that the facts and trends of overpopulation are not what we say, we can argue about that as a scientific question: but if they insist that its consequences should be left to God, they must allow us as citizens to take the opposite view."†

Finally, I would like to examine the scientists' obligations in the field of *education.*

First, there is an obligation to insist on sound scientific training for all. I do not mean that everyone should be trained as a scientist but that everyone should have an understanding of the problems of science and how such problems are attacked. There should be developed a faith in the unity of nature, a belief in the rational explanation of natural phenomena instead of a willingness to

† *Bull. Atomic Scientists,* **8**, 265 (1952).

accept supernatural answers. There should be an understanding of what is and what is not possible. There must be an understanding of the place of science in the modern world. We live in an age dominated by the discoveries of science. We must face the fact. The attainment of the bachelor's degree without a single credit in science, as was possible in my own university until a few years ago, is an intolerable situation in this day.

Second, there is an obligation to insist that the education of scientists, both pure and applied, be not so specialized that the trainee is incompetent to play a useful role outside his own narrow branch of training. There must be room in the science curriculum for a liberal sprinkling of such studies as will bring about the development of an educated man rather than merely the development of a competent technician. The scientist, in addition to being professionally competent, should be able to live a personally satisfying life and a socially useful life. A scientific education which fails to permit sufficient study of the humanities and the social studies is a fraud, just as a course stressing the humanities to the exclusion of the sciences is a fraud.

It seems to me that every curriculum in civil engineering ought to contain a generous sprinkling of courses in art, political science, and sociology—not just to provide breadth of education, but to add to professional competence. We should not be satisfied with the engineer who is capable of designing and building a durable bridge. We should also insist that he be able to build a bridge that is intrinsically beautiful and esthetically fitting to its environment. Further, it should be a socially integrated part of its community, meaning that it should be capable of fulfilling its role as a bridge without creating congestion, hazard, and inconvenience.

Finally, we should insist that education instill a love of truth, freedom, justice, and integrity that will stand as a bulwark against the anti-intellectualism which is rampant in this country today. We must build a people who demand fair play not only in athletics but in politics, in business, and in social relations. We must build a people who can recognize and expose quacks, faddists, self-seekers, and demagogues, not only in science but in other fields as well. But we must also be able to distinguish quackery from honest but different opinions. We must be willing to hear the honest minority.

In concluding, let me make the plea that we apply the findings of science to the elimination of the unpleasantness and drudgery of life, but let us use care that science remains our servant. It must not rob us of the opportunity to enjoy life to the fullest. There must remain in human life the opportunity for the expression of individual creativeness. If science ever suceeds in shoving the humanities into the background then man is headed for some such fate as Aldous Huxley pictured for him in his *Brave New World*. If, however, man can utilize the discoveries of science but still retain his individualism —and I use the word not in the sense of freedom to exploit his fellow men, but in the sense of living his own life and expressing his own creative talents —then we can look forward to the flowering of a civilization such as the world has never seen.

The Composition of Wines

MAYNARD A. AMERINE

The author is a graduate of and received his Ph.D. in plant physiology from the University of California. With the repeal of national prohibition he entered the field of enology—the study of wine. In addition to numerous reports on his researches, Professor Amerine has written several books. He is professor of enology at the University of California and is currently studying some biochemical aspects of wines in Spain and Germany on a Guggenheim grant.

WINE, a fermented beverage produced from grapes, is one of the oldest foods known to man. Every year nearly four billion gallons are produced. In all occidental countries it is a popular beverage, and in France, Italy, and Spain its production is a major factor in the national agricultural economy.

Many different types of wines are produced, depending on the variety of grapes, the climate of the region where the grapes are grown, the fermentation procedure, and the treatments given the wine during aging. But basically wines can be divided into two major categories, those to which no alcohol is added during or after fermentation, and those to which alcohol is added. The first group contains the table wines of 8 to 14 or 15 per cent alcohol, which, as their name implies, are intended for consumption with food. Wines of the second group, called dessert wines, are usually sweet, and a part of their alcohol (they usually contain 17 to 21 per cent) has been added to prevent fermentation of all the sugar.

Alcohol is not the only constituent of wine that has physiological interest. This review summarizes what scientists have learned about the composition of the common types of table and dessert wines. It deals with only the types commercially available in this country. Home-made and some minor foreign types fall outside the limits to be discussed here.

Sugars and Related Compounds

Grapes contain dextrose and levulose. During ripening the dextrose/levulose ratio decreases from 1.5 to about 1.0 or slightly less. Very little sucrose is found in European varieties of grapes—less than 1 per cent. During ripening the total sugar content increases rapidly; it should be between 20 and 23 per cent for table wines and 24 to 26 for dessert wines. In the eastern United States and the northern European countries grapes often fail to ripen adequately, and sucrose or invert sugar may be added. This is unnecessary in the Mediterranean countries and is illegal in California. In table wines practically all the sugar is fermented—usually dextrose slightly more rapidly than levulose—and normally less than 0.2 per cent remains in the finished product. There are two exceptions. A few sweet table wines are produced. These are sold as Sauternes (sauterne in California) or under other names. A second exception is the new, slightly sweet, red table wines now being prepared for the American market under various trade names. These contain about 1 per cent sugar. A third possible exception is the kosher-type wines that have become popular. They have the alcohol content of table wines (below 14 per cent), but are very sweet, containing up to 14 per cent sugar. They are really more of a dessert wine than a table wine. The reason for this is that sugars have a specific dampening effect on appetite. To drink sweet drinks before or during meals reduces appetite and thus interferes in that free selection of food which is the basis of a balanced diet. Dry table wines are thus the wine of choice with meals. Dessert wines contain more or less sugar depending on when the extra alcohol was added during fermentation.

Small amounts of pentose sugars, mainly arabinose and traces of xylose, are found in wines. Related compounds, such as rhamnose, pentosans, and methyl pentosans, and dihydroxymaleic acid are likewise reported. The pectin content of grapes is relatively small—less than 1 per cent—and this is reduced to negligible amounts in wines. Even so, the removal of pectins by means of added pectolytic enzymes often increases the clarity of new wines. Dextrins are reported in amounts of less than 0.1 per cent. Mannitol is found in some diseased wines.

Acids

Grapes contain relatively large amounts of tartaric and malic acids and a small amount of citric and tannic acids. Some varieties have a much higher acidity than others, even at the same stage of maturity. Grapes grown in warm regions always contain less acidity, at least of malic acid, than those grown in cold regions. The acids are of great importance in the fermentation, aging, and taste of the wine. Tartaric is considerably stronger than

malic acid. Table wines usually contain 0.5 to 1.0 per cent titratable acidity calculated as tartaric, while dessert wines contain 0.3 to 0.6 per cent. From 10 to 40 per cent of the acidity is due to malic acid. Only 0.01 to 0.05 per cent citric acid is found in wine.

Although the relationship is not direct, owing to the very good buffer capacity of musts and wines, high acid wines usually have a low pH. At the lower pH the fermentation is cleaner, color and flavor extraction are better, and clearer wines of better stability are produced. Wines of lower pH are also more resistant to bacterial attack; they have a brighter color and a fresher taste. The pH of table wines is usually from 3.0 to 3.5 and of dessert wines from 3.5 to 4.0.

During fermentation, succinic, lactic, acetic, and formic acids are produced, while the tartrate and malate contents decrease. The tartrate content decreases because of the insolubility of potassium acid tartrate in alcohol, while the malates are partially fermented. During aging, the malate may be attacked by certain microorganisms to produce lactic acid and carbon dioxide. Sparkling wines, of course, contain relatively large amounts of carbonic acid. Succinic acid is present in amounts of 0.05 to 0.20 per cent in table wines, depending on the amount of fermentation. Lactic acid varies from 0.03 to 0.40 per cent or more, depending on the amount formed in fermentation and the extent of the malo-lactic fermentation.

While only small amounts of acetic acid are formed during alcoholic fermentation—usually less than 0.02 per cent—various bacteria will produce much larger amounts during storage, particularly if the casks are not kept completely full. Spoilage actually appears to be due to ethyl acetate rather than to acetic acid, but the acetic acid content is usually taken as a measure of the spoilage. The present American limit for white table and dessert wines is only 0.120 g. per 100 ml., and 0.140 for red table.

Formic acid is found in amounts of about 50 mg. per liter in sound wines and in higher quantities in spoiled wines. Butyric acid is reported in variable amounts, but the best results indicate only about 10 to 20 mg. per liter in sound wine. Glyoxylic acid is found in sound grapes and gluconic acid is reported in grapes attacked by certain molds.

To obtain the proper coolness for aging, the wine is often stored in caverns. The casks in the foreground are more than man sized. (Courtesy of Wine Institute.)

Alcohols

The most important alcohol in wines, as in beer and whiskey, is ethyl. Wines contain from 8 to 24 per cent alcohol depending on the degree of maturity of the grapes, the amount of sugar added before fermentation, the amount of alcohol added during or after fermentation, and the losses occurring during fermentation and aging. Occasionally increases occur during aging when the container is stored under conditions of very low humidity. The factor for alcohol yield (by volume) from sugar according to the Gay-Lussac equation should be about 0.64, but Pasteur obtained only 0.611, and many studies report only 0.50 to 0.55. The intoxicating property of wines is due entirely to ethyl alcohol.

Very little methyl alcohol is found in grape wines. It is not produced by alcoholic fermentation or from glycine. Its sole source appears to be in the hydrolysis of pectins. Usually less than 0.15 per cent can be found in wines.

Likewise, only small amounts of the higher alcohols are found, but because of their pronounced odor they contribute to the quality of brandy and probably of wines. The main constituents of the higher alcohols are isoamyl, *n*-propyl, isobutyl, active amyl, *n*-butyl, and *sec*-butyl. Probably present also are *n*-hexyl, *n*-heptyl, and *sec*-nonyl. About 10 to 175 mg. per 100 ml. of wine have been reported, but the average is below 100 and in the better studies below 50. The higher alcohols are derived from deamination of amino acids, though it is reported that more higher alcohols are formed than can be accounted for by the decrease in amino acids. This may be because yeast proteins are decomposed. Contrary to popular belief, the higher alcohols are present in too small amounts to have a significant physiological effect.

A typical scene in the coastal wine section of California, showing the rolling hills, the well-groomed vineyard, and the patches of oak trees in the distance. The cultivation of these steep hills must be done by horses, and the ripe grapes are brought down to the winery on sleds. (Courtesy of Wine Institute.)

Glycerin, a polyhydric alcohol, is found in amounts of 0.5 to 1.5 per cent in wines. While it is the second most important product of fermentation, its exact genesis is not known, though it appears along with pyruvic acid during fermentation. Glycerin seems to impart smoothness to wines and may ameliorate the burning taste of alcohol. The amount of glycerin produced decreases at high fermentation temperatures, but contradictory results are reported in the literature. It has been questioned whether the low carbon dioxide tension in laboratory fermentations was not a cause of the low glycerin production. Glycerin production is higher when sulfur dioxide is present, but acetaldehyde retention is greater. The alcohol/glycerin ratio also varies considerably, depending on the yeast employed, the temperature of fermentation, etc.

A closely related compound, 2,3-butylene glycol, is also always produced during fermentation, but it is odorless and has little organoleptic importance. More is produced in commercial than in laboratory fermentations. From 0.01 to 0.15 per cent has been reported. The glycerin/2,3-butylene glycol ratio varies from about 4 to 15. Acetylmethylcarbinol is produced in amounts of only 1 to 30 mg. per liter in sound wines. Wine vinegar contains much higher amounts. Diacetyl has a pronounced odor and may be a minor factor in the quality of some red wines which contain 2 to 4 mg. per liter. Diacetyl can be detected by the olfactory nerve at about this concentration.

Aldehydes and Related Compounds

Acetaldehyde, a significant intermediary in alcoholic fermentation, is the most important aldehyde present in wine. Its accumulation is strongly inimical to the quality of table wines, but its presence in appreciable amounts in sherry is normal. Amounts of over 100 mg. per liter usually give table wines a vapid or oxidized taste, but 200 to 300 mg. per liter are not uncommon in some sherries. Traces of formaldehyde, propionaldehyde, cinnamaldehyde, oenanthaldehyde, vanillin, acetone, and methyl ethyl ketone are also reported.

Acetal is present in table wines in small amounts, 0 to only 6 or 7 mg. per liter. But in dessert wines up to 30 or 40 mg. per liter are reported. An accurate and simple procedure for acetal determination remains to be developed. It has a rather pleasant odor.

Hydroxymethylfurfural, a dehydration product of levulose, is formed in sweet wines that have been heated. Thus, madeira wines and California sherries, both of which are produced by heating sweet wines, contain appreciable amounts—30 to 300 mg. per liter. Wines made with grape concentrate contain hydroxymethylfurfural. Thus, a positive test for this substance in a wine indicates that it has been heated or that concentrate has been added to sweeten it. Its odor, however, is not unpleasant, and it is probably a desirable factor in the olfaction of some heated wines. Furfural certainly occurs in

brandy and probably in wines, at least in dessert wines. A less common constituent is acrolein. This is a product of bacterial spoilage of glycerin. In some cases it appears to react with tannins to give a bitter taste to red wines.

Esters

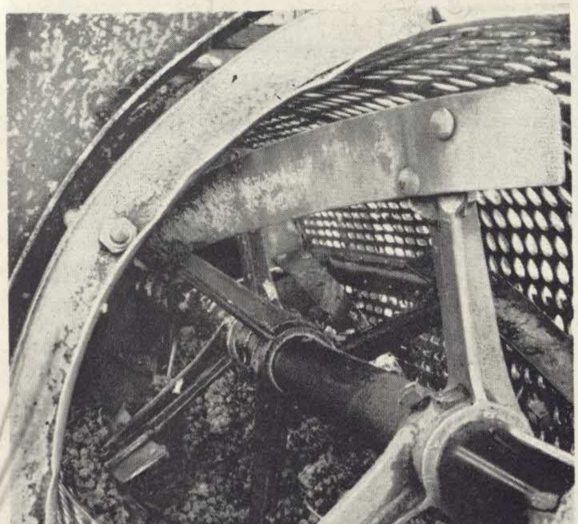
The importance of esters to the quality of wines has been greatly overemphasized in the literature. Actually, the only ester present in amounts large enough to influence the odor markedly appears to be ethyl acetate, and this has an objectionable acescent odor if over 150 mg. per liter are present.

There are reports from Russia that carbonic acid esters are important in the formation of the sparkling wine odor in sparkling wines. These studies have not been confirmed here and have been questioned there.

Esters of the acids previously reported, and valerianic, capronic, caprylic, caprinic, and pelargonic acids also occur, and possibly those of isobutyric, isovaleric, and oenanthic as well.

Polyhydroxyphenols

Tannins and coloring matter are the primary heterocyclic polyphenolic compounds present in wines. The tannins of wines are about equally *l*-gallocatechin and *d*-catechin. In white wines only



A mechanical crusher delicately separates the stems from the fresh grapes without breaking the seeds. After going through the crusher, the grapes and their juice, called "must," are pumped into large vats, where they are left to ferment. (Courtesy of Wine Institute.)

about 0.01 per cent tannin material is found, but in red wines it may mount to 0.3 per cent. The tannins have a bitter taste, and one purpose of the aging period for red wines is to reduce the tannin content. Tannins have some antiseptic value, act as antioxidants, and form precipitates with aldehydes.

The color of red wines is due mainly to the monoglucoside oenin, but petunidin, delphinidin, and the diglucoside of oenin are also reported in *Vitis vinifera* grapes, and other anthocyanins are found in other species. Oenidin appears to be present in almost equal amounts with oenin. By heating and aging the anthocyanins are partially demethoxylated.

The color of red wines is very important to their quality, and much experimentation has been performed to extract the maximum color from the skins, to protect the color during aging, and to provide artificial colors. The anthocyanins act as acid-base indicators, and the red color is sharper and less blue at the lower pH's.

In white wines the flavonol pigments quercitrin and isoquercitrin have been reported.

Nitrogen

The total nitrogen content of wines is likewise very variable, varying from about 300 to 1200 mg. per liter. The ammonia fraction, 0 to 200 mg., is important as a specific catalyst in alcoholic fermentation. It is reduced to a negligible value during fermentation. The amino acids so far reported include alanine, arginine, asparatic acid, cystine, glutamic acid, glycine, histidine, isoleucine, leucine, lysine, methionine, proline, serine, threonine, tryptophane, tyrosine, valine, and phenylalanine. Glutamic acid was reported in amounts as high as 150 mg. per 100 ml., which seems high, as does the value of arginine of 110 mg. per 100 ml. Amino acids originate from grapes and yeast autolysis.

The presence of excessive amounts of nitrogenous substances, particularly of the amino acids, is favorable to spoilage of wines. Only small amounts of protein have been reported and these are precipitated by the alcohol. In certain seasons enough protein is present in the new wine to cause cloudiness when the temperature of the wine is raised.

Enzymes and Vitamins

Various enymes are present in wines in addition to those associated with the yeast cell and alcoholic fermentation. Polyphenol oxidase, catalase, esterase, invertase, and pectolytic and proteolytic enzymes are the most common. Oxidizing enzymes of the peroxidase type are also present. Polyphenol oxidase is, however, possibly the most important, as it causes the oxidation of polyphenols, which is the main reaction when grape juices exposed to the air

darken in color. Grapes attacked by various molds contain higher amounts than sound grapes.

Ascorbic acid is present in small amounts in fresh grapes (1 to 18 mg. per 100 ml.), where it is an important factor in the reducing system of the must; only negligible amounts are found in wines.

Vitamin A is likewise present in negligible amounts. But thiamin is found in amounts of 0 to 50 μg. per 100 g. of wine. Riboflavin occurs in amounts of 5 to 120 μg. per 100 g. About 70 μg. of pyridoxin, 65–120 μg. of nicotinic acid, and 70–140 μg. of pantothenic acid are also reported. In addition, *p*-aminobenzoic acid, biotin, and inositol occur, the latter in appreciable amounts.

Aroma

Most important to the quality of wines is their odor. A few varieties of grapes, muscats for example, give a characteristic odor to the wine, and this is probably due to some specific compound or group of compounds. The Concord-flavor of some varieties of grapes grown in the eastern United States is apparently due primarily to methyl anthranilate.

The winelike odor of wine is due mainly to lauric acid, even though only a few parts per million are present. The roselike odor of certain wines has been attributed to *p*-hydroxyphenylethyl alcohol, which may be derived from the amino acid phenylamine. A terpene, α-terpineol, also occurs.

Actually, the odor of wines is an extremely complex blending of a great many compounds, none of which is present in large amounts. In addition, the volatility of ethyl alcohol and the solubility in it of the various odorous constituents probably markedly influence the odor. The acidity, temperature, and amount present are also important factors. It should be noted that the odor of many of the substances mentioned is very different at low concentrations than at high.

Inorganic Components

The ash content of wines varies from 1.1 to 5.2 g. per 100 ml., averaging about 2.5. The most important anions found are chloride, phosphate, silicate and sulfate. The phosphate, of course, is a significant factor in fermentation. The main cations are calcium, magnesium, potassium and sodium but traces of many other elements can be detected. As little as 1 mg. per liter of copper may influence the clarity of a white wine. The potassium content is far greater than that of the other minerals.

The various steps in champagne corking are shown in this picture from the early 1870's. In the shed were carried on the processes of disgorging, perfecting the fill, and recorking. The finished champagne, corked and labeled was packed in the handwoven baskets, ready for market. (Courtesy of Wine Institute.)

The New Orleans Academy of Sciences: Its First Hundred Years (1853-1953)

KARLEM RIESS

A native of New Orleans, Karlem Riess completed work for his B.S. and M.S. at Tulane University, and received his Ph.D. from Brown University in 1943. At present he is associate professor of physics at Tulane University. He has been president of the Tulane chapters of Phi Beta Kappa and Sigma Xi, and is now president of the New Orleans Academy of Sciences.

THE Old South in the period just before the Civil War has been presented by most historians as an agricultural society, with its accompanying system of slavery. Industry was practically nonexistent. Science and technology were insufficiently organized to exert a significant effect on the social pattern. There were, however, a large number of individuals genuinely interested in science. They were scattered throughout all the southern states and made substantial contributions to knowledge through their writings, experiments, and lectures.

The citizens of New Orleans, largest and most cosmopolitan city in the South during this period, were particularly interested in all types of science. A Physico-Medical Society was organized as early as 1820, and in 1839 the Louisiana Medico-Chirurgical Society was founded. These two, primarily for physicians, did admit as "assessors" or associate members persons scientifically inclined but without a medical degree. Assessors of the Physico-Medical Society were drawn from all professions, but those of the Medico-Chirurgical Society were limited to allied professions such as chemistry or pharmacology. Traveling lecturers, men of such caliber as Benjamin Silliman and Louis Agassiz, attracted large, enthusiastic audiences. One of the contemporary journals reported that there could be no complaint

> . . . that the money-making, money-spending, amusement-seeking citizens of New Orleans do not appreciate him [Agassiz] seeing that among his auditors are not only hundreds of learned lawyers, physicians and teachers, but hundreds of merchants and still other hundreds of the fair sex.

Benjamin Silliman, Jr.'s New Orleans lectures (1845) on agricultural chemistry are believed to have been the first of their kind given in the United States.

In 1834 a group of young, alert physicians organized the Medical College of Louisiana. The college was merged with the University of Louisiana in 1847, and is now the School of Medicine of Tulane University. The philosophy of the early New Orleans scientists was stated by Dr. Thomas Hunt, founder and first dean of the Medical College of Louisiana, in his initial lecture (1835):

> There is no mystery about science. Truth is simple and reveals her doctrines in a language intelligible to every mind. She affects no air of pedantry, and decrees it inconsistent with her vocation to annoy and perplex the learner with far-fetched and uncouth terms. To my mind, there is a moral sublimity in the picture of a learned and philanthropic man, conveying the lessons of wisdom and experience, in the beautiful language of simplicity, to an acquiring and intelligent student.

These physicians were not merely men of medicine; they possessed wide scientific interests in unrelated fields such as botany and geology. They met almost daily and discussed their work and reading. Thus it is not surprising that among these physicians were several charter members of the American Medical Association and of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, and, in 1849, leaders in the formation of a Louisiana State Medical Society.

It was in such a scientific environment that the New Orleans Academy of Sciences was formed. On the twenty-first of March, 1853, five New Orleans physicians—Doctors Bennet Dowler, Noah B. Benedict, Howard Smith, Joseph S. Copes, and Henry D. Baldwin—met at the home of Dr. Copes, "to

confer upon the expediency of some kind of organization with a view to mutual improvement in medical and natural sciences." With Dr. Dowler in the chair, and Dr. Copes as secretary, they outlined a Constitution and By-Laws for the incorporation of a society to be devoted to the cultivation of medicine and natural sciences, with the power to establish lectureships and professorships. The five physicians agreed that their number should be expanded immediately and invited eight men to their second meeting. After five preliminary meetings, each time adding associates, formal organization was completed on April 25, 1853, with the adoption of the Constitution and By-Laws. Twenty-seven men were listed as founders, including thirteen physicians, two pharmacists, three university professors, three engineers, three ministers, one lawyer, one high school principal, and one chemist. Of the twenty-seven men, at least two were nationally known in their fields, Dr. Bennet Dowler (physiology) and Dr. John Leonard Riddell (chemistry, botany, and medicine). In subsequent years at least ten more of these men achieved noteworthy success. The Academy was not formally incorporated until 1856. Curiously, it was not learned until 1936 that the Act of Incorporation was not attested to by the Attorney General of Louisiana, a requirement in 1856. Thus for eighty years the Academy was not a legal corporation. This was corrected by reincorporation in 1939.

Membership was limited to persons who had given proof of their devotion to science by the production of a paper on a scientific subject. Election was by unanimous vote of the members, using a white- and black-ball system. The wording of these provisions resembles those in the constitutions of our social fraternities and Masonic organizations. The Constitution and By-Laws passed through many inevitable revisions, but the unanimous vote provision remained until the affiliation of the Academy with the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE in 1919.

The original Constitution termed the active members "fellows" and provided for the election of distinguished scientists as honorary members. A third class of membership, corresponding members, was soon added. In the first two decades of the Academy many corresponding members, but only six honorary members, were elected. The honorary members were Joseph Henry, Louis Agassiz, A. D. Bache, Baron von Humboldt, Lt. Matthew F. Maury, and Dr. E. K. Kane.

One of the first acts of the new society was to correspond with Joseph Henry, then secretary of the Smithsonian Institution. Henry expressed personal sympathy and promised "the hearty cooperation of the Institution in any project" for the promotion of natural history in New Orleans.

The first scientific session was held on March 28, 1853—a discussion based on specimens of cypress and iron brought to the meeting by Dr. Dowler from excavations in the suburbs of the city. The first two formal papers were presented on April 11, 1853. One was by Dr. Josiah Hale on the history of botany and classification schemes, the other by Dr. Bennet Dowler on the natural history of the mosquito. The Academy was organized into ten scientific sections: (1) Natural History of Animals; (2) Botany; (3) Geology and Mineralogy; (4) Chemistry and Natural Philosophy; (5) Astronomy and Mathematics; (6) Antiquarian Researches and Ethnology; (7) History and Biography; (8) Medicine and Physiology; (9) Geography, Statistics, and Political Economy; (10) Psychology and Aesthetics. The fellows of the Academy were permitted to affiliate with as many sections as they were interested in. Chairmen of the sections were chosen by ballot.

The early meetings of the New Orleans Academy must have been lively and stimulating sessions. The minutes contain references to many excellent papers and record pages of discussion of these papers. Unfortunately very few of the manuscripts exist today. By agreement, politics and religion were excluded from all discussions. Weekly meetings were held from September until July. Recess during the summer months was prompted by the prevalence of yellow fever.

The library and museum of the Academy were important adjuncts and developed into extensive collections. At each meeting donations of books and specimens were received and listed in the minutes. These donations came from many non-members as well as from the members themselves. The ravages of occupation troops during the Civil War and many moves from one location to another resulted in the loss of many of the original documents, books, and specimens.

The Academy accomplished a number of interesting things during its early years. Academy members, in cooperation with the Common Council of the City of New Orleans, supervised the drilling of an artesian well in the center of the business district of the city, making analyses of the borings and the water each day. The Academy urged the Legislature of the State of Louisiana to make a complete scientific survey of the State, with the Academy as director. The Academy took the initiative in arousing public interest for proper protection of the City of New Orleans against flood waters of the

Mississippi River by advocating a system of protection levees and drainage canals. Academy members kept accurate daily meteorological records for many years. And not the least important contributions were the individual researches of the members, as indicated by papers presented before the Academy and later published in national and local journals.

In 1860, by action of the Legislature, the Academy was affiliated with the University of Louisiana as its Department of Natural Sciences. The Academy wanted this affiliation so that permanent meeting rooms and museum space in the University buildings would be available. In return for these privileges the members of the Academy gave courses of lectures and allowed the students and faculty to use the library and museum. This arrangement did not prove to be a happy one. The Academy was well satisfied, but the University authorities resented the intruders. The feuding continued for about fifteen years. On several occasions attempts were made to evict the Academy. In 1880 the library and all collections were moved into a corridor and the rooms declared vacated. This finally forced the Academy to move to other quarters.

The Civil War had its effect on Academy activities as on all activities in the South. Meetings were suspended from 1861 to 1866, except for a few scattered gatherings. But, as one of the local historians wrote,

> . . . the Academy, not without much struggling . . . soon reestablished itself in its efforts and regained its former position without any outside aid whatever.

In the post-Civil War period the number of scientific sections was increased to twenty-one, essentially a section for each subdivision of the principal sciences. The Academy offered its first research grants in 1872—prizes for the best collections and descriptions of the entomology of Louisiana. These were given wide publicity, and evoked much public interest. The status of the Academy in this period (1872) is best summarized by its president, Dr. Joseph S. Copes, who wrote of the

> . . . continued and vigorous existence [of the Academy] notwithstanding the many adversities it has met; and upon the straightforward integrity it has always maintained in the face either of bitter and inflated enmity or of designing and spurious friendship.

Its library and museum

> . . . also created a taste and predilection for scientific studies and pursuits in our midst, and has bestowed encouragement and assistance upon those who have been disposed to undertake the study of nature or to collect for examination the rarer specimens of her handiwork.

The Louisiana Legislature of 1871 appropriated five thousand dollars to the Academy for explorations of the geological, agricultural, and hydrodynamic conditions and resources of the state. This grant was vetoed by the governor of the state, for the reason that the Academy was not under state control, noting that state funds could only be allotted to organizations under its control.

During the 1870's John Tyndall was elected an honorary member and many foreign scientists were added to the list of corresponding members. In this period the first women became associated with the Academy as corresponding members. No women were admitted as active members until 1912.

The second important era in Academy history was from 1885 to 1890. The membership was substantially increased during these years, and many notable papers were presented and published. Meetings were held monthly instead of weekly. The members in 1885 proved to be a very discerning group. They revised the Constitution and By-Laws to provide for election of members distinguished in the arts and literature as well as those primarily interested in science. The newly established Tulane University, which had been formed from the University of Louisiana in 1884, had an exceptional faculty, headed by William Preston Johnston as president. Johnston, Florian Cajori, Brown Ayres, Alcee Fortier, R. H. Jesse, J. M. Ordway, Robert Sharp, Hanno Deiler, Dr. Joseph Jones, Dr. C. E. Kells, Dr. Stanford Chaille, and others became Academy members. The Academy and its library returned to the University buildings. The museum was abandoned and all remaining specimens were given to the museum of Tulane University. The number of scientific sections was reduced to three: Section A, mathematics, physics, astronomy, geodesy, and mechanical science; Section B, chemistry, mineralogy, geology, geography, archaeology, biology, histology, and microscopy; Section C, linguistics, history, political science, metaphysics, philosophy, anthropology, ethnology, economic science, and statistics. The absence of medicine and botany will be noted, particularly since these were prominent in the original Academy.

There was a lapse of interest in the Academy from about 1895 to 1912. The reasons for this are not known. There is no record of much organized activity, although occasional meetings were held. After a reorganization in 1912, monthly meetings were held with regularity until 1917. Many original scientific papers were presented. Throughout the years of World War I meetings were infrequent. The library was maintained on the Tulane University campus and expanded annually. The meetings

during this period tended to be more popular in character, covering such topics as the cattle tick and its control in Louisiana, hay fever control and prevention, control and eradication of the cottony cushion scale, and control of the Argentine ant in New Orleans. The Academy sponsored public control campaigns against the cottony cushion scale, camphor tree scale, and Argentine ant, with noteworthy success. The cooperation of the local newspapers and of municipal and state agencies was particularly good in these instances, with results of great benefit to New Orleans. Similar campaigns against the cattle tick and against weeds associated with hay fever were not as successful.

The Academy was affiliated with the American Association for the Advancement of Science in 1919, and has participated in the affairs of that group through its representatives at the Academy conferences and at the annual meetings of the AAAS. The affiliation proved to be a turning point in the life of the Academy, a turn in the wrong direction because many of the members stopped paying dues when the AAAS contributions to the Academy were received.

There was another period of decreased activity and a consequent drop in membership between 1920 and 1930. This resulted in a formal reorganization in 1933. A new Constitution and By-Laws were adopted, changing the financial basis of operation of the Academy by reducing the dues and initiation fees. The membership was increased and was quintupled within a few years. Monthly lecture meetings were resumed, many with visiting speakers brought to New Orleans for the occasion. The Academy also sponsored a series of radio programs.

There was also a general change in the perspective of the Academy. The organization matured, as Dr. E. C. Faust stated, from one

> . . . jealous of the activities of younger scientific groups in the City of New Orleans to the more logical position of coordinator and cosponsor in scientific activities and projects. Thus it has succeeded in bringing together scientific men in federal, state, institutional and private capacities, without overdominating the individual research groups.

In 1920 Dr. Reginald S. Cocks, Secretary of the Academy and Professor of Botany at Tulane University, suggested that the Academy sponsor the establishment of an arboretum and botanical garden in one of the parks of New Orleans. The suggestion furnished a lively issue for almost twenty years. The many committees which considered the suggestion were faced with apathetic public officials, insufficient funds, and a marked hostility on the part of the officials because an outside organization was attempting to aid in the administration of a part of the municipal property under their jurisdiction. The plan finally crystallized into a proposal to establish an arboretum and botanical garden, primarily containing tropical and subtropical plants, in the new addition to the New Orleans City Park, an undeveloped section (at that time), with the Academy as one of the sponsors. A booklet emphasizing the need for the arboretum and botanical garden was published by the Academy. The material result of all the discussions was the dedication of a tract in City Park for the project, and subsequent building of roads, lagoons, and bridges, and filling, grading and planting of the area with over four thousand shade trees. The botanical garden was not established, since, as one of the committees stated,

> . . . botanical gardens are the product of a certain stage of social development which has not quite been reached here.

The arboretum was not in the form desired by the Academy nor was the Academy permitted to be a legal sponsor. The Academy served principally as a coordinator between state and municipal officials and the interested civic groups, such as the Southern Forest Experiment Station, Tulane University, and the various garden clubs.

A beneficial result of these arboretum discussions was the drawing up of a new charter and Act of Incorporation for the Academy. The legal procedure involved in revising the Constitution and charter occupied many business meetings, but the results have proved very beneficial to the Academy.

One feature of the revised Constitution is worthy of note. It restricts the election of honorary members to those active members who have served the Academy in a distinguished, unselfish fashion. Since the abolition of the class of corresponding members, the active membership in the Academy has been open to scientific men and women from all states, and associate membership to nonscientists.

One of the important activities begun during this period was the Junior Academy of Sciences. The original proposal was for a junior section of the Academy, with membership composed of high school, normal school, and college students. However, the intricacies of internal politics produced a different sort of Junior Academy, a section composed of science teachers in the New Orleans high schools as representatives of individual science clubs within the schools. The initial response on the part of the science teachers was far from satisfactory. This rather peculiar type of Junior Academy sponsored annually a single demonstration lecture for high school students and a seminar for teachers, but

that was all. The late Dr. Otis W. Caldwell, on a visit to the New Orleans Academy in 1936, was amazed at this situation, and indicated it was the only Junior Academy in the United States without any junior members. Through the persistent efforts of one of the energetic officers of the Academy the situation was changed, and a Junior Academy composed only of high school students was formed. The sponsorship of the juniors is one of the major functions of the New Orleans Academy today. A member of the senior Academy serves as advisor and aids in the planning of programs, but the officers and committee members are all high school students. An exhibit of projects and a program of papers by the juniors is a feature of each year.

The first research grant jointly sponsored by the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE and the New Orleans Academy was awarded in 1936 to Dr. Howard R. Mahorner for a study of the experimental production and the causes of goiter. Similar grants have been awarded annually since that time, resulting in the publication of some important papers. The recipients represent all fields of science.

In recent years the Academy has held monthly or semimonthly meetings, with an annual meeting in the spring of the year. The monthly meetings are public lectures, discussions, or laboratory field trips, usually of a semipopular nature. The annual meetings and special symposia are devoted to more serious and more technical subjects. The Academy has been fortunate to be able to arrange frequent joint meetings with many of the other scientific societies in New Orleans. On three occasions annual meetings have been held in conjunction with the Louisiana Academy of Sciences. Some of the annual meetings have been divided into sections, with a series of short papers and one feature paper at each session. On other occasions a single session has been held, with a symposium or set of featured speakers following the Academy's annual dinner.

One of the annual programs is the presentation of research papers by graduate and undergraduate students from the universities in the vicinity. These are always stimulating meetings for the regular members as well as for the many students who attend. Recent civic projects include active leadership in the antirabies and rat eradication campaigns in New Orleans.

The Academy has not maintained a very active publication program. The first *Proceedings* of the Academy were issued in 1854, but no subsequent issues appeared. Printed Constitutions and By-Laws appeared in 1854, 1859, 1871, 1885, 1913, and 1919. Since that time the Constitutions have been mimeographed for distribution. Abstracts of the programs presented at the annual meetings have been published, as well as programs for the meetings themselves.

The library of the Academy has been deposited with the Howard-Tilton Memorial Library of Tulane University. The volumes are cataloged separately and are on separate shelves. Exchanges with foreign and domestic academies and other scientific bodies are maintained, with the librarian of the Howard-Tilton Memorial Library serving as curator (librarian) of the Academy. The archives, including the original minute books and similar items, are also deposited in the Howard-Tilton Memorial Library.

There are some who believe that in a modern metropolitan society there is no place for an Academy of Sciences, and who feel that the New Orleans Academy of Sciences and all similar organizations have outlived their usefulness. Yet, as we look back over the first century of its activity, it is apparent that the New Orleans Academy has fulfilled the aims of its founders and is continuing to do so. It has emerged as the sponsor of an extremely live and successful Junior Academy. It stimulates research through its scientific programs and research grants and it serves as a scientific coordinating agency in New Orleans. Thus we start our second century with faith in our Academy and hope for the future.

Archaeological Occurrences of Pronghorn Antelope, Bison, and Horse in the Columbia Plateau*

DOUGLAS OSBORNE

Dr. Osborne is Curator of Anthropology, Washington State Museum, and Assistant Professor, Department of Anthropology, University of Washington. He received his Ph.D. from the University of California in 1951. Before his affiliation with The University of Washington, he was Acting Field Director of the Smithsonian Institution's River Basin Surveys, Pacific Coast Area.

SINCE 1947 there have been extensive but often unprogrammed archaeological excavations, testings, and surveys in the area known to physiographers as the Columbia Intermontane Province[1] and to anthropologists simply as the Plateau.[2] It is probably safe to say that nearly all this work has been either stimulated by or supported by the federal government's projected dams on the Columbia or on its tributaries. The famous Grand Coulee Dam and Reservoir, built by the Bureau of Reclamation, Department of the Interior, was the first of many that have been completed or are in construction or in planning stages by the Bureau or by the U.S. Army Corps of Engineers. Realizations of regional needs in the categories of flood control, electric power, and irrigation potentialities come nearer as each dam is completed.

It was recognized that there was a federal responsibility for the salvage of the scientific and cultural values that would be covered or obscured by the large reservoirs behind the dams. To the Smithsonian Institution was entrusted the responsibility for the organization of facilities and personnel capable of studying and publishing on archaeological, historical, and paleontological sites which were within the flood lines of the reservoir areas. Appropriations for the work have never been sufficient, but excellent administration of the funds and close cooperation between the National Park Service, the Smithsonian Institution, and local institutions has resulted in the amassing of a large amount of archaeological data. Much of this information has not yet been assimilated.

Thus far, most of our archaeological excavation has been in the McNary Reservoir, on the Columbia, from Umatilla, Oregon, to above Pasco, Washington, and in the Chief Joseph Reservoir in Southern Okanogan County, Washington.[3] A site near Trinidad (Potholes site) supplies the only data between the southeast central (McNary) and the northeast central (Chief Joseph Reservoir plus a cave site, 45-GR-2, near Grand Coulee) concentrations of activity. A large share of the work done in both of the reservoirs was either superintended or directed by the writer, who was on the Smithsonian staff during the periods when these sites were investigated.

A by no means unimportant aspect of the study of a defunct culture by a modern archaeologist is that of the economy, the method of gaining a livelihood that was practiced by the inhabitants. Such a study often becomes largely ecologic. Plant and animal life of an area, at various time periods, becomes of direct interest to the archaeologist's generally culturologic studies. Hence, all remains of plants or animals found in archaeological deposits are saved and data concerning them are recorded. The archaeologist of course takes these remains to a specialist for identification. The bones in the

* Permission to use herein the data derived from Smithsonian Institution River Basin Surveys fieldwork has been granted by Dr. Frank H. H. Roberts, Jr., Director, River Basin Surveys.

following lists have been largely fragmented by the Indians for the marrow.

Status of the Problem

This paper was prompted primarily by two references: Tipper *et al.*,[4] and Dalquest.[5] The first of these describes a bison (*Bison bison bison*) skull found in Whitman County, Washington; it mentions a few of the finds of fossil bison in the region and then, in a short discussion of the range of the animal, makes the incorrect statement that bison have not been reported in Washington in historic times. Dalquest mentions this mammal as a probable casual within the state. Haines[6] and Kingston[7] have both covered the question of the buffalo in the Pacific Northwest thoroughly. The paper of the latter probably contains nearly all the references to the buffalo that appear in early writings. Both authors find evidence of bison in eastern Washington in historic times.

Dalquest[5] discusses briefly the possibility that the antelope (*Antilocapra americana*) was an early native of the plains of eastern Washington. He concludes, in effect, that the animal should have existed there but states that he is unable to offer the necessary documentary or osteological evidences to clinch the point. Einarsen[8] indicates that the southern half of eastern Washington was occupied by the pronghorn at the "greatest extent of range." He does not, however, document this statement, and I am in no position to judge the validity of its foundation. It is my belief that recent archaeology has supplied information that will be of interest to all persons concerned with the above questions. Data on horse finds in Indian sites have been included as an addition to the body of dated information on the spread of that animal from the American Southwest.[9]

Roe's excellent monograph[10] on the buffalo gives a most cursory treatment of that mammal's range in this area. To his work must be added the compilations of Haines[6] and Kingston,[7] the ethnographic material from California,[11] and my data.

The Finds*

Raw data on the bones of the three mammals of interest here are given below, site by site. Distributions are plotted on Fig. 1. No attempt is made to show relative quantity on this map. It is doubtful if our information is sufficient to support any statements that rest upon quantitative or percentile comparisons.

45-BN-3.† This was the first site excavated by the Smithsonian Institution, River Basin Surveys, on the Columbia. It is in Umatilla tribal country, on the north bank of the Columbia about five miles above the Umatilla confluence. The site was important; over fifty burials were removed and large areas of moderately good midden were investigated. The dig was 50 by 150 feet. By far the greater area of the site was occupied, and a large number of the burials were made, in historic times. Trade goods, glass beads, copper ornaments, and scraps of iron were found in the midden and in the graves. Horse bones were not rare. The offerings of traders via native trade channels were commonplace in the area long before posts were established anywhere in the Columbia valley. Lewis and Clark certainly did not record the site.[12] With these data in mind, the site has been placed in the middle and late eighteenth century.

Antelope: no bones positively identified as antelope were found. Three bones, all somewhat the worse for wear, were labeled "deer or antelope" by Dr. White, who handled the material from this site: 1 radius, 1 distal humerus, 1 calcaneum, all from midden in both deep and shallow levels.

Bison: from the midden, middle depths: 1 cervical neural arch.

Horse: bones of horses were almost commonplace: 6 otoliths, 2 cannon-bones, 5 or 6 foot bones, 1 hoof fragment, 1 calcaneum, 1 astragalus, 1 metapodial, 1 axis, and numerous teeth.

Most sections of the site that yielded animal bones, had horse bones. They were found in all levels. The conclusion is inescapable that trade material and horses were present through much of the life of the site. Here antelope are doubtful,

* The identifications that form the basis for this paper were done by Dr. Theodore White, Smithsonian Institution, River Basin Surveys, and by Arthur Freed, Department of Anthropology, University of California. The expenses of the work done by Mr. Freed were defrayed by a grant from the Initiative 171 Fund for Biological Research of the University of Washington. Martha Flahaut, Curator of Biology, Washington State Museum, has advised during the preparation of the paper. Drs. White and Roberts have criticized an earlier draft.

Data are drawn from nineteen archaeological sites: ten of these were excavated under Smithsonian Institution direction; three were under National Park Service contracts with the University of Washington; three were by the University of Washington; one was made by that University in the 1920's; two were surveyed only by the University, and data from one are drawn from a published source. A grant from the Agnes Anderson Fund defrayed the expenses of an archaeological survey that gathered surface data from the two sites pertinent to this paper.

† The number designations for archaeological sites are those used by the Smithsonian Institution, River Basin Surveys. The first element, 45, is the State of Washington, forty-fifth in an alphabetical listing of the states. BN is an abbreviation for Benton County; 3 is a simple sequence or find number. This method of site designation, or variants, is widely used throughout the country.

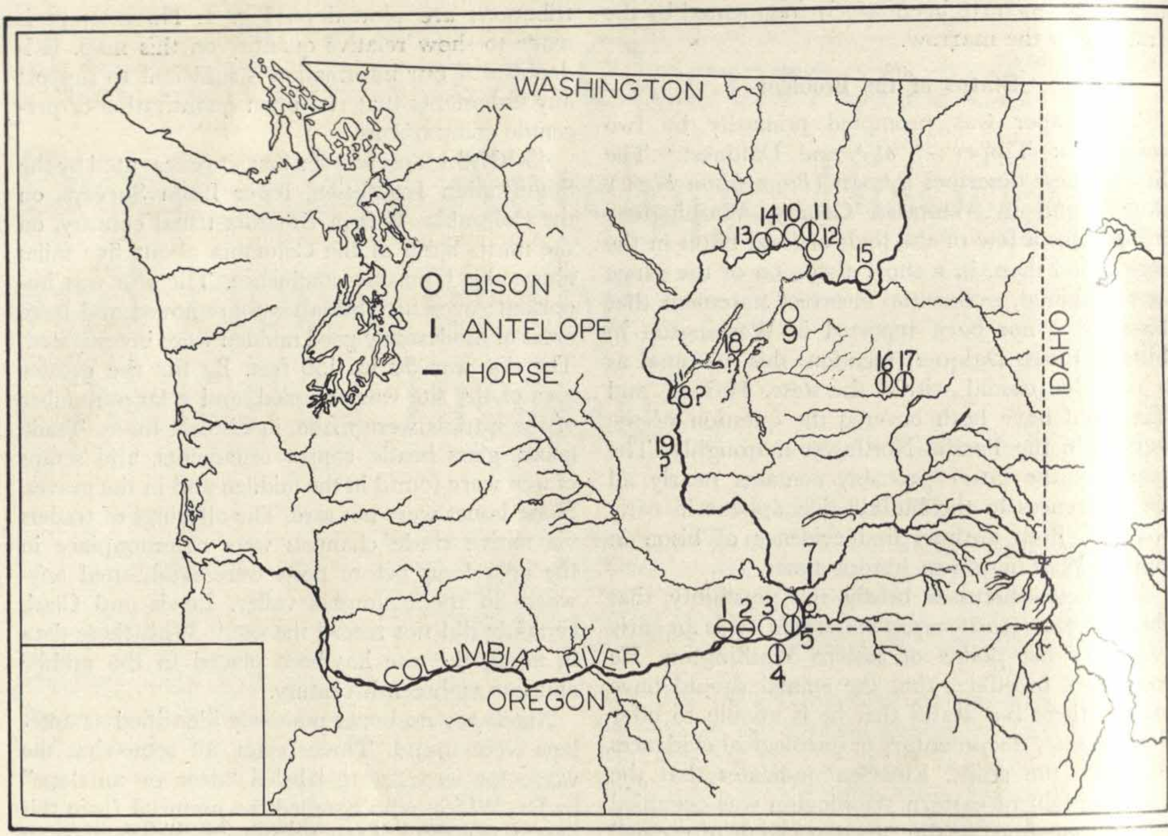


FIG. 1. Archaeological sites and mammal bones.

1. 45-BN-3, Late site; deer or antelope?, bison, horse
2. 45-BN-6, Late site; bison, horse
3. 45-BN-53, Prehistoric; bison
4. 35-UM-17, Prehistoric; antelope, bison (Oregon)
5. 45-BN-16, Prehistoric; antelope, bison
6. 45-BN-15, Prehistoric; bison
7. 45-FR-5, Prehistoric; antelope
8. Potholes site, Late; antelope?
9. 45-GR-2, Prehistoric; bison
10. 45-OK-10, Late; bison
11. 45-OK-5, Prehistoric; antelope, bison
12. 45-OK-2, Late; bison
13. 45-OK-11, Prehistoric; bison
14. 45-OK-12, Prehistoric; bison
15. Collier, *et al.*, 1942, site 11, Prehistoric; bison
16. 45-LN-3, Prehistoric; antelope, bison
17. 45-LN-1, Prehistoric; antelope, bison
18. 45-DO-3, Not excavated; deer or antelope?
19. 45-KI-18, Not excavated; deer or antelope?

bison *ca.* 0.017 per cent, and horse *ca.* 10 per cent.

45-BN-6. This, another late site, is five miles beyond BN-3, upstream on the same bank. Sixty housepits and a few rock piles, possible cairns, are scattered for nearly half a mile along a low flat terrace which varies from 4 to 12 feet above the Columbia. Most of the housepits are concentrated at the downstream end. Only four were excavated. BN-6 is probably one of the most recent villages worked by archaeologists in the area. Much in the way of traded material was found; even a bit of canvas was recovered from one housepit. A reference indicates that the site is to be identified with one noted in 1812.[13]

Antelope: no bones.

Bison:

Housepit 5, in central, trenched, part of housepit at 1.5–2.0 feet from surface: 5 ribs; at 2.0–3.0 feet from surface, 11 articulated dorsal vertebrae, 1 phalanx.

Housepit 6, at 1.0–1.5 feet from surface: 1 scapula, 1 phalanx.

Housepit 7, at 1.5–2.0 feet: 1 patella, 1 mandible; at 1.5–1.7 feet: 1 metapodial.

Horse:

Housepit 5, at 0.5 foot below surface: 1 radioulna, 1 metapodial.

Housepit 6, no location: 1 metapodial.

Housepit 7, 1.0–1.5 feet from surface: 1 skull fragment.

Housepit 59, at 0.5–1.0 foot from surface: 1 femur.

Test pit 1, in thin midden near river bank, no depth recorded: 2 radioulnae, 1 metapodial.

At BN-6, bison bones constituted 26 per cent, and horse bones about 12 per cent, of the mammalian remains.

45-BN-53. This is a large housepit village site; 183 pits were mapped. It lies between 45-BN-6 and BN-3. Only seven housepits of the large total were adequately investigated. No trade items were found in either the excavations or on the surface, with the sole exception of two glass beads found near the river's edge. The large site is thus before contact:* early eighteenth century, at the very latest. It is probable that the people who lived at BN-53 abandoned that site shortly before contact and began to live, while in their seasonal, semi-permanent, shallow pit houses, at BN-6, a few miles upstream. It will be recalled that this latter site had a contact occupation.

Antelope: no bones.

Bison: *Housepit 45*, lower levels: 1 radius, several cervical vertebrae.

Horse: no bones (as is usual for precontact occupations).

Here, the bison bones were a minor percentage of all mammalian bones.

35-UM-17. This island site is in the state of Oregon, Umatilla County. It is near Cold Springs, some thirteen water-miles above Umatilla, Oregon. Excavations were largely confined to trenching and excavating of the more promising of the nineteen housepits that comprised the site. In all, four were examined. The island is in Walula or Umatilla territory, the site precontact.

Antelope:

Housepit 15, at depth of *ca.* 2 feet in an area of bone disposal, 2.0–2.5 feet below surface: 1 mandible.

Bison:

Housepit 15, 2.0–3.0 feet below surface: 1 metapodial.

Housepit 14, 4.0–4.5 feet below surface: 4 phalanges.

Horse: no bones.

Of the bones found here, 2 per cent were bison and 4 per cent were antelope.

45-BN-16: This group covers the occupations of an entire island—Goat Island in the Columbia about six miles south of the Snake confluence in Benton County. Unfortunately, in spite of extensive prospecting, no deposits worthy of full-fledged excavations were found. One burial turned up; numerous test pits and clearings of debris were made, but thin or nearly sterile deposits led us to abandon the work there. Like FR-5, this site is in Walula tribal country. It is not possible to place the whole site or sections of it chronologically, but the areas excavated were prehistoric.

Antelope(?): test pit 1A, in level 1.0–2.0 feet: 1 distal metatarsal.

Bison: test pit 1, 0–1.0 foot: 1 head of femur.

Antelope bones form 4 per cent and those of bison form 4 per cent of the nonhuman mammalian remains from this site.

45-BN-15. This site, or more properly, group of sites, is on Rabbit Island immediately upstream from the Goat Island of BN-16. Like the latter, collectors have ravaged most of the better archaeological manifestations, with one exception. This is a series of burials protected by their greater than average depth. The deeper burials were found by the archaeologist while working out some more superficial ones. There thus is a burial sequence here. The artifacts found with the older burials, which were interred extended, differed from those found with the later, flexed or folded inhumations. Thus far, twenty-six burials have been found. The site bids fair to be an important one. Neither houses nor full-fledged midden was found on the island; no trade goods occurred.

Antelope: no bones.

Deer or antelope: midden, 42 inches deep: 1 right distal humerus.

Bison: from midden: 1 tibia.

Horse: no bones.

45-FR-5. A large housepit site, 45-FR-5, is on Strawberry Island, eight miles up the Snake River from its confluence with the Columbia. There are at least 131 housepits near the head of the island. They are saucer-shaped, rimmed depressions varying from 15 to 54 feet in diameter and from 0.3 to 2.9 feet in depth. No large midden deposits or burials were found on the island. All the animal bones studied were found in the small midden accumulations in the four housepits that were partly excavated or tested. Judging from the excavation and surface gleanings, FR-5 is wholly prehistoric. This observation may not be correct, but there can be no doubt that the housepits dug were occupied before trade goods entered the area or before they became an important aspect of local culture. This would place the site, or at least such of the widely scattered pits as were dug, at not later than the early eighteenth century. Lewis and Clark, who traveled in the area in 1804 and 1805, mention no occupation of the island.[12] No horse bones, also a good criterion of recency, were taken from the site. Haines suggests 1720–30 as the date for the

* "Contact" is a term used to designate that period when the Indian made face-to-face contact with the white man and during which trade objects and immaterial aspects of European-American culture began lively diffusion to the natives.

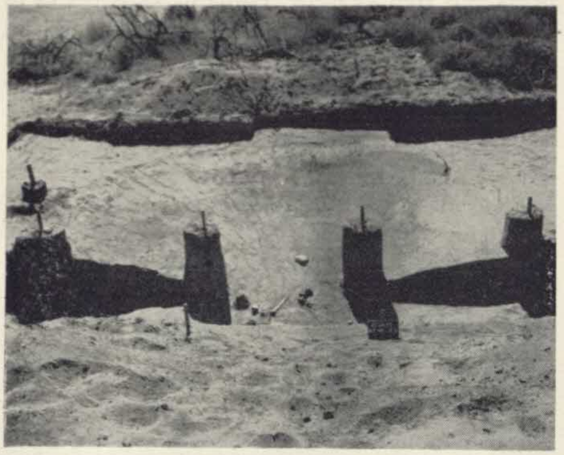


FIG. 2. Bison radius and cervical vertebrae near the fire area, main "floor" of housepit 49, 45-BN-53, McNary Reservoir. The saucer-shaped floor was cleared following the primary occupation level.

entrance of the horse into local Indian culture, a dating that has in so far as I am aware, been acceptable to workers in the region. This site is in country now assigned to the Walla Walla or Walula tribe by ethnographers.

Antelope:

Housepit 2, (trenched) from surface to 3.3 feet deep: 2 left distal tibiae, 1 right calcaneum, 1 distal metatarsal, 3 skull fragments, 2 sternebrae.

Housepit 59, (trenched) from surface to 1.0 foot deep: 2 astragali (right and left), 2 metatarsals, 2 teeth; from 1.0–2.0 feet: 2 right astragali, 2 left distal tibiae, 1 right distal humerus, 1 right scapula, 2 right proximal ulnae, 1 left calcaneum, 1 lower tooth; from 2.0–3.0 feet: 2 metatarsals (distal and proximal), 1 right distal tibia.

Housepit 78 (tested and trenched) 0–1.0 foot: no bones; 1.0–2.0 feet: 2 vertebrae (1 dorsal, 1 sacral), 1 jaw fragment, 1 distal metatarsal.

Housepit 87, 0–1.0 foot: no bones; 1.0–2.0 feet: 1 horn core, 3 teeth, 1 axis; from 2.0–3.0 feet: 1 maxilla, 2 distal tibiae; from 3.0–3.5 feet: 1 jaw, 2 distal tibiae, 1 radius.

Bison: no bones.

Horse: no bones.

Forty-eight antelope bones were found in the limited excavations; they are representative of most of the major parts of the skeleton. Well over half (twenty-nine) of the bones are appendicular; they are the bones that one would logically expect to come into the village with partly butchered meat. The assemblage is not at all suggestive of bones traded in or saved for future use in making artifacts. The results of the food quest, primarily, are present. Antelope bones formed 28 per cent of the mammalian bones found at FR-5; jackrabbit (*Lepus townsendii*) bones formed 46 per cent. The remaining bones were those of squirrel, salmon, beaver, and frog.

Potholes site. This site was excavated by a nonarchaeologist in the 1920's. Material found was deposited in the Washington State Museum. Apparently only graves were sought. Presumably most of the unworked bones found were discarded. Historically, the site would be in Columbia tribal territory. Contact or trade objects were found in the graves; the site was thus of a late date.

Antelope: one piece, worked into a wedge, is possibly an antelope radius (Washington State Museum, catalog No. 9304).

Bison: no bones, or none saved.

Horse: no bones, or none saved.

45-GR-2. This is a rock shelter or cave site, as we have rather loosely used the term. It is about eight footpath miles southwest of the town of Grand Coulee in the basaltic west wall of the upper Grand Coulee, Grant County and thus is in the southern part of the Nespelem tribal range. An analysis of the archaeological aspects of the excavation has been published.[14] The site deposits were prehistoric in date.

Antelope: no bones.

Bison: from second 6-inch level from surface: 1 mandible, 1 tibia, 1 astragalus; at *ca.* 4.0 feet from surface: 1 horn; at 5.2 feet depth: 1 mandible, 1 hoof; at *ca.* 2.5 feet: 1 astragalus, 1 calcaneum.

Horse: no bones.

Four per cent of the mammalian bones of GR-2 were those of bison.

FIG. 3. Housepit 14, 35-UM-17, under excavation. The series of trenches yielded a series of profiles that enabled the archaeologists to visualize subterranean construction.

45-OK-10. This is a small, three-housepit site some twenty miles down the Columbia from the Grand Coulee Dam. One housepit was excavated; several tests were made. The housepit yielded trade goods. This site, like the others, has already been described.[3]

Antelope: no bones.

Bison: test pit 5, from 1.0–2.0 feet: 1 astragalus.

Horse: no bones.

Only 0.7 per cent of the bones were those of bison.

45-OK-5. A small, five-housepit site, 45-OK-5 is on the north bank of the Columbia River about twenty miles below the Grand Coulee Dam in Okanogan County. Two of the housepits were thoroughly trenched; test pits were dug. The site is within the modern Nespelem tribal range. Results of the excavation of OK-5 and other sites in the area, i.e., within the Bureau of Reclamation's Chief Joseph Reservoir floodline, have been published.[3] The time of occupation of the housepits excavated was prehistoric.

Antelope:

Housepit 1, (trenched) in central section in 2.0–2.5-foot level: 1 mandible (right half).

Bison:

Housepit 1, 1.0–2.0 feet from surface: 1 femur, 2 tibiae, 8 metapodials, 2 carpal or tarsal bones, 2 astragali, 1 calcaneum; depth 2.0–3.0 feet: 1 femur, 2 tibiae, 8 metapodials, 4 phalanges, 1 ungual phalanx, 1 carpal or tarsal bone; at 3.0–4.0 feet in depth: 1 tibia.

Housepit 3, 1.0–1.5 feet in depth: 1 scapula. Unlocalized buffalo bones from the site: 1 humerus, 1 femur, 3 metapodials.

Horse: no bones.

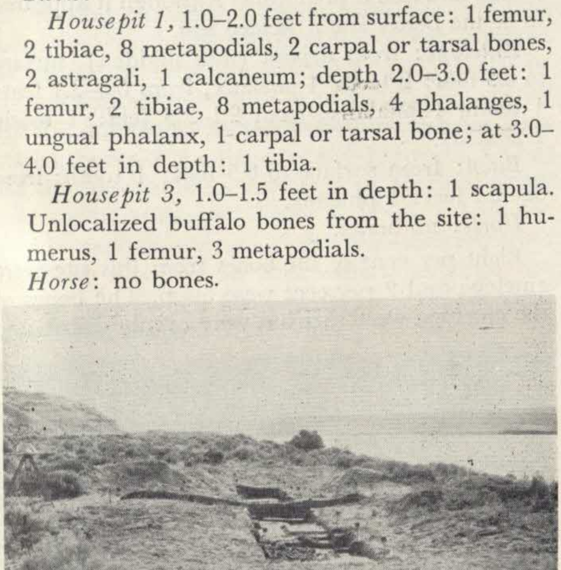


FIG. 4. The remains of a recent type, elongate, mat house under excavation. Housepit 59, site 45-BN-6. A wide central trench and a narrow cross trench dissected the deposit.

FIG. 5. Excavation in the basalt cave, 45-GR-2, near the Grand Coulee Dam. Preservation of bone was excellent here in spite of the coarse, rocky fill. Note the hard hats.

Bison from 4 per cent and antelope 0.01 per cent of the mammalian bones from this site.

45-OK-2. This is a housepit site (16 pits present) about fifteen miles below the Grand Coulee Dam. It is within Nespelem tribal territory. Seven housepits were tested or otherwise excavated during the 1950 season. Two of the pits were postcontact, and contained an abundance of trade goods. The others lacked European- or American-made items. The site had no doubt been used for a long period. A description of the excavation has already appeared.[3]

Antelope: no bones.

Bison:

Housepit 5, occupied during postcontact times, 0–1.0-foot level: 2 phalanges, 1 astragalus.

Horse: no bones.

Three per cent of the bones found were those of bison.

45-OK-11. This is also a small site, in keeping with the settlement pattern of the area. It is about thirty miles below the dam. The eight pits were divided into four-pit groups. Four of the housepits were trenched. This site, which is prehistoric, has already been reported upon.[3]

Antelope: no bones.

Bison:

Housepit 6, prehistoric (no contact material), 0–1.0 foot in depth: 1 metapodial; 0.8 per cent of the bones recovered.

45-OK-12. This site is about 1000 feet downstream from OK-11. Only three pits were found; one was tested and was found to contain only aboriginal material.[3]

Antelope: no bones.

FIG. 6. Unexcavated housepit 6, site 45-OK-11, Chief Joseph Reservoir, Columbia River. The dark circular area in the mid-foreground outlines the shallow saucer-shaped pit over which a (presumably) wood and mat superstructure was erected.

Bison:

Housepit 2, 0–1.0 foot: 1 phalanx.

Horse: no bones.

Few bones came from this site; this phalanx represents 12 per cent of the amount found.

Upper Columbia 11. Collier *et al.*[15] found bones of bison four to five feet deep in the undisturbed midden of this prehistoric (no contact material) site.

45-LN-3. This cave or rock shelter site, in Lincoln County, Washington, is about seventeen air miles north northeast of Ritzville in the basalt cliffs above the north bank of Crab Creek. A ten-foot trench to the sterile soil was cut into the deposit. The site is wholly precontact.

Antelope: from deposit (thin midden), surface to 1.0 foot: 2 astragali, 15 metapodials (6 proximal, 9 distal), 1 sacrum, 9 phalanges, 7 lower teeth, 1 right distal humerus, 1 right jaw fragment, 3 maxillae (2 right, 1 left), 1 tooth, 3 distal metacarpels, 1 dorsal vertebra; from 1.0–2.0 feet: 1 tibia, 5 teeth, 2 phalanges, 4 metapodials (3 proximal, 1 distal); from 1.0–6.0 feet: 3 left maxillary fragments, 1 right jaw fragment, 22 deer or antelope bones (deer were also present at the site); from 2.0–3.0 feet: 1 right maxilla, 2 lower teeth, 3 metapodials (1 proximal, 2 distal), 1 distal humerus, 1 right astragalus, 2 phalanges, 1 right proximal ulna; from 2.0–6.0 feet: 1 left jaw fragment, 1 right premaxilla, 16 deer or antelope bones.

Bison: from deposit (thin midden), surface to 1.0 foot: 1 right radius, 1 upper tooth, 1 left jaw fragment, 1 left ilium, 5 vertebrae (3 dorsal, 2 lumbar), 9 carpal bones, 2 phalanges, 1 right proximal tibia, 2 sternebrae, 1 sacrum; from Room A, a superficial pocket: 1 distal humerus, 1 fetal humerus, 1 dorsal vertebra, 1 sternebra, 1 carpal bone, 3 phalanges, 1 fetal metatarsal; from 1.0–2.0 feet: 2 hyoids (1 right side, 1 left side); from 1.0–6.0 feet: 1 immature right pelvis, 1 cuboid, 1 lower tooth, 2 phalanges; from 2.0–3.0 feet: 1 dorsal vertebra, 2 phalanges, 1 right proximal radius, 2 carpal bones; from 2.0–6.0 feet: 4 phalanges, 2 carpal bones, 1 immature distal tibia, 2 immature distal metapodials.

Horse: no bones.

Of the large number of bones (670) found at this site, about 11 per cent are those of antelope and 8 per cent those of bison. The remainder are largely from small game such as ground squirrel, jackrabbit and cottontail, gophers and marmots. The top foot of the deposit held almost half of the antelope and bison bones. There is a marked increase in the amount of bones of both animals from the deeper levels to the surface; this is more regular for the antelope than for the bison. The levels removed here should have been six inches, rather than one foot, to secure a more sensitive record.

45-LN-1. This is a site similar to LN-3. It is approximately two hundred yards east of the former and also is prehistoric. Although it appeared to be the better site it yielded less.

Antelope: from deposit (thin midden), surface to 0.5 foot: 2 teeth, 1 phalanx; from 0.5–2.1 feet: 1 tooth, 1 phalanx; from 2.1–3.9 feet: 1 tooth, 1 phalanx.

Bison: from surface to 0.5 foot: 1 tooth; from 0.5–2.1 feet: 1 phalanx.

Horse: no bones.

Eight per cent of the bones from this site were antelope and 2 per cent were bison. The bones of the antelope were rare but were evenly distributed

FIG. 7. Housepit 1, site 45-OK-1. Partly cleared, in process of mapping.

throughout the deposit; those of the bison were within the top two feet of this site.

45-DO-3. A cave in Douglas County, this site was found by survey in 1952. Surface collection only: deer or antelope: 1 distal metapodial.

45-KL-18. This is a cave as is DO-3. Surface finds only: deer or antelope: 1 carpal bone. Sternberg[16] found modern bison bones associated with artifacts at "Pine Creek."

Data from these last three sites are not included in the final tabulation or in the discussion.

Conclusions

When the finds of animal bones are plotted on a map it becomes apparent that both of the wild forms have sufficient distribution so that their presence in the area cannot well be a point of argument. True, FR-5 and BN-16 are not so far from Oregon that hunting parties could not have carried antelope meat home from territory that is now within that state. When one considers the physiography and the hunting habits of the people, however, this possibility becomes little less than remote. The antelope mandible at 45-OK-5 is probably an import into that foothill-like region but it certainly could not be taken to indicate trade with, or hunting parties to, Oregon. Also, the fact that antelope was the second most important mammal at FR-5 (the most important was jackrabbit) indicates nothing more than local hunting. At nearby BN-16, antelope was also important. As has been stated, bones from the sites BN-3, BN-15, DO-3, KL-18, and Potholes were deer or antelope bones and are thus dubious. Those at BN-3, a Late site (trade goods abundant), were across the river from northern Oregon, an area where antelope are known historically. The Lincoln County caves had a respectable quota of antelope.

Bison, with the exception of sites FR-5, BN-16, and the Lincoln County caves, was the more important game animal. Like the antelope at FR-5, sufficient parts of the skeleton were generally present to indicate that the animal was hunted close to home. Bones were fragmented for the marrow, evidence that they had not been carried great distances for other uses. It is well known that tribes of Washington and Oregon, from the Okanogan to the Nez Percé, made journeys in the latter half of the nineteenth century and possibly somewhat earlier to the country east of the Rockies to hunt bison. This hunt was often as much for purposes of pleasure, travel, and trade as for procuring meat. Loads of dried meat were, of course, brought home. The bones that we have found do not fit into this situation nor into a theory that they were selected

Fig. 8. Housepit 3, site 45-OK-4. Beginning of excavation.

as raw material for bone artifacts. In the sites bearing bison bones, deer (*Odocoileus*) was usually the most important mammal. This was doubly true in the north, in Okanogan County. It appears that bison, like antelope, were available to Indian hunters in both the north and the south. The quantities and proportions of bones suggest that these animals were, at least from time to time and place to place, more than casual visitors. Probably small bands and strays were neither unusual nor rare in both prehistoric and protohistoric or very early historic times in eastern Washington. The fetal and immature bones from 45-LN-3 indicate breeding in the area.

Table 1 presents some interesting information. Here the site occurrences have been given for the three mammals by lower and upper levels. It would not be of value here to present my reasons for assigning each find to an upper or lower level. With stratified sites such assignment is simple. However, stratified sites are rare in the Plateau; consequently, it has been necessary to split each site according to its relative depth, i.e., a site with a four-foot-deep deposit would, all other things being equal, be split into an upper level of the top two or two and one-half feet and a lower one of two or one and one-half feet. This is obviously arbitrary. The measure of a site's recency is of course the presence or absence of trade items such as glass beads, iron and copper. Horse bones, too, are a mark of recency and of course, as such, appear only in the Late sites in Table 1. This is not so much an aspect of reasoning in a circle as it is evidence of the control that the mammalogists have of their material. I should also add that no site has been judged "Late" solely on the presence of horse bones. The finds of trade items were primary evidence;

TABLE 1*

		Bison	Antelope	Horse
Late sites	Upper levels	4	1(?)†	2
	Lower levels	2	1(?)	1
Prehistoric sites	Upper levels	8	4	
	Lower levels	5	5	

* Potholes site, omitted. No data available as to depth of questionable antelope find.

† 1(?) is 45-BN-3, site 1 Fig. 1. Separate occurrences listed in columns.

the osteological reports came in later as interesting corroborative evidence.

I am not impressed by the fact that there are more finds of bison for the Prehistoric sites. There are four Late sites and eleven Prehistoric ones. It is true that more digging has been done on the Late sites, so that disparity of earth turned is far less than a 4 to 11 ratio. As mentioned before, the time is not yet ripe for a quantitative analysis here. What is important, in so far as bison are concerned, is that they have been present from wholly precontact times until well into the contact period.

Antelope, I am convinced, show a different picture. All antelope bones from Late sites are "deer or antelope" bones in poor condition. The certain finds are all from Prehistoric sites, 28 per cent antelope at FR-5. It would appear that the obvious interpretation is that antelope were present in prehistoric times, possibly as late as the 1600's, and that they then become nearly extinct in Washington. A few old-timers in the Pasco area (some eight miles from FR-5) recall antelope in the region in early days. Whether these men are thinking of herds in Oregon or in Washington is obscure. Ethnographers now in the field are finding that antelope were well known to the Indians of southern Washington. It is wholly possible that temporal depth on the ethnographic level can be found when both northern and southern tribes are well studied. Ray[17, 18] describes or lists antelope hunting for the Sanpoil and Nespelem, Wenatchi, of north central Washington, and the Klikitat, Tenino, Umatilla, and Kittitas of southern and central Washington. It is of interest that antelope and horse are never found together in spite of the fact that the horse (a late arrival in the area) finds are grouped in the south, nearest the present antelope range. Certainly the information that Dalquest[5] presents and that which has come from this study coincide. He sees the antelope as a mammal that should have ranged in the eastern part of the state; my study shows it as a recent habitant.

The data for the presence of bison and antelope in eastern Washington in prehistoric time and, in so far as the bison is concerned, in protohistoric and historic times, are sufficient. The questions raised by these data must remain unanswered, at least by an archaeologist, for the present. Digging will be done in the future in the area, and it may be that clarifying information will be forthcoming.

It is difficult to suggest reasons for the apparent abandonment of the region by the pronghorn, a mammal so suited to it. I can see no reason why the acceptance of the horse, particularly by the tribes of the southern Washington-northern Oregon areas, should have made life more difficult for antelope in the northern state than in Oregon. It must be that climatic changes were primary in displacing the antelope from the northern section of their range, if my data and interpretations are correct. Nothing less than a change in living conditions or a most virulent epidemic illness could adequately explain the situation, and the latter is a sort of last-refuge suggestion.

In his magnificent study of postglacial forest successions, Hansen[19] has presented evidence of a moist, cool, climatic pulsation beginning about 4000 years ago, and possibly tapering off at about the present time. Colder winters, deeper and longer-lasting snows, and an influx of northern predators —all of them aspects of such a climate in the region—might well have upset the balance that made the eastern part of Washington habitable for the antelope. Their next stand would have been in eastern Oregon, a still drier area than Washington, and a part of the antelope range to the present day.

References

1. FREEMAN, O. W., FORRESTER, J. D., and LUPHER, R. L. *Ann. Assoc. Am. Geographers,* **30**, 2 (1945).
2. RAY, V. F. *F. W. Hodge Anniversary Publ. Fund,* No. III, Southwest Museum, Los Angeles (1939).
3. OSBORNE, D., CRABTREE, R., and BRYAN, A. *Am. Antiquity,* **17**, 360 (1952).
4. TIPPER, H. W., JIZBA, Z. V., and KONIZESKI, R. L. *Am. J. Sci.,* **249**, 322 (1951).
5. DALQUEST, W. W. *Univ. of Kans. Publ.,* No. 2, 404 (1948).
6. HAINES, F. *Pacific N.W. Quart.,* **31**, 389 (1940).
7. KINGSTON, C. S. *Wash. Hist. Quart.,* **23**, 163 (1923).
8. EINARSEN, A. S. *The Pronghorn Antelope and Its Management,* map and p. 3. Washington, D. C.: Wildlife Management Inst., 1948.
9. HAINES, F. *Am. Anthrop.,* **40**, 429 (1938).
10. ROE, F. G. *The North American Buffalo. A Critical Study of the Species in Its Wild State.* Toronto, Canada: Univ. of Toronto Press, 1951.
11. RIDDELL, F. A. *Am. Antiquity,* **18**, 168 (1952).
12. THWAITES, R. G. *Original Journals of the Lewis and Clark Expedition, 1804–1806,* **6**, map No. 31. New York: Dodd, Mead & Co., 1905.
13. ROLLINS, P. A., Ed. *The Discovery of the Oregon Trail, Robert Stuart's Narratives,* p. 73. New York: Scribner's, 1935.

14. Mills, J. E., and Osborne, C. *Am. Antiquity*, **17**, 352 (1952).
15. Collier, D., Hudson, A. E., and Ford, A. *Univ. of Wash. Publ. Anthrop.*, **9**, 126 (1942).
16. Sternberg, C. H. *Trans. Kansas Acad. Sci.*, **28**, 90 (1903).
17. Ray, V. F. *Univ. of Wash. Publ. Anthrop.*, **5**, 82 (1932).
18. ———. *Univ. of Calif. Anthrop. Records*, **8**, 116 (1942).
19. Hansen, H. P. *Trans. Am. Phil. Soc.*, **37**, 129 (1947).

Books Reviewed In SCIENCE

October 2

Structure Reports for 1949, Vol. 11. A. J. C. Wilson, Gen. Ed. Utrecht: Oosthoek, 1952. (For the International Union of Crystallography.) 477 pp. Illus.
Reviewed by Karl Lark-Horovitz.

Astrophysics: The Atmosphere of the Sun and Stars. Lawrence H. Aller. New York: Ronald Press, 1953. 412 pp. Illus. $12.00.
Reviewed by D. ter Haar.

Synthetic Organic Chemistry. Romeo B. Wagner and Harry D. Zook. New York: Wiley; London: Chapman & Hall, 1953. 887 pp. $11.50.
Reviewed by Ralph L. Shriner.

October 9

New Zealand Pollen Studies: The Monocotyledons. Lucy M. Cranwell. Cambridge, Mass.: Harvard Univ. Press, 1953. (For the Auckland Institute and Museum.) 91 pp. Illus. + plates. $5.00; paper: $3.50.
Reviewed by F. R. Fosberg.

Differential and Integral Calculus. Philip Franklin. New York–London: McGraw-Hill, 1953. 641 pp. Illus. $6.00.
Reviewed by M. E. Shanks.

Industrial Wastes: Their Disposal and Treatment. Willem Rudolfs, Ed. New York: Reinhold, 1953. 497 pp. Illus. $9.50.
Reviewed by W. T. Read.

An Introduction to Statistical Science in Agriculture. D. J. Finney. Copenhagen: Ejnar Munksgaard; New York: Wiley, 1953. 179 pp. Illus. $3.75.
Reviewed by Gordon E. Dickerson.

Atlas of Medical Mycology. Emma Sadler Moss and Albert Louis McQuown. Baltimore: Williams & Wilkins, 1953. 245 pp. Illus. $8.00.
Reviewed by Russell J. Fields and Raymond A. Osbourn.

October 23

Modern Radiochemical Practice. G. B. Cook and J. F. Duncan. New York: Oxford Univ. Press, 1952. 407 pp. Illus. + plates. $8.50.
Reviewed by Richard M. Lemmon.

Chemical Process Machinery. 2nd ed. E. Raymond Riegel. New York: Reinhold, 1953. 735 pp. Illus. $12.50.
Reviewed by Ju Chin Chu.

Speech and Hearing in Communication. 2nd. ed. of *Speech and Hearing.* Bell Telephone Laboratories Series. Harvey Fletcher. New York–London: Van Nostrand, 1953. 461 pp. Illus. $9.75.
Reviewed by John Q. Stewart.

The Science of Color. Committee on Colorimetry of the Optical Society of America. New York: Crowell, 1953. 385 pp. Illus. + plates. $7.00.
Reviewed by A. Chapanis.

October 30

Phosphorus Metabolism: A Symposium on the Role of Phosphorus in the Metabolism of Plants and Animals, Vol. II. Sponsored by the McCollum-Pratt Institute of The Johns Hopkins University. William D. McElroy and Bentley Glass, Eds. Baltimore: Johns Hopkins Press, 1952. 930 pp. Illus. $11.00.
Reviewed by Mark H. Adams.

Sampling Technique. William G. Cochran. New York: Wiley; London: Chapman & Hall, 1953. 330 pp. Illus. $6.50.
Reviewed by Marvin Zelen.

BOOK REVIEWS

The New Force. Ralph E. Lapp. x + 238 pp. $3.00. Harper, New York. 1953.

IN this latter-day Smyth Report, Dr. Lapp presents, primarily for the lay reader, an account of the evolution and present status of atomic energy, as it relates to both war- and peace-time uses. Scientists who have not been closely associated with the developments he details will find that this volume serves as a nontechnical summary of and commentary on numerous newspaper reports and technical accounts, and thus performs a useful function.

In nontechnical language (and occasionally using oversimplifications which in a different context would subject him to criticism from his colleagues), the author traces the experimental steps leading to the discovery of fission and the war-time development of the first A-bombs. The tale he recites in his early chapters is much more than twicetold, but the "I was there" approach he uses, together with the journalistic style employed, makes the reading pleasant and painless, if not exciting.

Postwar developments are viewed from the vantage point of a participant in many of the developments, and numerous personalized comments and pertinent quotations shed new light on some of the happenings in the atomic energy field since VJ-day. The transition from Manhattan District days to the period of AEC activity is treated in some detail, and the growth and accomplishments of the Commission are given adequate space.

In the chapter he entitled "Destruction Unlimited," the author comments on the greatly increased efficiency and destructive power of post-Nagasaki A-bombs, and devotes an extensive section to a treatment of the principles underlying the H-bomb. A discussion of the attempts made thus far to reach international agreement on the control of atomic energy leads the author to conclude ". . . the real significance of releasing atomic energy is that war must be abolished from this earth."

Recent technical developments and trends are summarized in chapters dealing with battlefield A-weapons (such as the small A-bombs for tactical use) and atomic power. Possible peace-time benefits are dealt with in his discussions of both the varied uses of radioisotopes and the commercial development of atomic power.

In a strongly worded statement on "Secrecy and the Atom," the author deplores what he considers to be an overemphasis on security. He feels that current security measures result in keeping from the American people facts they need in order to evaluate properly the meaning of atomic energy in war and peace. He also discusses the difficulties these policies pose for American scientists as they strive to unravel further the mysteries of the nucleus. His arguments along these lines are generally convincing, but the author at times makes overstatements which weaken his case.

With relatively few reservations, this reviewer finds it possible to agree with a summarizing sentence in Stewart Alsop's foreword to the book: "In *The New Force*, . . . Dr. Lapp has been able to present the facts of atomic energy clearly, readably, and yet with complete scientific accuracy."

BOWEN C. DEES

National Science Foundation
Washington, D. C.

Teamwork in Research. George P. Bush and Lowell H. Hattery, Eds. xii + 191 pp. $4.00. American University Press, Washington, D. C. 1953.

IN the last ten years, research and development has become an important phase of big business; in fact, research and development has become big business on its own account. Although research and development had a place in industry before World War II, the real impetus toward increasing it came through military research in the years from 1940 to 1946. During this period many research men who were excellent scientists had a chance to try their skill as scientific administrators. Fortunately, a large percentage of them were successful, and those who were not could ordinarily hide their failures in the fog of changing objectives, loss of personnel, and unstable finances of this war period.

At the end of the war, a small group of these amateur research administrators got together and talked about their successes and failures. It was quite apparent that there was much to be learned from both. This led to a symposium or seminar on the administration of research on a national scale. One such group, starting in 1947, has held a meeting each year and has brought together research administrators from Government, industry, and the colleges. It is almost inevitable that similar symposia and conferences, both transient and permanent, will spring up here and there. Several colleges have instituted courses in this field, and the literature on research administration is becoming quite extensive. It seems that by now almost all facets of research administration have been discussed and thoroughly explored—costs, recruitment, training, inventiveness, patents, and publication policies have all been the subject of one or more extensive discussions.

This particular collection of papers is aimed at a discussion of "teamwork in research." No one denies the importance of such teamwork, and most of us will admit that the lone inventor and scientist is not as prevalent or as important in the total research picture as he used to be. Science is too broad and requires too wide a knowledge for a lone inventor to contribute. A team, contributing from many fields of science, is a much surer way of achieving success on large problems.

This book covers four phases of teamwork—organization, personnel factors, aids to teamwork, and case examples by some who have been only analysts of the

situation. What use either an experienced or prospective research director might make of this book is obscure, but one talk, by M. A. Tuve, clearly shows what makes for teamwork. This chapter, which is all too short, faithfully reflects the enthusiasm, hard work, and devotion to the job which makes a team a productive one. However, how this is done or by what rules one accomplishes it is not set down in books. Baseball, like research, is big industry too. The Chicago Cubs alone operate on a budget of three million dollars per year. Baseball too, like research, is played by teams, the individuals of which are experts in their jobs. It is my opinion that the best way to play center field, as Ralph Kiner plays it, is to have some native talent and then serve as an understudy to Kiner, and not to read a book on teamwork in baseball. Similarly, the best way for a leader to learn to encourage teamwork by his research group is to watch how men like Tuve do it and then do likewise.

Eric A. Walker

School of Engineering
Pennsylvania State College

The Road to Abundance. Jacob Rosin and Max Eastman. vii + 166 pp. $3.50. McGraw-Hill, New York. 1953.

THIS book is a gospel of two freedoms made possible by chemistry.

Freedom from food: someday we shall make fats and carbohydrates in the chemical factory, and satisfy our needs for proteins by eating synthetic amino acids or plankton or Chlorella algae grown in tanks. For "there is nothing profoundly natural about our eating natural food. . . . The starch of the potato is meant by nature to be a food reservoir for the potato plant, not the human stomach; the milk of the cow is meant for calves. . . . A balanced mixture of (synthetic) amino acids is a more logical raw material for the natural synthesis of our body proteins than the animal and vegetable proteins we eat." (p. 33.)

Freedom from the mine: abundant, cheap, energy from the sun will revolutionize our chemical processes and allow us to extract traces from tons. The tons will be sea-water, low-grade ores, neglected raw materials, and recovered industrial wastes. The traces will be gold, magnesium, bromine, uranium, nitrogenous fertilizers, rubber, titanium, and a host of new synthetics.

How shall we achieve these two freedoms? By a "central authority backed by governmental funds (mobilizing) the entire industry, assembling America's best brains in an organized effort." (p. 92.) And what will these two freedoms give us? The "eight-hour week or month. Human effort may be absorbed in pure and scientific research and aesthetic creation, in a society based on absolute abundance." (p. 108.)

It is unfortunate that the style is so studied as to make the exciting subject matter seem dry indeed; that in preaching their gospel the authors should lack humor, should speak cynically of the "pathetic efforts of technological reactionaries to defend natural fibers" (p. 30), and should appear as defeatists in pleading their cause. Elsewise they might achieve more converts to their doctrine than, I fear, there will be.

There are some scientific errors: the critical step in getting magnesium from sea water is in precipitating filterable magnesium hydroxide (p. 71); scintillation of a single alpha particle is one of many chemical experiments involving less than a thousand molecules (p. 153); war was a consequence, not a cause, of the Haber process, first described in 1908 (p. 128).

But there is also a multitude of fascinating little facts that will excite the scientist: two-thirds of our energy from coal, oil, and gas is wasted into the air; three days of sunshine falling upon our earth gives us more energy than could be gotten from all the oil, coal, and timber on the earth; a one-acre algae farm could rival 100 acres of wheat; radioactive carbon-14 experiments show that the earliest agricultural settlement was 7,000 years ago in Mexico, not in the Old World; and a single transcontinental telephone call utilizes 13,200 electronic tubes.

Although written in a pedantic style, the book has much original thought and a real message.

Hubert N. Alyea

Frick Chemical Laboratory
Princeton University

Digging Beyond the Tigris. Linda Braidwood. xii + 297 pp. Illus. + plates. $4.50. Schuman, New York, 1953.

EXCAVATIONS in Mesopotamia began about a hundred years ago, largely because of the western world's interest in the Scriptures. The feverish quest in such ruins as Nineveh, Khorsabad, and Nimrud, was primarily for reliefs and inscriptions shedding new light on the Bible.

At the turn of the century a broader humanistic approach began to pervade the Near Eastern excavator. His major concern was no longer Biblical relevance but the history and culture of the three high ancient civilizations: Sumerian, Babylonian, and Assyrian. At the same time he developed more trustworthy excavating techniques based on stratigraphy and architecture. Inscriptions were still more than welcome, but now all significant "finds" were carefully gathered and studied, particularly the lowly but indestructible potsherd. Even so, the stress was still on the literate cultures, and the prehistory tended to suffer from neglect.

In more recent years, largely due to the rise of anthropology as a full-fledged academic discipline, Mesopotamian archaeology is witnessing a considerable shift in temper and attitude. Interest is no longer concentrated on the elaborate city sites with their promise of temples, palaces, and inscriptions. Some archaeologists prefer to dig the ruins of the peasant villages which preceded the urban civilizations by many centuries, particularly those located in the out of the way highland districts of Northern Iraq. For there is reason to believe that this is the region where man's first radical economic revolutions took place, where the food-gatherer was transformed to a food-producer.

One of the more prominent pioneers in this recent trend is Robert Braidwood, of the University of Chicago, who combines a rich experience in Near Eastern archaeology with the fresh and fruitful anthropological approach. In 1950–51 he conducted excavations at Jarmo, a remote site in the Kurdish hills, which covers the ruins of the oldest known Mesopotamian farming village. *Digging Beyond the Tigris* is the story of this excavation.

The author of the book, however, is not the director, but his wife Linda, and its pages do not deal with the scientific results of the expedition except for one highly informative chapter near the end. The book is devoted primarily to a description of "life" on the dig, its laughter and its tears, its thrilling excitement and its dismal monotomy. The reporting is at times a bit too literal and obvious. Yet by the time the reader has turned the last page he will have participated vicariously in a rather novel, and refreshing, if far from comfortable way of life. What is even more important, he will be deeply moved, as I was, by the unquestioning and unswerving devotion to fundamental scholarship and pure research on the part of a small band of American scientists eager to help uncover the history of man's past—a not uncomforting phenomenon in this atomic age with its pressure for "salvation" rather than truth.

SAMUEL NOAH KRAMER
University Museum
University of Pennsylvania

Cacti and Succulents. G. Gilbert Green. 238 pp. Illus. $7.00. Pitman, London, New York. 1953.

CACTI and succulent plants will probably always be fascinating for those who grow plants and at the same time are attracted to the bizarre in plant form. Numerous books have been written upon the subject of growing succulents since they first became known horticulturally, which actually means since their discovery, for many of them have been made known to the scientific world by growers rather than by botanists. *Cacti and Succulents*, one of the latest of many similar works in several languages, is devoted in great part to sketchy descriptions of the plants, many of which are not available to his readers. His horticultural advice is mostly sound, and is applicable wherever the plants may be grown; however, the soil mixtures that he recommends are a bit coarse for the American climate, and keeping small pots dry all winter is not recommended in this climate. It is also questionable whether all of the plants he indicates as being suitable for window-sill culture are truly satisfactory for that purpose. The illustrations are rather pleasing, although some are too small to give the proper conception of the plant shown. The color pictures of the yellow-flowered cacti are not good; the yellows are much too pale.

The author does not show too thorough an understanding of scientific terminology in his use of the word family, for he all too frequently speaks of a genus as a family, and calls genera races. This type of confusion of terms is a common error of many horticulturists, and makes one wish they would better inform themselves before they write upon such subjects. There are also some other nomenclatorial errors, such as listing the same species under two different genera, the use of subtribes as genera, and the use of some names that have no botanical standing. His geography is not good when he refers to the cactus tribe Opuntiae as "from America, California to Guatemala," whereas members of the genus *Opuntia* grow from Canada to the Straits of Magellan. There are also other geographical blunders and numerous errors as to the color of flowers.

In spite of these and other errors which are disturbing mostly to the specialist, the book will no doubt be a useful one for many amateurs who would like a quick resume of the general field.

EDWARD J. ALEXANDER
The New York Botanical Garden

Calculus. C. R. Wylie, Jr. x + 565 pp. Illus. $6.00. McGraw-Hill, New York. 1953.

THIS text presupposes a course in analytic geometry, covers the conventional elementary material of differential and integral calculus, and closes with an introductory chapter on differential equations. Except for the first two chapters, the material is conventionally arranged; differentiation is studied extensively before any detailed material is presented on integration. There are numerous illustrative examples and exercises, many of which involve familiar applications.

The novel first chapter, on "The Calculus in Perspective," presents a clear intuitive account of the problems of slope and area. Simple quantitative examples and applications are discussed rather skillfully.

With this promise of ulterior significance, the thoughtful student may be expected to take an active interest in the long (32 pages) and demanding second chapter on "Limits and Continuity." One misses here the essential facts on absolute value and inequalities, usually unfamiliar to students, without which even a modest ε-δ technique is impossible. Moreover, the definition given for "$f(x) \to \infty$" is really that for "$|f(x)| \to \infty$"; by the definition given, $n(-1)^n \to \infty$ as $n \to \infty$. Finally, although the notion of continuity is clearly discussed and admirably illustrated, only algebraic properties of continuous functions are listed. As a consequence, the attainment of extreme values is tacitly assumed (p. 159), the "intermediate value property" is tacitly assumed (p. 173), and an essential statement really depending on uniform continuity is offered (foot of p. 161) with inadequate evidence. (If $z_{in} = i/n$ ($i = 1, \ldots, n$), then with i fixed, $z_{in} \to 0$ as $n \to \infty$; but $Z_n = \max_i z_{in} = 1$ so that Z_n cannot approach zero.)

Minor criticisms for which argument stronger than the reviewer's prejudice can be given, involve: Occasional use of the notation $\int_a^x f(x)\,dx$ instead of, e.g., $\int_a^x f(t)\,dt$; ln x instead of ln $|x|$ as an antiderivative for x^{-1}; questionable symbolism, language, and equations in connection with the indefinite integral (pp. 184–5); use of technical terms not listed in the index (and presum-

ably undefined)—e.g., "piecewise continuous" (p. 179), "bounded" (p. 456); reference (p. 173) to an "average" height of a function with a jump discontinuity before such functions have been brought informally (p. 179) under the scope of the theory. The diagrams are generally good.

Criticisms similar to the foregoing can be urged against most calculus texts. The principal advantage of this text seems to be a pervading concern with the student's difficulties. The first chapter may turn out to be a genuine pedagogical contribution, and there are many valuable details.

F. A. Ficken

Department of Mathematics
University of Tennessee

Stewart's Scientific Dictionary, 4th ed. Previously titled *National Paint Dictionary*. Jeffrey R. Stewart. 788 pp. $10.50 Stewart Research Laboratory, Alexandria, Va. 1953.

NO work more misnamed has come to the Editor's desk in years. This is the fourth edition of what was previously called the *National Paint Dictionary,* certainly a far more appropriate title. No terms from the biological sciences are included in this "scientific dictionary" at all. Its coverage of physical terms is sufficiently apparent from the absence of "protron," "neutron," and "electron"; and its adequacy for general chemistry is revealed by the absence of "valence" and by the definition of "bond" as "the attachment at an interface between an adhesive and an adherent, or as a verb, it means to attach materials together by adhesives. The term is frequently used in the Adhesives Industry."

Within the limits of the Paint Formulary, this is perhaps a useful reference work. It has a beautiful binding.

B. G.

High Jungles and Low. Archie Carr. xvi + 226 pp. Illus. $4.50. University of Florida Press, Gainesville. 1953.

A professor at the University of Florida who spent several years at the Escuela Agrícola Panamericana in Honduras, tells of his experiences during those years in the surrounding country with enthusiasm and the keen awareness of a naturalist. The book has all varieties of subject matter in four major sections dealing with The Land, People In The Land, The Sweet Sea, and Halls Of The Mountain Cow.

Anyone interested in additional information on jungle wildlife or the people of the tropics will find this book very delightful. There is even an abundant sprinkling of common Spanish words used in Central America, particularly those that apply to the ecology of the region. One finds that they are compelled to keep referring to the adequate index so as to grasp the full understanding of the terms. After a while, this going back and forth in the book ceases and one emerges with a wealth of material.

With sympathetic understanding Archie Carr speaks wisely, "that any naturalist worth his salt—and blessed with the opportunity—will go back day after day to the weeping woods or the rain-fed *selva* alike, and wander through the dim green light they share, conditioned by the prehuman look of the draped and festooned corridors to face any strange beast whatever, but not really disappointed if he should see nothing but the forest." So many expeditions have been made into the jungles expecting to see all manner of wildlife, but the initiated scientist knows better, "forest beasts may show almost unbelievable capacity for self-effacement."

There is an excellent plea for our wasting tropics and we can only hope that this is a step nearer in helping the Caribbean countries salvage their landscapes. The sections telling of the natives and the history of the country are very pleasant, but to us the most attractive parts of the book were the adventures in tapir country when he visited the Caribbean side of Nicaragua. The author kept a journal of his month-long expedition which makes fascinating reading for he encountered numerous adventures with wildlife in many surprising ways. Added to this are his experiences in camping and association with the natives.

The introductory material in the front of the book makes good reading and the several pages of photographs at the back add considerable interest and meaning.

Lorus J. Milne and Margery J. Milne
Zoology Department
University of New Hampshire

The Yields of a Crop. W. Lawrence Balls. xv + 144 pp. Illus. + plates + charts. 21s. Spon Ltd., London. 1953.

THIS book is an abridged account of varied experimental studies that were directed toward understanding and improving the yield of the cotton crop in Egypt. It concerns itself with the effects upon that yield of the reform and development of the irrigation system from 1882 onward.

In the first part of the book, the author discusses the essential features of the provision of water. He presents a strong presumption that the water table has risen throughout most of the Delta of the Nile as a consequence of the formation and raising of the Barrage pond south of that Delta and of the high-level canals derived therefrom, and shows evidence from limited experiments that this general rise has continued and is still continuing, with the result that the soils of the Delta are now shallower than ever.

The author then proceeds to discuss the results of experiments on the effect on yield of various physiological phenomena produced by temperature, assimilation, water shortage, soil composition, insect pests, soil volume, rising water table, and season. The interpretations given are used to assess the action of the various environmental factors on the yield since 1882. He shows that over 64 years no climatic changes could account for the broad outlines of the change in yield and that the relentless downward trend in that yield during half a century after eliminating the four major variants, namely, the cotton leaf worm, the pink boll-worm, nitrog-

enous fertilizers, and differential yield of varieties, is simply "an expression above ground of the rising water table below the ground."

The author concludes that, if deep soil is again available, the yield that was brought back by modern agricultural methods toward the neighborhood of the old peak value would be susceptible to improvement. This would require, however, the reconstruction of the irrigation system with a view to lowering the water table toward an ideal of three meters depth during the summer months and minimizing the rise provoked by the flood. Otherwise, "the basis of cotton yield in Egypt will fall slowly and steadily further."

The findings and conclusions of the author should be of much interest to agriculturists, especially in irrigated areas, and to research staffs in cotton manufacturing industries the world over; and the methods used in his study could be applied with modification to other crops.

In his presentation, the author makes use to the fullest extent of graphs and illustrations of which there are 52, including one large sheet that summarizes the history of the cotton crop in Egypt and of its measurable environmental factors from 1882 onward. A valuable bibliography on the yield of cotton, together with a supplementary list of publications by the author on other aspects of that crop, are also included at the end of the book.

A. A. Moursi

Agricultural Attache
Embassy of Egypt, Washington, D. C.

Social Behaviour in Animals. N. Tinbergen. xi + 150 pp. Illus. $2.50. Wiley, New York; Methuen, London. 1953.

THE gifted author states: "This book is not intended as an exhaustive review of facts. Its aim is rather the presentation of a new, truly biological approach to the phenomena of social behaviour." This reviewer wishes the claim had been for the presentation of a "needed" rather than a "new" approach. He remembers warmly the stimulus from reading the penetrating and basically similar, though factually more limited, writings of Charles O. Whitman and of William M. Wheeler. It is an honor to Tinbergen to mention him in association with these brilliant pioneers.

The topics discussed include: mating behaviour; family and group life; fighting; analysis of social cooperation; relations between different species; growth of, and evolutionary aspects of social organization; and selected hints for research in animal sociology.

The method of presentation is mainly by the use of well selected examples on the basis of which generalizations are given or implied. It is clearly stated that many conclusions are still tentative. Sundry of the examples have been used by the author in earlier summaries, such as his "An objectivistic study of the innate behaviour of animals" (1942), and his "The study of instinct" (1951). Here they are re-used in another tapestry of evidence and interpretation.

There is so much that is timely and good about this readable small volume, and the field so greatly needs the thoughtful, provocative summaries such as Tinbergen characteristically gives, that I dislike to offer adverse criticism. There is one point, however, to which attention needs to be called.

I much appreciate the statement (p. 71) that "the American literature contains many valuable contributions on the peck-order." The next sentence needs documentation: "In many of these papers, however, peck-order is claimed to be the only principle of social organization." I try to keep abreast with the work in this field and regret my recent preoccupation with other matters, which must have prevented my seeing all the available literature since this attitude regarding any form of social hierarchy had hitherto escaped me.

The book deserves a wide audience. It will be most useful to those who are not special students of animal sociality. However, there are stimulating ideas for all. Matters I found thought provoking include the use of the releaser concept to weave plant pollination by insects and birds into social behaviour (Chapt. VI). Batesian and Müllerian mimicry are made a part of the social field by using the same device. The brief discussion of homology and of convergence in behaviour patterns (Chapt. VIII) similarly caught my attention.

In conclusion, it is a pleasure to be able publically to express my warm appreciation of Tinbergen and his work. It is perhaps not surprising that such a brilliant investigator appears not to hold himself always to read recent journal literature in a thorough and scholarly fashion.

W. C. Allee

Department of Biology
University of Florida

Outlines of Structural Geology, 3rd ed. E. Sherbon Hills. xi + 182 pp. Illus. + plates. $3.00. Wiley, New York: Methuen, London. 1953.

THAT this concise book has served a useful purpose is evidenced by its recent revision to make the third edition, thirteen years after its first appearance. The author states that he has "aimed at presenting a brief yet reasonably complete and well-documented summary of structural geology, with special reference to those aspects of the subject with which the field geologist should be acquainted." The book has been modernized by the presentation of current concepts of tectonics, which in the nature of the case are yet controversial, in order to guide thought "along profitable lines of inquiry."

The book is a convenient, lucid digest for those who wish to become familiar with the structural features of rocks, although they may find some prior understanding of the principles of geology desirable. The chapter topics are: Non-diastrophic structures, rock deformation, major crustal structures, folds, faults, structures of igneous rocks, and petrofabric analysis. Abundant references to the literature are given in footnotes. The ample index makes the book more serviceable for reference use.

Arthur Bevan

Illinois Geological Survey
Urbana, Illinois

ASSOCIATION AFFAIRS

AAAS SYMPOSIA AT THE BOSTON MEETING

SCIENTISTS well-established in their fields and young specialists alike have come to anticipate the AAAS symposia which have become an increasingly important aspect of the annual meetings of the Association. Characteristically, AAAS symposia explore relatively neglected areas of scientific inquiry or constitute up-to-date surveys of knowledge in particular fields. Such programs are significant and valuable because they focus attention upon critical areas, summarize the present status of current research, and provide positions from which to direct further research. Typically, these programs originate in the minds of the officers of the eighteen Sections and Subsections of the Association and are developed by them, either personally or by others deputized as program chairmen. Often the sectional symposia are concerned with interdisciplinary problems and are sponsored by two or more sections; participating societies also may collaborate. If the potential demand warrants it, the papers of such programs are gathered together and published by the Association as symposium volumes.

In recent years there has become well established the practice of arranging, for each annual meeting, several symposia of especially wide interest or timeliness. The responsibility for developing these general symposia, sponsored by the Association as a whole, rests upon the AAAS Symposium Committee, appointed each year by the president of the Association. Upon occasion, a proposed sectional program may be chosen to be developed as a general symposium; more commonly, the Symposium Committee, which represents a variety of scientific fields, plans and arranges these general sessions itself. The Committee may invoke the aid of consultants and appoint others to implement the general symposia.

This year the Symposium Committee consists of E. U. Condon, director of research, Corning Glass Works, and president, AAAS, (*chairman*); Frank A. Beach, professor of psychology, Yale University, and vice president for AAAS Section I; Bart J. Bok, associate director, Harvard College Observatory, and vice president for AAAS Section D; Charles D. Coryell, professor of chemistry, Massachusetts Institute of Technology; A. M. Gaudin, professor of mineral engineering, Massachusetts Institute of Technology; A. Baird Hastings, professor of biological chemistry, Harvard Medical School; Jerome C. Hunsaker, professor emeritus of aeronautical engineering, Massachusetts Institute of Technology; James R. Killian, Jr., president, Massachusetts Institute of Technology; Paul C. Mangelsdorf, director, Botanical Museum, Harvard University; Philip M. Morse, professor of physics, Massachusetts Institute of Technology; Alfred C. Redfield, associate director, Woods Hole Oceanographic Institution; Francis O. Schmitt, head, department of biology, Massachusetts Institute of Technology; Earl P. Stevenson, president, Arthur D. Little, Inc., and general chairman, Seventh Boston Meeting; George B. Wislocki, professor of anatomy, Harvard Medical School; and Raymond L. Taylor (*secretary*).

Of the general symposia at this 120th Meeting of the Association, "Species Which Feed Mankind" was suggested by Paul C. Mangelsdorf and "The Sea Frontier" was proposed originally by James B. Conant.

General Symposia

I. Species Which Feed Mankind. Arranged by Paul C. Mangelsdorf, director, Botanical Museum, Harvard University, and M. R. Irwin, professor of genetics, University of Wisconsin.

Part 1. Plant Species. 9:30 A.M., Sunday, Dec. 27; Paul Revere Hall, Mechanics Building.

Karl S. Quisenberry, assistant chief, Bureau of Plant Industry, U.S.D.A., *Presiding*

A. The World's Principal Food Plants. Karl S. Quisenberry.

B. America's Principal Food Species: *Zea mays*

1. Origin and Evolution of Maize. Paul C. Mangelsdorf.
2. The Nature of Variation in Maize. Edgar Anderson, associate director, Missouri Botanical Garden.
3. The Inflorescences of Maize. O. T. Bonnet, professor of plant genetics, University of Illinois.
4. Chromosomes, Cytoplasm, and Mutations in Maize. Marcus M. Rhoades, professor of botany, University of Illinois.

Part 2. Animal Species. 2:00 P.M., Sunday, Dec. 27; Paul Revere Hall, Mechanics Building.

Roy C. Newton, vice president, Swift & Company, *Presiding*

A. Agriculture versus Chemistry in the Nutrition of Man. Frederick J. Stare, professor of nutrition,

Harvard School of Public Health.

B. Animal Species Which Feed Mankind

1. The Role of Nutrition. L. A. Maynard, director, School of Nutrition, Cornell University.
2. The Role of Physiology. S. A. Asdell, professor of animal physiology, Cornell University.
3. The Role of Genetics. T. C. Byerly, director of animal and poultry research, Bureau of Animal Industry, U.S.D.A.

II. **The Sea Frontier.** Arranged by Alfred C. Redfield, associate director, Woods Hole Oceanographic Institution, and Jerome C. Hunsaker, professor emeritus of aeronautical engineering, Massachusetts Institute of Technology.

Part 1. 9:30 A.M., Tuesday, Dec. 29; Paul Revere Hall, Mechanics Building.

Henry B. Bigelow, curator of oceanography, Harvard University, *Presiding*

1. The Origin of Ocean Water. William W. Rubey, staff geologist, U.S. Geological Survey. (lantern, 30 min.)
2. Some Recent Results in Geology and Physical Geography of Ocean Basins. W. Maurice Ewing, professor of geology, Columbia University. (lantern, 30 min.)
3. The Sea as a Productive System. Alfred C. Redfield, Woods Hole Oceanographic Institution. (lantern, 30 min.)
4. Biological Resources of the Sea in Relation to Man. Lionel A. Walford, chief, Branch of Fishery Biology, U.S. Fish and Wildlife Service. (lantern, 30 min.)

Part 2. 2:00 P.M., Tuesday, Dec. 29; Paul Revere Hall, Mechanics Building.

Karl T. Compton, chairman of the Corporation, Massachusetts Institute of Technology, *Presiding*

1. The Edge of the Sea. Rachel L. Carson, Silver Spring, Maryland. (lantern, 30 min.)
2. The Continental Shelf. Henry C. Stetson, research oceanographer, Harvard University. (lantern, 30 min.)
3. Fresh Water from the Sea. Robert V. Kleinschmidt, consulting engineer, Boston. (lantern, 30 min.)
4. Shores and Their Sands. Frederick K. Morris, Air University and Massachusetts Institute of Technology. (lantern, 30 min.)
5. Making Ore Reserves. Antoine M. Gaudin, professor of mineral engineering, Massachusetts Institute of Technology. (lantern, 30 min.)

Sectional Symposia

B—Physics

Physics of the Upper Atmosphere (two sessions); arranged by G. G. Harvey.

Physics in Biology (two sessions); arranged by Richard S. Bear.

C—Chemistry

Comparative Nutrition Requirements of Animal Species (two sessions); arranged by Robert S. Harris and Frederick J. Stare.

Chemicals in Food (one session); arranged by Charles N. Frey.

Recent Advances in Food Technology (one session); arranged by Bernard E. Proctor.

Growth and Nutrition of Plants (one session); arranged by P. W. Zimmerman.

Chemistry of the Sea as Related to Food Problems (one session); arranged by Harold J. Humm.

D—Astronomy

Current Progress in Radio Astronomy (three sessions); arranged by Bart J. Bok.

Origin of Meteorites (three sessions); arranged by H. H. Uhlig and F. L. Whipple.

E—Geology and Geography

New England Geology (two sessions); arranged by Louis W. Currier.

Water for Industry (two sessions); arranged by Jack B. Graham and Meredith F. Burrill.

The Metropolis (one session); arranged by Victor Roterus.

G—Botanical Sciences

Second National Pollen Conference (three sessions); arranged by Stanley A. Cain.

The Uses of Large Scale Algal Cultures (two sessions); arranged by Barry Commoner.

H—Anthropology

Theoretical Models for the Study of Culture Change (one session); arranged by Evon Z. Vogt.

The Indians of New England: Their Archaeology and Ethnology (one session); arranged by Douglas S. Byers.

Nonhuman Primates and the Problems of Human Evolution (two sessions); arranged by James A. Gavan.

I—Psychology

Experimental Approaches to the Study of Brain Function (one session); arranged by Walter A. Rosenblith.

Comparative Studies of Social Behavior (one session); arranged by Burton S. Rosner.

Human Engineering and Information Theory (one session); arranged by Leonard C. Mead.

Sensory Processes (one session); arranged by Edwin B. Newman.

K—Social and Economic Sciences

The Scientist in American Society (two sessions); arranged by a committee of Section K, Conrad Taeuber, chairman, and a subcommittee of the Symposium Committee, Charles D. Coryell, chairman.

Regional Analysis (one session); arranged by Walter Isard.

The Economic State of New England (three sessions); arranged by Sumner H. Slichter and Walter Isard.

L—History and Philosophy of Science

Validation of Scientific Theories (one session); arranged by Philipp G. Frank.

Science and Its History: Three Currents of Interpretation (one session); arranged by Henry Guerlac.

Art and Science (one session); arranged by I. Bernard Cohen.

New Developments in Psychology of Thought (one session); arranged by J. S. Bruner.

Science and General Education (one session); arranged by Raymond J. Seeger.

M—Engineering

Communication Aids for the Blind (one session); arranged by Eugene F. Murphy.

Conservation of Human Resources: Highway Safety (two sessions); arranged by Irving P. Orens.

Nm—Medicine

Antimetabolites and Cancer (four sessions); arranged by Allan D. Bass and Cornelius P. Rhoads.

Nd—Dentistry

Recent Animal Experimentations in Caries Research (one session); arranged by Reidar F. Sognnaes.

Pathologic Disturbances of the Dental Pulp Resulting from Dental Operative Procedures (one session); arranged by Helmut A. Zander.

Periodontia (one session); arranged by Irving Glickman.

Np—Pharmacy

Professional Resources in Pharmacy in the United States (one session); arranged by George F. Archambault.

Accreditation of Hospitals: Its Effects on Pharmaceutical Services and Better Patient Care (one session); arranged by George F. Archambault.

O—Agriculture

Agronomic Problems of the Northeastern States (four sessions); arranged by Karl S. Quisenberry and C. E. Millar.

P—Industrial Science

Identification and Development of Senior Executives in American Industry: Contributions of Modern Science (two sessions); arranged by the Section Committee, Francis J. Curtis, chairman, and Allen T. Bonnell, secretary.

Q—Education

The Next Generation of Young Scientists and Their Science Teachers (one session); arranged by Morris Meister.

The Prediction of Child Development and Its Educational Implications (one session); arranged by J. R. Wittenborn.

Why Teachers Do and Do Not Use Films (one session); arranged by Mark A. May.

Visual Efficiency in Industry (two sessions); arranged by N. Franklin Stump.

Research on Higher Mental Processes (one session); arranged by Gerald V. Lannholm.

Symposia of the Participating Societies

Among the paper-reading sessions, seminars, and other events of the more than sixty participating societies and other organizations, are the following symposia:

American Society of Zoologists

Bioluminescence as a Tool in the Study of Cell Processes (one session); arranged by E. Newton Harvey.

Society of Systematic Zoology

The Phoronidea, Bryozoa and Entoprocta, and Brachiopoda: Their Status as Phyla and Their Relationships (one session); arranged by Waldo L. Schmitt.

Nomenclature: Results of the Sessions at the Copenhagen Congress (one session); arranged by R. E. Blackwelder and J. A. Peters.

The Subspecies versus the Cline: Their Biological and Nomenclatural Significance (one session); arranged by J. A. Peters.

American Society of Human Genetics *et al.*

Human Genetics and Medical Education (one session); arranged by Madge T. Macklin.

Genetic Factors Affecting Intelligence (one session); arranged by Frederick Osborn.

Genetics and the Races of Man (one session); arranged by William C. Boyd.

American Society of Naturalists and **Genetics Society of America**

Some Biological Effects of Radiation from Nuclear Detonations (one session); arranged by Alexander Hollaender.

National Academy of Economics and Political Science

Scientific Research and National Security (one session); arranged by Amos E. Taylor.

Society for the Advancement of Criminology

A Scientific Approach to the Problem of Delinquency (one session); arranged by Donal E. J. MacNamara.

American Association of Hospital Consultants

The Research Function of the Hospital (one session); arranged by E. M. Bluestone.

American Book Publishers Council, American Textbook Publishers Institute, and **AAAS**

Transmission of Ideas (two sessions); arranged by William E. Spaulding, John A. Behnke, and Richard H. Thornton.

Summary of Participating Organizations

With sessions of their own

AAAS Committee for Social Physics

AAAS Cooperative Committee on the Teaching of Science and Mathematics

Academy Conference

Alpha Chi Sigma

Alpha Epsilon Delta

American Academy of Forensic Sciences

American Association of Hospital Consultants

American Book Publishers Council
American Geophysical Union
American Industrial Hygiene Association
American Meteorological Society
American Nature Study Society
American Psychiatric Association
American Society of Human Genetics
American Society of Naturalists
American Society of Zoologists
Beta Beta Beta
Conference on Scientific Editorial Problems II
Conference on Scientific Manpower III
Genetics Society of America
Herpetologists League
History of Science Society
Honor Society of Phi Kappa Phi
Massachusetts Zoological Society
National Academy of Economics and Political Science
National Association of Biology Teachers
National Association of Science Writers
National Geographic Society
National Science Teachers Association
National Speleological Society
Philosophy of Science Association
Pi Gamma Mu
Scientific Research Society of America
Sigma Pi Sigma
Society for the Advancement of Criminology
Society for the Study of Evolution
Society of Exploration Geophysicists
Society of Systematic Zoology
Society of the Sigma Xi
United Chapters of Phi Beta Kappa

Cosponsors

American Academy of Arts and Sciences
American Association of Colleges of Pharmacy
American College of Apothecaries
American Drug Manufacturers Association
American Educational Research Association
American Eugenics Society
American Institute of Nutrition
American Pharmaceutical Association, Scientific Section
American Pharmaceutical Manufacturers Association
American Society of Hospital Pharmacists
American Society of Plant Physiologists, New England Section
American Textbook Publishers Institute
Association of American Geographers, New England Division
Ecological Society of America
Engineering Manpower Commission
Geological Society of America
Institute for the Unity of Science
International Association for Dental Research, North American Division
National Planning Association, New England Committee
National Research Council
National Science Foundation
New England Council
Scientific Manpower Commission

RAYMOND L. TAYLOR
Associate Administrative Secretary, AAAS

THE
SCIENTIFIC MONTHLY

DECEMBER 1953

The Origin of Stars and Galaxies

D. TER HAAR

This paper by Dr. Dirk ter Haar, like his earlier one, "The Age of the Universe," [THE SCIENTIFIC MONTHLY, 77, 173 (1953)], is based on material which will appear in a critical and historical survey he is currently preparing. In this present paper he discusses five of the principal theories on the formation of stars and galaxies.

THE scientifically minded human being is not satisfied to observe that according to astronomers stars exist and can be found together in huge agglomerations called galaxies. He immediately asks: "How were these stars and galaxies formed? When were they formed? Are they still produced?" And so on. It is the purpose of the present paper to discuss in broad outline some theories which try to answer some of these questions. Such theories are called cosmogonies since they deal with the origin or genesis of our universe, and it must be regretted that so often the term cosmology is loosely used in this connection, since cosmology is a much wider term which is concerned with the study of all aspects of the universe.[1] General cosmogonies vary enormously in scope and purpose. Some of these theories discuss in detail the formation of stars and galaxies, whereas others are content to sketch the general framework in which the formation of stars and galaxies presumably take place without considering the actual formation in any detail. We shall be mainly concerned with the first kind of theory, while a recent monograph by Bondi[2] is mainly concerned with the second kind of theory which, moreover, often uses the whole arsenal of accessible general relativity, that is, general relativity applied to homogeneous systems.* The main theories which we shall discuss are (1) Jordan's theory of continuous creation, (2) Gamow's general discussion in connection with the α-β-γ-theory[4] of the origin of the chemical elements, (3) The Bok-Spitzer-Whipple dust-cloud hypothesis, (4) Hoyle and Lyttleton's accretion theory, and (5) von Weizsäcker's general cosmogonical ideas.

At this point we would like to remind readers of two facts which we mentioned in a recent survey in this journal.[5] The first one is that some of the brightest stars in our own Milky Way are using up their nuclear fuel so fast that they must have been created during the present epoch, that is, during the last few billion years.[6] The second fact is that there are a great many independent indications that about three billion years ago "something" happened, and that the universe as we observe it has existed in practically the same state for the last

* The restriction to homogeneous systems may well be a very serious one, as even the slightest inhomogeneity may change the conclusions reached for a homogeneous system.[3]

FIG. 1. The spiral nebula M51. (Photo by Mount Wilson and Palomar Observatories.)

three billion years, but that most probably the situation before that period was completely different.

Observational Data*

Before starting our main discussion of general cosmogonies we shall give a brief survey of the observational data which are relevant to our discussion. Our Milky Way is one of the many concentrations of stars in the universe known as *galaxies*. These galaxies, or *extragalactic nebulae* as they are sometimes called, occur in different forms. Some of them seem to be ellipsoidal, ranging from practically spherical nebulae to elliptic nebulae for which the ratio of the projected axes is as high as three. A second class are the so-called spiral nebulae which consist of a central nucleus from which spiral arms start (see Fig. 1). Our Milky Way is probably such a spiral nebula. A third class is that of the barred spirals. In their case, the spiral arms start at the end of a straight "bar" which extends across the nucleus of the spiral (see Fig. 2). Finally, there are the irregular nebulae which have no definite structure at all.

We may mention in passing that our sun is situated far outside the center of the Milky Way which is an extremely happy circumstance for, if we were situated in the center of our galaxy, the whole sky would be between ten and a hundred times as bright as the sky at full moon which would seriously interfere with astronomical observations in the photographic region.

* We have taken the material for this section mainly from an earlier survey article.[7]

Galaxies often occur in groups. Thus our own Milky Way is in fact a member of the so-called local group which contains at least three spiral nebulae, six elliptic nebulae and four irregular nebulae. Larger groups are known, however, and the so-called Virgo and Coma clusters (called after their position in the sky in certain constellations) contain about one thousand galaxies. The average size of a galaxy in such a cluster is probably that of our own Milky Way, that is, such a galaxy contains a few hundred billion stars and has a radius of a few ten thousands of light years.† The radius of a cluster of galaxies is of the order of magnitude of a million light years.

In many ways our Milky Way is a rather average galaxy, and for a description of smaller concentrations of stars and of matter in general we now turn to our own Milky Way. One must, however, bear in mind that other galaxies contain the same kind of mass concentrations. In our Milky Way we have many agglomerations of matter which are not stars. For our discussion we need be concerned only with those gas masses which have no direct connection with stars, the so-called interstellar gas clouds. Between the stars there exists a large amount of matter, mainly in the form of hydrogen atoms. The density of this interstellar gas is extremely small—there are only a few atoms per cubic centimeter.‡ However, owing to the large distances in our galaxy and especially between the stars (the average distance between two stars is of the order of a few light years), the total mass of interstellar gas is practically the same as the total mass of all the stars in the galaxy.

This gas is not evenly distributed between the stars. In places its density is between ten and a hundred times larger than the average. These places of higher densities are the so-called interstellar gas clouds. These clouds extend over tens of light years, and their total mass is of the order of a few thousand times the mass of the sun. These clouds consist of two types, the so-called dark clouds and the so-called reflection or emission nebulae (Figs. 3 and 4). Figure 3 gives a picture of a part of the Milky Way which is obscured by such a dark cloud. In such a dark cloud small solid grains can form, the so-called smoke particles.§ Their presence is shown by their scattering properties. Their dimensions are of the order of the wavelengths of visible light, and

† One light year is the distance traveled by light in one year and is equal to six million million miles.

‡ Compare this with the more than ten billion billion molecules per cubic centimeter in ordinary air.

§ The term *smoke* was coined by the Dutch astronomer van de Hulst[8] to indicate the mode of formation of the solid particles.

they scatter blue light more strongly than red light, thus producing a *reddening* of the stars whose light reaches us through interstellar clouds.

The emission and reflection nebulae are gas clouds which are illuminated by neighboring stars. These stars excite the atoms, and they in turn emit radiation. Figure 4 shows such a nebula.

As far as extragalactic nebulae are concerned, elliptic nebulae do not contain interstellar clouds, but irregular nebulae and spiral nebulae do, the latter especially in their arms.

The stars in our galaxy themselves often occur in groups or clusters. The galactic or open clusters contain from a score to a few thousand stars and the distances between the numbers of such clusters are relatively large. On the other hand, the spherical or globular clusters contain at least fifty thousand stars which are very near together.

In this connection it is of interest to mention Baade's two star populations.[9] Baade found that, roughly speaking, most stars fall into two distinct groups, called stellar populations I and II. Population II contains stars like the sun which on the whole are found in the center of spiral nebulae or in elliptic nebulae and in globular clusters, and

FIG. 2. The barred spiral NGC 1300. (Photo by Yerkes Observatory.)

FIG. 3. "Horsehead" nebula in Orion. (Photo by Mount Wilson and Palomar Observatories.)

which generally rotate slowly.* Population I taken by and large contains stars brighter than the sun, which are found in spiral arms and open clusters. Their rotation is often fast.

Continuous Creation?

Most general cosmogonies make the basic assumption that during the last few billion years, which is the period with which these cosmogonies are concerned, the laws of nature have been the same as we assume them to be at the present and that also the total mass of the universe has not changed during this period. In this case one is dealing with a more or less well-defined problem with well-defined rules which have to be observed during the solution. In Hoyle's theory of continuous creation of matter, however, one introduces necessarily more or less ad hoc† new concepts and one investigates their consequences.

Unfortunately the theories of Hoyle[10] and Bondi and Gold[11] are so bound up with general relativity that it is impossible to give a qualitative picture of these so-called steady-state theories beyond the remark that in these theories matter is created at all times. One might ask how it is possible to bring these theories, which have no definite "starting time" for the universe, in line with the great amount of evidence to the fact that about three billion years ago "something" happened.[5]

A cosmogony which to some extent is related to these theories is the one developed by Jordan,[12] in

* Our sun, although belonging to population II, is a slight exception in so far as it is not situated in the center of the Milky Way but well outside, a position which is not characteristic of stars of the spectral type of the sun.

† It is in this connection of interest that the term introduced by Hoyle in the equations of general relativity, which is responsible for the creation of new matter, was very carefully discarded by Einstein when he formulated these equations.

FIG. 4. The emission nebula 12 Mon. (Photo by Yerkes Observatory.)

so far as also in Jordan's cosmogony matter is created continuously, but in this case only for the last few billion years and not since time immemorial as in the steady-state theories. The starting point of Jordan's ideas are two papers by Dirac,[13] which we may discuss briefly.

Dirac notes that there is strong experimental evidence that the mass and charge of an electron, the velocity of light, the mass of a proton (or of a hydrogen atom), and the quantum of action are real constants, that is, are independent of time. Using these universal constants, we can construct by dimensional analysis an elementary unit of time.* If we divide the age of the universe by this unit we get a pure number, that is, a dimensionless quantity, which is very large, namely about 10^{40}, that is ten thousand billion billion billion billion.

Another dimensionless quantity can be found by taking the ratio of the electric force between a proton and an electron to the gravitational force between these two particles. Once again we get a large number equal to about 10^{40}. The fact that these two numbers which involve completely different quantities are nevertheless of the same order of magnitude led Dirac to the following principle, *"Any two of the very large dimensionless numbers occurring in Nature are connected by a simple mathematical relation in which the coefficients are of the order of magnitude unity."*

If one accepts the principle as fundamental, it follows that the gravitational constant should be inversely proportional to the age of the universe, since of the two large numbers which we discussed earlier the first one is proportional to the age of the universe and the second one inversely proportional to the gravitational constant.

By a similar argument Jordan shows that (1) the total mass of our universe is proportional to the square of the age of the universe and that (2) its radius is proportional to the age itself (expanding universe!).†

Having arrived at an increasing mass of the universe, Jordan continues his considerations by discussing the formation of stars. This part of his cosmogony is rather sketchy and open to criticism, but certainly provides a fascinating speculation on the formation of stars. Jordan assumes that stars are born spontaneously (*Spontanentstehung*) and shows that a spontaneous birth of matter will lead to bodies with masses of the order of magnitude of the solar mass, provided this creation leaves the total energy of the universe zero. The birth cries of such new stars are identified with the so-called supernovae. A supernova is a star which suddenly produces an enormous brightness—much larger than the brightness of an ordinary star like the sun.

Gamow's Cosmogonical Ideas

Gamow, Alpher, and Herman[4] have developed a detailed theory to account for the fact that so many different chemical elements exist. In their theory, which we cannot discuss here, they assume that about three billion years ago the density of our universe was much higher, its decrease being due to the expansion of the universe, which is governed by the equations of general relativity. During this expansion density fluctuations may occur and these may lead to "condensations," that is, a region of slightly higher density may retain this higher density or even increase its density, so that the universe which originally was homogeneous will become inhomogeneous and populated by mass concentrations. These mass concentrations are

* This unit is the time light takes to travel a distance which is equal to the so-called classical electron radius.

† It is interesting to note that if these relations are true, the total kinetic energy and the total potential energy (which has a negative sign) of the universe are approximately equal, so that one is tempted to assume that the total energy content of the universe is exactly equal to zero.

the so-called proto-galaxies, that is, from them the galaxies are developed.

A formation of mass concentrations from density fluctuations is called the process of *gravitational instability*.[14] One can see how it can become important. A mass of gas will expand into space, if its temperature is so high that the kinetic energy of the gas particles is larger than their potential energy in the gravitational field of the gas mass itself.* The larger the total mass, the larger are its retentive powers, but small masses will disperse.

Gamow[15] has shown that the smallest gas masses which can form a stable concentration of mass are of the right order of magnitude, that is, of the order of the mass of our Milky Way. He has not, however, developed this cosmogony any further and, for instance, has not considered the formation of stars.

The Dust-Cloud Hypothesis

We mentioned earlier that some interstellar clouds contain small solid particles, the so-called smoke particles. These smoke particles will be subject to radiation pressure from the star. In the same way as the bombardment by gas particles on a wall of a container produces the ordinary gas pressure, the bombardment by light quanta will produce the so-called radiation pressure. In most circumstances radiation pressure is much smaller than gas pressure, but in interstellar space, the density of the gas is so small that radiation pressure will become important. The first effect of radiation pressure on smoke particles is that it will blow together smoke clouds, that is, interstellar clouds containing smoke particles, to form regions of much higher density.[16] Such regions of high density can sometimes be seen as small globular dark objects, and some of them have been studied by Bok,[17] who sees these as the first stage of star formation. The further development of such dark clouds has been studied by Whipple[18] and Spitzer.[19]

This development takes place in three stages. In the first stage the radiation pressure continues to sweep smoke particles together. Figure 5 shows how this may happen. Smoke particle I will cut out some of the radiation which otherwise would have fallen on particle II from the left. This shadow effect will have as a result that the radiation pressure on particle II will not be uniform, but will have a resultant component in the direction of particle I, thus pushing particle II toward particle I. This "mock gravity" force, as it is sometimes called, will in the first stage of the development of the dark clouds be much larger than the gravitational force between the two particles. As a result the smoke particles in the dark cloud will "gravitate" toward the center.

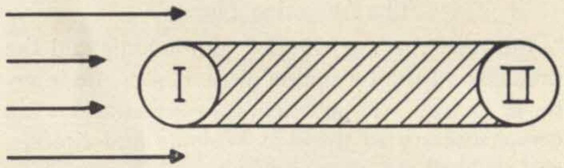


Fig. 5

The first stage will end when the cloud has become opaque to stellar radiation. The concentration will slow down until in its last stage gravitational capture by the central mass conglomeration will again produce a fast increase in mass.

Although on the one hand the theory leads to mass concentrations of the right order of magnitude, that is, with masses of the order of the solar mass, and on the other hand dark clouds such as occur in this theory have been observed, there are many reasons why one must be extremely wary in accepting the smoke cloud origin of stars as the only, or even the predominant, method of star formation. It seems far more likely that the processes studied by Whipple and Spitzer will play an important role in a more general cosmogony such as the one presented by von Weizsäcker, but that by themselves they very seldom will lead to the formation of a star. Some of the reasons for this statement are the following ones, first given by Whipple. A quantitative analysis of the process shows (1) that relative velocities in the cloud must be extremely small since otherwise the process will first of all be retarded and secondly is likely to become impossible because of increasing rotation of the proto-star† and (2) that it is doubtful whether the brightest stars in our galaxy can be produced in this way.‡ How far these difficulties can be surmounted by taking into account the influence of electromagnetic fields, as suggested by Spitzer, remains to be investigated.§

* Compare a similar situation arising in the case of planetary atmospheres. The moon could not retain an atmosphere, since its mass and hence its gravitational field is not sufficiently large.

† Since angular momentum must be conserved, the rotation of the cloud will increase during its contraction. This will lead to large centrifugal force which will act against the forces producing the concentration of matter toward the center.

‡ As soon as the central mass begins to resemble a star, it will also begin to radiate and the ensuing radiation pressure will prevent a further agglomeration of mass.

§ In a different connection it has been suggested[20] that such effects can be important in the slowing down of stellar rotation.

The Accretion Theory

Just after the war, Hoyle[21] also investigated the possibility of the formation of stars from the interstellar gas. To a large extent his considerations are complementary to those of Whipple and Spitzer, and we shall therefore discuss them separately. As Hoyle does not take into account the influence of turbulence in the interstellar medium, many of his conclusions must be revised, if not qualitatively at least quantitatively.

Hoyle's investigations fall into several parts. First of all, he shows that the interstellar gas substratum in a galaxy will be concentrated to the equatorial plane of the galaxy, that is, the plane through the center perpendicular to the axis of rotation.* From the observational data about the total mass of a galaxy and its mass distribution, Hoyle is able to estimate the density of the interstellar medium in an average galaxy such as our Milky Way. He then considers the evolution of a region where the density is accidentally higher than the average density, in much the same way as Whipple starts his considerations. Hoyle shows that the larger the mass of the proto-star, the faster *the rate of concentration toward the center* will be. However, this will mean that large mass concentration will stand a very large chance of being disrupted by rotational instability. We can see this as follows. A large concentration of mass at a certain distance from the center of the galaxy will usually be rotating because the parts nearer to the center of the galaxy will move around this center at a higher angular velocity than those parts which are farther removed.† This means that the cloud possesses angular momentum, which must be conserved during its contraction. This conservation can be attained only if the mass increases its rate of rotation and, in turn, this can happen only as long as the centrifugal force due to this rapid rotation is smaller than the gravitational force which is holding the mass together. For large masses the concentration is so fast that the centrifugal forces will, indeed, in most cases break up the proto-star before a real star is born. Hoyle sees this as the reason that in our galaxy most of the stars are smaller than the sun and that very few are larger.

In the case of smaller proto-stars, the concentration is much slower, and Hoyle considers it possible that the proto-star while moving through the galaxy can sweep up a sufficiently large amount of material without angular momentum to counteract the centrifugal instability and at the same time to provide most of the matter of the final star.‡

In this framework Hoyle and Lyttleton[22] have also considered the formation of binaries. They suggest that binaries are formed through chance encounters between two stars whose mean distance apart is decreased through accretion of interstellar matter.

von Weizsäcker's General Cosmogony

If we do not wish to leave the realm of present-day physics, as is done by Jordan, and at the same time wish to consider cosmogony in the framework of classical physics, that is, without using general relativity, we still can arrive at a picture of the formation of stars and galaxies. This was recently shown by von Weizsäcker,[23] who drew attention to the importance of turbulence and rotation in our universe. As we think that his considerations are probably the most worth while of all recent general cosmogonies, we shall discuss his ideas in slightly more detail than we have done with the other cosmogonies.

As working hypotheses von Weizsäcker uses the following assumptions. (1) Stars and galaxies have been formed during the present epoch, that is, during the last few billion years, and are still being formed. (2) All the laws of physics have been the same as at present during that period. (3) The gas filling the universe was, and is, composed of the chemical elements with the same relative abundances as we find at present.§ (4) Different parts of the gas had large relative velocities. von Weizsäcker assumes that one must look for the origin of these relative velocities which may according to him be connected with the expansion of the universe to periods before the present epoch. Gamow,[25] on the other hand, has remarked that the possibility that our universe as a whole is rotating should not be excluded a priori. This rotation might thus be the reason for the initial turbulence.

von Weizsäcker starts his considerations by dividing all celestial bodies or collections of bodies into three groups according to their degree of

* Hoyle actually finds a concentration toward this plane which is much stronger than the concentration of the stars in the galaxy, but this conclusion seems to be no longer valid, if turbulence is taken into account.

† Compare similar considerations in the discussion of the age of star clusters.[5]

‡ This last conclusion of Hoyle's seems to be rather ill founded. A quantitative analysis of the process seems to indicate that accretion can account only for a very small proportion of the mass of the final star.

§ This hypothesis is really irrelevant to von Weizsäcker's discussion, and it can be shown[24] that it is probably not a necessary assumption either as there are probably processes in nature which have been active during the present epoch and which will produce a relative abundance distribution of the chemical elements such as we find at present.

rotational symmetry. Group I comprises those structures which show spherical symmetry; these structures are either non-rotating or very slowly rotating. Group II contains all structures with rotational symmetry. The irregular bodies form group III. Typical examples of group I are the spherical star clusters, stars like the sun and smaller (Baade's population II), the earth, and the other planets. Typical examples of group II are the elliptic and spiral nebulae, the bright stars (Baade's population I), binary systems, planetary systems,* satellite systems, and Saturn's rings. Irregular nebulae, star clouds, open star clusters, and interstellar gas and smoke clouds belong to group III. All the systems in group III show "cloudiness"; some of the systems in group II show cloudiness, but others do not; none of the systems in group I is cloudy. This cloudiness points to turbulence, as can be seen from the following. Cloudy structure means fluctuations in density and hence in pressure. Thus currents will be set up to counteract this cloudiness, and it is only possible to retain a cloudy structure, if the currents correspond to a turbulent state, since otherwise the situation would approach an equilibrium condition where there are no density fluctuations—hence no cloudiness. It turns out that one can also conclude from dimensional considerations that systems in which interstellar gas is present will be in a turbulent state.† However, one can show that a system such as a spherical nebula, which does not contain any interstellar gas but only stars, will not show turbulence.‡

Let us now consider the development of a universe filled with a turbulent gas. Owing to the turbulence, some eddies, or turbulence elements, will be regions of slightly higher density and may act as a potential hole, or trap because of the slightly larger gravitational field strength inside. Particles entering this potential hole will gain energy because of the difference in gravitational field strength. This gain of energy should enable these particles to leave the turbulence element again, were it not for the interaction with the material in the element through which they will lose their excess energy.§ It thus seems possible that such a turbulence element may grow in mass, and that in this way the universe may be divided into many rotating subsystems which we shall call proto-galaxies.

Inside such a proto-galaxy there are again turbulence elements, and a process similar to the one leading to the proto-galaxies may take place, leading to smaller rotating subsystems, say proto-clusters. This process will go on repeating itself as long as turbulence exists inside the proto-bodies, and in successive steps proto-stars, proto-planets, and proto-satellites may be formed.

One must bear in mind, of course, that the picture given here is extremely simplified. In reality there may be many more steps, and the steps will proceed simultaneously and more or less independently. One of the intermediate steps may, for instance, be the formation of proto-clusters of galaxies corresponding to systems such as the Virgo cluster.

Before we discuss the further development of such rotating proto-systems we may consider in slightly more detail the formation of the sequence of proto-systems of decreasing size. In general, we may expect that the linear dimensions of a turbulence element will be about an order of magnitude smaller than the dimensions of the whole system. The mass of a proto-system will then be smaller than the mass of the preceding proto-system by at least a few orders of magnitude, say, by a factor of about a hundred to a thousand. This mass ratio will be preserved, if in the subsequent developments essentially the same fraction of the mass is lost. It is therefore perhaps not irrelevant that a cluster of galaxies contains about one thousand galaxies, that a galaxy contains about a million times as many stars as a star cluster, and that a star cluster contains about a hundred thousand stars,‖ and that the sun's mass is about one thousand times the mass of Jupiter, and Jupiter's mass about ten thousand times that of its largest satellite.

* Although the sun, the earth, and other planets themselves belong to group I, since they are practically speaking spheres, the planetary system taken as a whole has only rotational and not spherical symmetry and thus belongs to group II.

† One must calculate the so-called Reynolds number, which is a dimensionless quantity measuring the ratio of the shearing forces due to velocity differences and the viscous forces which tend to level out velocity differences. If the Reynolds number is larger than a certain critical value, turbulence will occur, but if it is smaller than its critical value, the movement will be laminar, that is, smooth.

‡ Such a system can be compared to a so-called Knudsen gas. The mean free path of the stars, that is the distance over which a star will travel before it encounters another star, is large compared with the dimensions of the system.

§ Compare a similar situation which arises in the formation of the compound nucleus in nuclear reactions where the incident particle also loses its excess energy through interactions with the nucleons in the target nucleus.

‖ If we should take our rule of a factor of a thousand between successive stages to be universally valid—for which there are no good reasons—we should expect that between a galaxy and a star cluster, and between a star cluster and a star, a system is missing.

A completely simultaneous formation of all proto-systems will, however, not be realized. The size, and thus the mass, of the smallest turbulence element is determined by the density in the system.[26] Taking for the density the overall density in our universe we find for the mass of the smallest turbulence element a mass of the order of magnitude of the mass of our galaxy. Inside the proto-galaxy the motion will not be turbulent, but laminar. In this picture we get thus the simultaneous formation of proto-clusters of galaxies and proto-galaxies, but not of proto-clusters, proto-stars, and so on. These will be formed only as soon as the density through concentration of matter has sufficiently increased to allow turbulence to be set up inside the proto-galaxy. When the density has reached a value equal to the present density in our galaxy, the smallest turbulence element has just about the mass of a star cluster. The mass of a star is reached for densities such as are found in dense interstellar clouds, and so on.

Let us now consider the development of a rotating proto-system. The rotation of a system is the remnant of the angular momentum of the turbulence element. The only rotational motion of a gaseous system which does not consume energy through viscous interaction is the rotation of a rigid body where all points have the same angular velocity. The loss of energy, which as we shall see presently entails dissipation of the system, will cease only when the state of rigid rotation has been attained. However, a free gas mass, such as a proto-system will be, will not rotate rigidly, since the rotational velocity will be determined by the gravitational field. In general, in a proto-system there will be a concentration of mass toward the center, and the rotational velocity will decrease with increasing distance from the center. Also, the centrifugal forces which are acting on a rotating mass will tend to flatten the system, and it can be shown that, indeed, a proto-system will have a lens rather than a spherical shape corresponding to the observed shape of most galaxies.

Once some concentration toward the center has taken place, subsequent developments will accentuate it. As the velocities decrease when we move away from the center, the outer regions will try to slow down the inner regions through viscous interaction. This will result in two concomitant effects. (1) The rotation of the central part will be decelerated, thus decreasing its angular momentum and postponing the moment at which rotational instability will occur. (2) Part of the outer mass will fall into the center, thereby gaining sufficient gravitational energy to allow other parts to disappear from the system, taking with them angular momentum.

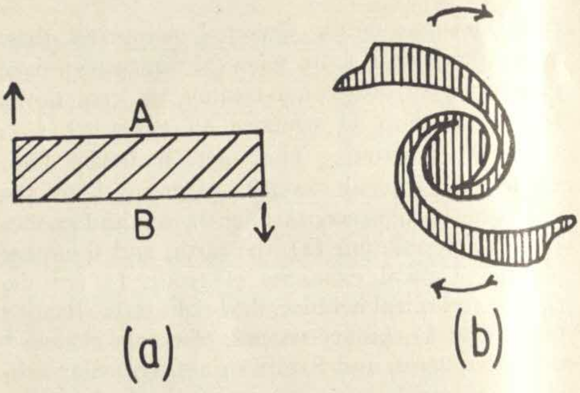


Fig. 6

One can ask how long it will take a system to become reduced to its central concentration. von Weizsäcker[23] has shown that this period is essentially equal to the time it takes a turbulence element in the system to wander from the edge of the system to its center. This same period is also approximately the time during which the proto-system will lose most of its rotation. If this period is long compared with the length of the present epoch, which is a few billion years, the system will not yet have lost much of its rotation, and we shall call it a *young* system. If, on the other hand, the period is small compared with the length of the present epoch, the system will have lost most of its rotation, and we shall call such a system an *old* system. We have thus found a first criterion for the age of a system. *The faster the rotation, the younger the system.* Of von Weizsäcker's three groups of systems mentioned a little while ago, group I contains the oldest and group III the youngest objects. We must emphasize that the age of a system as defined here is measured in its own time scale. Of two systems which were formed at the same time, one may be young and the other old. Some systems age more rapidly than others, so to speak, and the rate of aging is determined by the rate of dissipation.

As far as extragalactic nebulae are concerned, von Weizsäcker[23] has shown that both the criterion of rotation and the criterion of the ratio of time of dissipation and length of the present epoch lead to the conclusion that spherical nebulae are the oldest, the elliptic nebulae come next, then the spiral nebulae and finally the irregular nebulae.*

* It is amusing to note that owing to now obsolete ideas of the evolution of extragalactic nebulae, elliptic nebulae are called early type galaxies, and spiral and irregular nebulae late type galaxies, since it was assumed that elliptic nebulae would develop into spiral nebulae.

Another criterion for age is the absence or presence of interstellar gas. In the early stages of the evolution of a galaxy interstellar gas will be present, and stars with extended envelopes will continually interchange matter with their surroundings.[27] In later stages, however, all or practically all of the gas will have disappeared from the system or have been swept up by the stars. This criterion also indicates that elliptic nebulae are old and irregular nebulae young systems.

We may mention here that the spiral structure of galaxies easily follows from the rotation of an irregular cloudy system.[28] To illustrate this we have drawn Fig. 6. Part (b) of this figure shows the result of a rotation of a rectangular mass (a), if the angular velocity decreases with increasing distance from the center. The situation (b) is reached after the points A and B in (a) have completed one complete revolution around the center and the formation of two spiral arms is clearly shown.

Let us now consider the age of stars, again, of course, with respect to their own time scale. We saw earlier that bright stars, the so-called early type stars, are on the whole rotating at a fast rate, while the not so bright stars, the so-called late type stars, show little or no rotation.* We may thus conclude, and this conclusion is borne out by a further discussion as we shall see, that early type stars are young, and late type stars old.

If our classification of stars is correct, we should expect early type stars to be found mainly in spiral and irregular nebulae, while the nuclei of a spiral nebula and elliptic nebulae should contain mainly late type stars. This is, indeed, the case. Furthermore, spherical or globular star clusters contain mainly late type or old stars while open clusters contain young stars. This again is in agreement with the general picture that group I should contain old systems and group III young systems. Finally, we may remind ourselves that the luminosity of early type stars is so great that they are using up their nuclear fuel so fast that their lifetime can be only a few million years, whereas late type stars have a much smaller energy output.[5, 6] Once again we are led to assign the label "young" to the early type stars and the label "old" to the late type stars. We see thus that the various age criteria form a consistent set and that in general it is possible to assign uniquely a system to an old or a young group.

We may remark here that if stars are formed from proto-stars which are turbulence elements in a turbulent proto-galaxy, we should expect a correlation between the mass distribution among the various types of stars[30] and the mass distribution among the various sizes of turbulent eddies.[32] Such a correlation does, indeed, exist.

In conclusion we may consider briefly the fate of the early type stars. We saw that, on the one hand, they were rotating rapidly and, on the other hand, they were using up their hydrogen, which is their nuclear fuel, at a fast rate. One might ask what will happen first. Will they lose their rotation before exhausting their hydrogen, or will they exhaust their hydrogen before their rotation has disappeared? von Weizsäcker concludes that both cases are possible. In the latter case the star will first contract after its hydrogen is exhausted, in order to obtain the energy which it is losing through radiation from its own gravitational field. This contraction will proceed only as long as the centrifugal force at the equator of the star due to its rotation is smaller than the gravitational force. Because of the conservation of angular momentum the centrifugal force will increase during the contraction and an explosion may take place.† This explosion might well be similar to the so-called supernova phenomenon where a star is suddenly seen to increase its luminosity enormously. It is also possible that during the explosion heavy nuclei are formed, since the density in the star just before the explosion may well be of the order of nuclear densities and thus sufficiently high for the formation of heavy nuclei. A detailed discussion of this process would, however, lead us too far, and we therefore refer the reader to the literature.[4, 7, 24, 33]

If stars lose their rotation before exhausting their hydrogen they will not die young, but become old stars, that is, they may become late type stars. We may briefly consider how stars can lose their angular momentum.[20] The following mechanisms have been suggested: (1) accretion of matter with zero angular momentum, (2) slowing down of the rotation through viscous interaction inside the proto-star, (3) electromagnetic braking, and (4) the formation and shedding of extended gaseous shells. von Weizsäcker considers the last mechanism to be

* We refer the reader to Struve's work[27, 29] for a thorough discussion of stellar rotation and its cosmogonical significance.

We restrict ourselves in our discussion to the so-called main sequence stars. The main sequence stars form about 95 per cent of all stars in the neighborhood of the sun.[30] Of the remaining 5 per cent about 1 per cent is giants, about 3 per cent white dwarfs, and about 1 per cent subdwarfs. We refer the reader to astronomical textbooks such as Russell, Dugan, and Stewart's[31] for a discussion of the various types of stars. As far as our discussion is concerned, early type stars coincide with Baade's population I and late type with his population II.

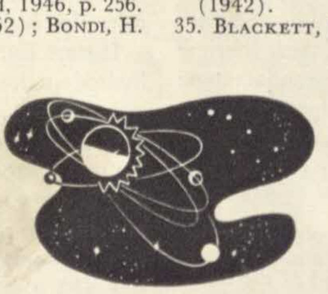
† It must be remarked that the contraction is a rapid dynamic process and not a slow development through equilibrium configurations.

responsible for the slowing down of the rotation, but the process cannot easily be analyzed quantitatively, and it remains therefore at the moment an open question whether or not this mechanism can be sufficient. We mentioned earlier that the total amount of material captured by accretion is much smaller than the original mass of the star, and it follows that in that case the angular momentum cannot appreciably be changed during the lifetime of an early type star. The second mechanism is also insufficient, as one can show by a quantitative analysis, and one is led to investigate electromagnetic processes of the kind suggested by Alfvén.[34] We assume with Blackett[35] that a rotating star will also possess a magnetic moment and we consider the influence of the magnetic field of the star on passing interstellar clouds. The radiation from the star will ionize the atoms in the cloud and owing to the magnetic field of the star ion currents will be set up in the cloud. These currents will tend to slow down the rotation of the magnetic field, that is, they will reduce the rate of rotation of the star. A rough calculation of the accumulative effect of encounters with interstellar clouds leads to the very tentative conclusion[20] that this electromagnetic effect may well have produced the slow rotation of late type stars.

In summarizing we may say that there have been lately a number of attempts to produce a general cosmogony and that probably von Weizsäcker's ideas have the best chance to lead to a more detailed understanding of the formation and evolution of stars and galaxies. It must, however, also be stressed that this cosmogony is only in its early stages and that of most of the processes only an overall picture has been given, while a detailed analysis has not yet been made.

References

1. *The Concise Oxford Dictionary,* Oxford, 1946, p. 256.
2. Bondi, H. *Cosmology.* Cambridge (1952); Bondi, H. *Monthly Notices Roy. Astron. Soc.,* **108,** 104 (1948).
3. Omer, G. C., Jr. *Astrophys. J.,* **109,** 164 (1949).
4. Alpher, R. A., Bethe, H. A., and Gamow, G. *Phys. Rev.,* **73,** 803 (1948); Alpher, R. A., and Herman, R. C. *Revs. Mod. Phys.,* **22,** 153 (1950).
5. ter Haar, D. *Sci. Monthly,* **77,** 173 (1953).
6. Unsöld, A. *Z. Astrophys.,* **24,** 278 (1948).
7. ter Haar, D. *Revs. Mod. Phys.,* **22,** 119 (1950).
8. ter Haar, D., *et al. Nederland. Tijdschr. Natuurk.,* **10,** 238 (1943).
9. Baade, W. *Astrophys. J.,* **100,** 137 (1944).
10. Hoyle, F. *Monthly Notices Roy. Astron. Soc.,* **108,** 372 (1948); **109,** 365 (1949).
11. Bondi, H., and Gold, T. *Ibid.,* **108,** 252 (1948).
12. Jordan, P. *Physik. Z.,* **45,** 183 (1944); **45,** 233 (1945); *Acta Phys. Austriaca,* **2,** 356 (1949); *Nature,* **164,** 637 (1949).
13. Dirac, P. A. M. *Ibid.,* **139,** 323 (1937); *Proc. Roy. Soc.* (*London*), **A165,** 199 (1938).
14. Jeans, J. H. *Astronomy and Cosmogony.* Cambridge (1929); Chandrasekhar, S. *Proc. Roy. Soc.* (*London*), **A210,** 26 (1951).
15. Gamow, G. *Nature,* **162,** 680 (1948); *Proc. Danish Acad. Sci.,* **27,** no. 10 (1953).
16. Spitzer, L. *Astrophys. J.,* **94,** 232 (1941).
17. Bok, B. J. *Harvard Observatory Monographs,* **7,** Ch. 5 (1948).
18. Whipple, F. *Astrophys. J.,* **104,** 1 (1946).
19. Spitzer, L. *Harvard Observatory Monographs,* **7,** Ch. 7 (1948).
20. ter Haar, D. *Astrophys. J.,* **110,** 321 (1949).
21. Hoyle, F. *Monthly Notices Roy. Astron. Soc.,* **105,** 287, 302, 363, 381 (1945); **107,** 334 (1947).
22. Hoyle, F., and Lyttleton, R. A. *Proc. Camb. Phil. Soc.,* **35,** 592 (1939); **36,** 325, 424 (1940); *Monthly Notices Roy. Astron. Soc.,* **101,** 227 (1941).
23. von Weizsäcker, C. F. *Z. Astrophys.,* **24,** 181 (1947); *Astrophys. J.,* **114,** 165 (1951); *Festschrift Akad. Wiss. Göttingen* (1951), 86.
24. ter Haar, D. *Science,* **109,** 81 (1949).
25. Gamow, G. *Nature,* **158,** 549 (1946).
26. ter Haar, D. *Z. Astrophys,* **32,** 251 (1953).
27. Struve, O. *Stellar Evolution,* Princeton, N. J.: Princeton Univ. Press, 1950.
28. Heisenberg, W., and von Weizsäcker, C. F. *Z. Physik.,* **125,** 290 (1948).
29. Struve, O. *Popular Astronomy,* **53,** 201 (1945).
30. Kuiper, G. P. *Astron. J.,* **53,** 194 (1948).
31. Russell, H. N., Dugan, R. S., and Stewart, J. Q. *Astronomy,* Vol. II, Boston, 1938.
32. Onsager, L. *Phys. Rev.,* **68,** 286 (1945).
33. Hoyle, F. *Monthly Notices Roy. Astron. Soc.,* **106,** 343 (1946).
34. Alfvén, H. *Arkiv Mat., Astron. Fysik.,* **28A,** no. 6 (1942).
35. Blackett, P. M. S. *Nature,* **159,** 658 (1947).



Homeostasis Versus Hyperexis: or Saint George and the Dragon*

DICKINSON W. RICHARDS†

Dr. Richards completed his undergraduate work at Yale University, and received his M.A. (1922) and his M.D. (1923) from Columbia University. He is visiting physician and director of the First (Columbia) Medical Division, Bellevue Hospital, and Lambert Professor of Medicine at Columbia University. He has carried on research in the physiology of respiration and circulation in man, and from 1942 to 1945 made studies of the circulation in wound shock in man. He is especially concerned with catheterization of the heart.

OF the terms in this title, homeostasis is one that you know about. Hyperexis is new, a word that comes from the *Timaeus* of Plato: I will have more to say about it later. Saint George and the Dragon is a bit of obvious imagery.

Homeostasis

There is a remarkable book, published twenty-odd years ago, called the *Book of Christopher Columbus.*[1] The author was Paul Claudel. It is at once a picture book, a play, a history, and an allegory. There is one scene in it where Columbus is being brought back to Spain, this time as a prisoner in chains in the ship's hold, because he had failed to find the gold of the Indies which he had promised. With him is the Ship's Cook, a friendly fellow. Columbus has been boasting of his prowess. The Cook says: "Are you to make a new heaven and a new earth?" Columbus says, "My business is not to remake the world but to discover it." The Cook says, "The world is wicked." Columbus says, "So much the worse for it, then, for I shall unveil it."

The notion which I should like to convey in this paper has to do with biological wickedness as applied to man. It is my thesis that in our thoughts about living processes, even in spite of our constant preoccupation with disease, we still give too much credit to the powers of good, and too little to the powers of evil, anthropocentrically speaking: too much, that is, to living and recovery, and too little to dying and death-dealing influences.

More specifically, I argue that the concept of homeostasis, as developed by Walter Cannon, useful and sound though it is, has now too strong a hold upon us, so that it seems almost to have possessed all our physiological thinking. Other balancing concepts are needed.

From much of the physiological talk that one hears, one might think that the Saint George of homeostasis always prevails, and that the dragons of disease are always in the end destroyed.

One should appreciate, from the beginning of our argument, that the notion of homeostasis, as developed by Cannon, was intended to have a broad meaning and broad implications. After defining homeostasis as the maintenance in the animal body of steady states, by coordinated physiological processes, Cannon[2] goes on to say (*Wisdom of the Body*‡), "It seems not impossible that the means employed by the more highly evolved animals for preserving uniform and stable their internal economy (i.e., for preserving homeostasis) may present some general principles for the establishment, reg-

* Paper read at a meeting of the Practitioners' Society, New York City, December 12, 1952. Some changes have been made from the paper as originally given.

† The writer wishes to express his indebtedness and gratitude to Alfred R. Bellinger for suggesting the passage in the *Timaeus,* and for some greatly valued criticism; and to Harold G. Wolff and Rene Dubos (members of the Practitioners' Society) for information and helpful comment about Claude Bernard.

‡ Page 24.

ulation, and control of steady states, that would be suggestive for other kinds of organization—even social and industrial—which suffer from distressing perturbations."

It is of interest to inquire what are the origins of the attitude represented by this term homeostasis.

In searching for the beginnings of an idea, one can go into history as remotely as one pleases, and find some sort of analogy almost as far back as the written word can be traced. To my eye, however, the climate in which the atmosphere of homeostasis first appeared was the eighteenth century. I am now thinking of the implicit attitude rather than the explicit idea. Whitehead has said[3] that the Middle Ages were the Age of Faith, based on reason, the eighteenth century the Age of Reason, based on faith. The faith of the eighteenth century was indeed boundless, faith in a rational plan of the universe, presided over by a beneficent Deity. If philosophers would just get their heads together, so they opined, this plan could be understood, and everything worked out to the best interests of all. Voltaire[4] poked his long satiric finger at them, but their faith was unshaken. If everything was not yet for the best, in the best of all possible worlds, it soon would be.

"Life, liberty, and the pursuit of happiness." Was there ever a fine phrase more completely indigenous to the century of its origin? As if happiness were ever attained by pursuit. But one should not press this too closely. As a preamble the phrase is magnificent. The trouble is, it soon became a philosophy.

With the nineteenth century, the moralists, the poets, and others, found increasing difficulty with the mechanistic approach, but in the sciences, determinism pursued its way triumphant; and why should it not have done so, considering the vast achievements derived therefrom? In most of the sciences the same philosophy still prevails, though in physics, if I understand rightly, strict mechanism has faltered somewhat of late, and natural philosophers are no longer secure. It is not our purpose, however, to pursue this, even if we could, but to concentrate on one aspect only of this historic trend.

Halfway between the end of the eighteenth century and the present day rises a gigantic figure, perhaps the greatest of all physiologists, Claude Bernard. It was by him, as everyone knows, that the general concept, of which homeostasis is one immediate descendent, was explicitly declared.

Bernard[5] had no doubts about the validity of determinism in science nor about the central place of physiology in its study. "There is," said he, "an absolute determinism in every phenomenon of life." He continues, "General physiology is the fundamental biological science, toward which all others converge. Its problem consists in determining the fundamental conditions of life's phenomena. Pathology and therapy rest equally upon this common base. It is by the normal activity of organic elements that life manifests itself in the state of health; it is by the abnormal manifestations of the same elements that diseases are characterized."

Best known of Bernard's physiological principles is that of the maintenance of the internal environment, and he himself was frank to say that he was the first physiologist to state clearly this concept.[5] But it is not alone the constancy of this internal environment that interests him. It is equally the reactivity of the internal environment with the external. "The organism," he wrote, "is only a living machine constructed in such fashion that, on the one hand, there is free communication between the external environment and the organic milieu interieur, and on the other, that there are protective functions of organic elements holding living materials in reserve and maintaining without interruption humidity, heat, and other conditions indispensable to vital activity. Sickness and death are only a dislocation or perturbation of that mechanism."

Of the many students and commentators of the work of Claude Bernard, probably the most influential in the English speaking world have been: Lawrence J. Henderson, in his two notable essays, *The Fitness of the Environment*[6] and *Blood: a Study in General Physiology*;[7] Joseph Barcroft in *The Architecture of Physiologic Function*;[8] and Walter Cannon in *The Wisdom of the Body*.[5] J. S. Haldane should also be mentioned in this group.

Henderson, whose primary interests were those of a biochemist and general physiologist, turned his attention first to the relation between external and internal environment as it concerned the "fitness" of such components as water, carbon dioxide, oxygen, and the compounds of carbon, both to constitute and to support organic life.

The next development, in which both the British and American physiologists of the early years of this century shared, concerned itself more with the milieu interieur itself. "It is the fixity of the milieu interieur which is the condition of free and independent life," was Bernard's now familiar statement, "and all the vital mechanisms, however varied they may be, have only one object, that of preserving constant the conditions of life in the internal environment."

Be it noted that all these men were physiologists, their interests directed chiefly toward normal bodily processes rather than disease.

A perusal of almost any of Barcroft's books will reveal that one could not find anywhere a physiologist with a more lively or wider range of interests. And yet he, too, had all the physiologist's preoccupation with the maintenance of normal performance. In the course of his many activities, he did join in some studies of disease states, but only as measurements of single episodes and not as persisting pathological conditions.

Walter Cannon, also, was a general physiologist in the best sense of the term. Let us look for a moment now at *The Wisdom of the Body,* the textbook, so to speak, of homeostasis, the book in which Cannon brought together his general thoughts about physiology and natural philosophy. We find here various fascinating descriptions of processes and sequences by which the body maintains its equilibrium, in respect to water, salts, sugar, temperature, blood pressure, and the rest, as well as an account of the large reserves which the body has at its command.

A number of acute and potentially lethal conditions are mentioned, such as shock, diphtheria, and heat stroke. But the emphasis upon acute diseases in which homeostasis fails is very light indeed. More important than this, there is no mention of chronic disease at all, except old age; no consideration of any of those slow, progressing, wasting, strangling forces by which most men die. It is with Cannon as with most physiologists of his day: not so much that they discarded chronic disease; as that they actually did not appear to be aware of its significance in the scheme of things.

The tone of Cannon's book is very reassuring. The Wisdom of the Body is such that "life, liberty, and the pursuit of happiness" should always be attainable, and Saint George dispose of his various dragons with agility and ease. Cannon himself is impressed with his own optimism. At one point about the middle of the book he asks a very ingenuous question. "If," he says, "the body can largely take care of itself, what is the use of a physician?" It may be a good question at that; but we cannot stop to answer it just now.

The influence of this book by Walter Cannon upon current medical thought, and medical writing, has obviously been immense. Physiologists, clinical physiologists, even the rising young clinicians must now call practically any bodily reaction homeostatic, since it always can be argued that the reaction is protecting something, or bringing some aspect of bodily structure or performance nearer a so-called normal. And all mechanisms have to be called homeostatic mechanisms.

Now the concept of homeostasis has great validity and great usefulness. No one should deny that, and I do not mean to. There are many and highly important mechanisms which are properly described as homeostatic. But is this concept of large enough scope to include the whole range of bodily performance, pathological as well as physiological?

As old-time clinicians perhaps we can take another look at certain disease processes, especially those of chronic disease, and ask ourselves once more about this never-failing Wisdom of the Body. One can start almost anywhere. Take the process of fibrosis, scar tissue formation. This heals wounds, disposes of infections—surely a homeostatic mechanism. But is it always so? What about scar tissue in rheumatoid arthritis, ending in frozen immobile joints, scar tissue in the kidneys, ending in glomerular nephritis, scar tissue in the liver, ending in cirrhosis, scar tissue in the lungs, choking the breathing process into asphyxia? In trying to be homeostatic in one direction, the body finds that it has been most un-homeostatic in another. It is like Macbeth's "vaulting ambition, which o'erleaps itself and falls on the other."

Or, take the many aspects of the hyperimmune reaction. Inflammation, ordinarily so helpful in mobilizing the body's defensive forces, has suddenly gone off the deep end in one or another odd and explosive fashion, with febrile, vasomotor, or tissue disturbances far out of proportion to the apparent stimulus or injury.

Or, take the growth response. Growth is certainly a necessary and vital thing. But when growth erupts beyond its rightful bounds, what then? What happens to homeostasis in cancer?

What, in fact, has happened to the Wisdom of the Body in all these phenomena? Is the body, indeed, so wise? No, one must conclude that it is not. It is stupid, egregiously, calamitously stupid. It seems, alas, that the body may be no wiser in the end than we ourselves, being itself, in fact, ourselves.

Someone might well write a companion book called The Stupidity of the Body. As author one could recommend almost any good, experienced, old-fashioned pathologist.

Georges Ungar, writing recently on the subject of inflammation and its control,[9] finds himself confused. "The introduction," he writes, "of the question of purpose into the pattern of inflammation, instead of helping to solve it, tends to confuse the issue. Inflammation is useful when it fulfills what, from a strictly pragmatic point of view, can be

called its purpose: fixation of the aggressive agent, and consequently, protection of the rest of the organism. Quite as often, however, inflammation serves no detectable purpose and can even be harmful. Neither of the two mechanisms can therefore be called unreservedly good or bad." I would say that this writer is confused, not by the events that he describes but by his own terminology. One need not bring purpose into the picture, but only recognize that a given process moves sometimes, if it is well controlled, to the net benefit of the organism, and at other times, with improper control, to the net harm.

At all events, it is obvious that so-called homeostatic mechanisms can go dreadfully wrong and end up by being most un-homeostatic. While this does not appear to be well comprehended by most physiologists, at least those of Cannon's day, it certainly was no secret to Claude Bernard. He was a pathologist as well as a physiologist, and thought very clearly in this matter. May I give his words once more? "It is by the normal activity of organic elements that life manifests itself in the state of health; it is by the abnormal manifestations of the same elements that diseases are characterized."

There it is, as simple as that. This is the other half of Claude Bernard's idea that Barcroft, Cannon, and most physiologists of the normal, have missed.*

You will have appreciated that Bernard has made a large generalization. If I understand him correctly, his position is that all pathological bodily processes are only modifications of normal processes. It would require knowledge of cellular activities and biochemical reactions far beyond anything that I have, to determine whether infections, atrophy, the cancerous change and such, should properly be considered as modifications of normal mechanisms or as new events, not seen in the normal. It may resolve itself into a question of assumptions and definitions. In any case it is beyond the scope of this argument.

What I wish to consider now is only that part of pathology that is brought about by so-called homeostatic responses leading, by reason of their excess, to bodily injury or death. This excess response accounts for a considerable proportion of pathological states. There are, of course, other mechanisms in pathology that are of equal or perhaps greater importance: atrophy, necrosis, and so forth. There are, probably, opportunities for more general classification here, but with these we are not now concerned.

* But some now are discerning the limitations of homeostasis (Hamilton,[10] Oliver[11]).

It will be obvious, upon reflection, that there is no fixed line between the homeostatic and the excess response. In many situations, a given process may be exerting both effects at the same time. In the intense vasoconstriction of traumatic shock, for example, there is both the homeostatic mechanism, tending to sustain blood pressure, and the excess mechanism, un-homeostatic, depriving organs, such as the kidney, of vital blood flow, with perhaps eventually total breakdown as lower nephron nephrosis. The process of fibrosis has been mentioned; this also may be at the same time homeostatic, and un-homeostatic in the direction of excess response. Another is hypervolemia in heart failure, the congestive state, apparently useful up to a point in filling a weakened heart chamber, but leading ultimately to total failure by way of excessive intravascular pressures and overdilated heart chambers. Many other examples will suggest themselves.

If, then, it is true that in many states, especially those of chronic disease, where the body is no longer wise, an important mechanism is the excess response, the question arises whether this is not an entity that should be identified by a name.

Hyperexis

"As a body which has one leg too long, or some other excess out of proportion, is an undesirable thing . . . and is the cause of a multitude of misfortunes to itself."—Plato, *Timaeus*.

The *Timaeus*, the dialogue that follows immediately upon that of the Republic, has as its theme an even broader subject: the origin and nature of the created universe, and the place and functioning of man within this framework. The limited mathematics and physics, and exceedingly limited physiology of Plato's time, make much of the speculative reasoning in the *Timaeus* rather absurd to our view; yet within it are flashes of brilliant insight; and, as Whitehead has shown,[12] the basic questions and assumptions upon which it rests are still among the foundations of modern philosophic thought.

The quotation above is taken from a part of the dialogue's discourse on the familiar Platonic theme of symmetry and proportion; but its main purpose for us is that it contains the word excess, in this instance the Greek word "hyperexis." The derivation of this word is simple, and means "having too much." This fits our need quite well.

As compared with homeostasis, hyperexis as a word is somewhat inflexible. It does not lend itself to adjective-formation too well, and the antonym —hypo-exis, or an-exis—would be quite awkward. For a more manageable word, one might try hyper-

telesis, "beyond the mark," or "beyond the goal."

But do we want a word with an easy transition to the opposite and the negative of itself? Just because a physiological or pathological word can be expressed as an opposite does it mean that the exact opposite also exists in physiology and pathology? Take even the most common, atrophy and hypertrophy. We think of them casually as biological opposites. They are not. Hypertrophy is one thing—a number of different things, actually; atrophy represents another set of things, at the other end of the line, so to speak, from hypertrophy, but not in any biological sense an exact opposite. Our easy application of names sometimes obscures rather than clarifies what we are thinking about.

I am inclined, then, to vote for hyperexis, with the notion that, if it is worth anything, it ought to stand by itself.

Homeostasis and Hyperexis

In the endeavor to establish the validity of unhomeostatic mechanisms, in the above argument, it may appear that homeostasis itself has been slighted. There is no such intention. The two categories must exist, and do exist, together. The one represents a tendency toward unity, a dynamic uniformity and stability; the other an equally inevitable diversity, upheaval and "perturbation," uncontrollable change. In the dimension of time, homeostasis and the Wisdom of the Body hold for certain periods, greater or less, each a small cycle of the biological system under consideration; eventually to be displaced, or destroyed, as some larger cycle takes over, through, let us say, some form of hyperexis; or some more violent pathological process; or by way of slow decay: the "free and independent life" giving way to shrunken activity, invalidism, and ultimate dissolution.

Saint George and the Dragon

Finally, since Walter Cannon ventured to suggest the extension of the general notion of homeostasis beyond the realm of physiology into the world at large, there is good precedent for doing the same with homeostasis and hyperexis considered together.

It was Cannon's proposition, as we have already seen, that the homeostasis of the mammalian body might serve as a prototype for society—a system of checks and balances which should provide stability in an uneasy world: a series of Saint Georges to kill off the dragons of unwelcome change.

In the final chapter or "epilogue" of his book, Cannon compares physiological homeostasis with that which may be said to exist in the social, industrial, and political relationships of mankind, and points out many analogies. A hopeful picture is drawn of the possibilities for a better, healthier, more stable, more unified, less angry world. Yet with all the author's kindly optimism, one feels that some important things have been left out of the picture which ought to be there. For one thing, even if the social system is stable and secure, human beings will continue to suffer ill disappointment, calamity, sickness and death, all degrees and combinations of unfortunate or tragic occurrences. There is no lasting security. We suggest that this is not pessimism, it is simple realism. Any inclusive philosophy must face the pathological just as steadily as the physiological. We cannot all be lucky all the time.

In the larger social and historical cycles also, there must be and inevitably will be larger disruptions and perturbations. It was the most conservative, best protected, and most traditional of poets laureate who reminded us of the old order changing, yielding place to new. What changes will take place cannot be predicted, only that they will take place. Homeostasis may mitigate these great and devastating events—whether of natural or human origin—but it will not basically remove or prevent all of them. And perhaps these larger changes may still be homeostatic in the view of some larger entity. Our point here is that they are not homeostatic for the smaller entity.

There is nothing new in this. All that we are doing, perhaps, is to reintroduce to the currently modern mind an essential dualism which, in the thinking of past ages, faced and met reality rather better, in some respects, than we do now. One could argue, in fact, that much that is wrong with us, in this particular day and generation, and in this particular land of ours, derives from the life—liberty—pursuit-of-happiness, homeostatic, generally polyanna attitude that we have, and were born and raised in; that we expect too much good and are too impatient of ill; that we need to have driven in upon us more of the notion that the essence of living is struggle and suffering; that we need a more vigorous and fearless acceptance of the pathological; that pathology is as necessary a part of life as physiology, and dying as necessary as living; that hell and its devils are as real, and as important, as the angels in heaven; that our good Saint George, for all his valor, does not kill all the dragons, and that he will find, as indeed all saints have found, that the powers of evil still crowd upon him, when his arm has become weary and his sword is still.

May I summarize my argument as follows:

1. Homeostasis and the Wisdom of the Body,

admirable concepts in their place, do not adequately describe all bodily processes, especially those of disease. In chronic disease, homeostasis does not prevail and the body is not wise.

2. In looking for a wider approach, can we safely rest upon the more complete version of Claude Bernard's concept, namely, that disease is only perturbed activity of essentially normal elements; or is this still not enough?

3. In any case, an important category in pathology is the excess response, a homeostatic effort that overreaches itself, to the detriment or even the death of the organism. The term hyperexis is suggested for this type of excess response; a term that appears in Plato's *Timaeus,* and means "having too much." Other general categories of pathology are doubtless equally important and could be defined.

4. Pathology should be considered as well as physiology in general descriptions of living processes.

References

1. Claudel, Paul *The Book of Christopher Columbus.* New Haven: Yale Univ. Press. 1930.
2. Cannon, Walter B. *The Wisdom of the Body.* Philadelphia: W. W. Norton Co. 1939.
3. Whitehead, Alfred N. *Science and the Modern World.* New York: Macmillan. 1925.
4. Voltaire, F. M. A. *Candide.*
5. Bernard, Claude *Introduction a l'Etude de la Medicine Experimentale.* Paris: Balliere et Fils. 1865.
6. Henderson, Lawrence J. *The Fitness of the Environment; An Inquiry Into the Biological Significance of the Properties of Matter.* New York: Macmillan. 1913.
7. Henderson, Lawrence J. *Blood: a Study in General Physiology.* New Haven: Yale Univ. Press. 1928.
8. Barcroft, Joseph *The Architecture of Physiological Function.* Cambridge: Cambridge Univ. Press. 1934.
9. Ungar, Georges *Lancet,* 263, 742 (1952).
10. Hamilton, W. F. *Circulation,* 8, 527 (1953).
11. Oliver, J. *Am. J. Med.,* 15, 535 (1953).
12. Whitehead, Alfred N. *Adventures of Ideas.* New York: Macmillan. 1933.

LYCOPODIUM, THE CLUB MOSS AND GROUND PINE

The delicate plants of the genus *Lycopodium* leave their native haunts in woods, thickets, and clearings, on mountainsides and over the barrens in order to make their appearance among us at Christmas time. They have no economic usefulness to man save this one, that, although flowerless, they appeal to us and delight us with their simple grace and beauty, like their fairly near relatives, the ferns. To the botanist, however, club moss and ground pine speak of wonderful things, of days when the earth, far toward its poles, was clothed in a weird jungle of giant trees with strange scale-like and feather-duster foliage that reared above the swamps—swamps in which splashed the monstrous flat-headed amphibians that preceded the reptiles upon earth. *Lepidodendron* and *Sigillaria,* the giant club-mosses of the Carboniferous, fell into the acid swamps and were preserved and gradually converted into pure coal. Trains filled with holiday travellers run, lights illuminating Christmas trees glow, and cold hands and feet are warmed by means of the forests of giant club mosses that grew 200 million years before any man thought of digging them up and burning them, or of placing their surviving descendants around a Christmas wreath. It is an appropriate gesture of recognition.

(The superb photo of *Lycopodium obscurum* on the cover was made by Professor A. M. Winchester, Chairman of the Department of Biology at Stetson University, whose hobby is the photography of nature.)

The National Bureau of Standards

NATE HASELTINE

A veteran newspaperman, Nate Haseltine has been reporting since 1931, except for approximately three years when he served as an artillery man during World War II. He began specializing in medical and scientific news about six years ago. In 1949 he was made science editor of the Washington Post, *and for the past two years has been listed among the top ten science writers of the nation. His 1950 series of articles, entitled "The Human Heart," has been reprinted in pamphlet form for public distribution by the National Heart Institute.*

SCIENTISTS working together through the years have made life safer, simpler, and more secure. In the United States, they came from fields of individual endeavor more than fifty years ago to pool their efforts in a new surrounding, the fledgling National Bureau of Standards. Today, all Americans have benefited from the scientific achievements that have come out of the limited facilities that Congress created in 1901. The congressional sponsorship, tendered hesitantly, changed to ever increasing support that has paid off in national and individual terms of safety, convenience, and security.

For safer air travel, Bureau scientists developed ILS (instrument landing system) and uncovered many causes of air wrecks. For undersea safety in war or peace, they contributed an underwater antenna for submarines. For land travel, they produced the magnetic fluid clutch, and even determined the safest color tones for traffic lights.

In making ordinary living more comfortable, they set standards for electric light bulbs, tested such things as the wear and tear on shoes, and developed specific-purpose materials such as false teeth bases that will not shrink, break, or dissolve in mouth secretions or in the fluids that one drinks.

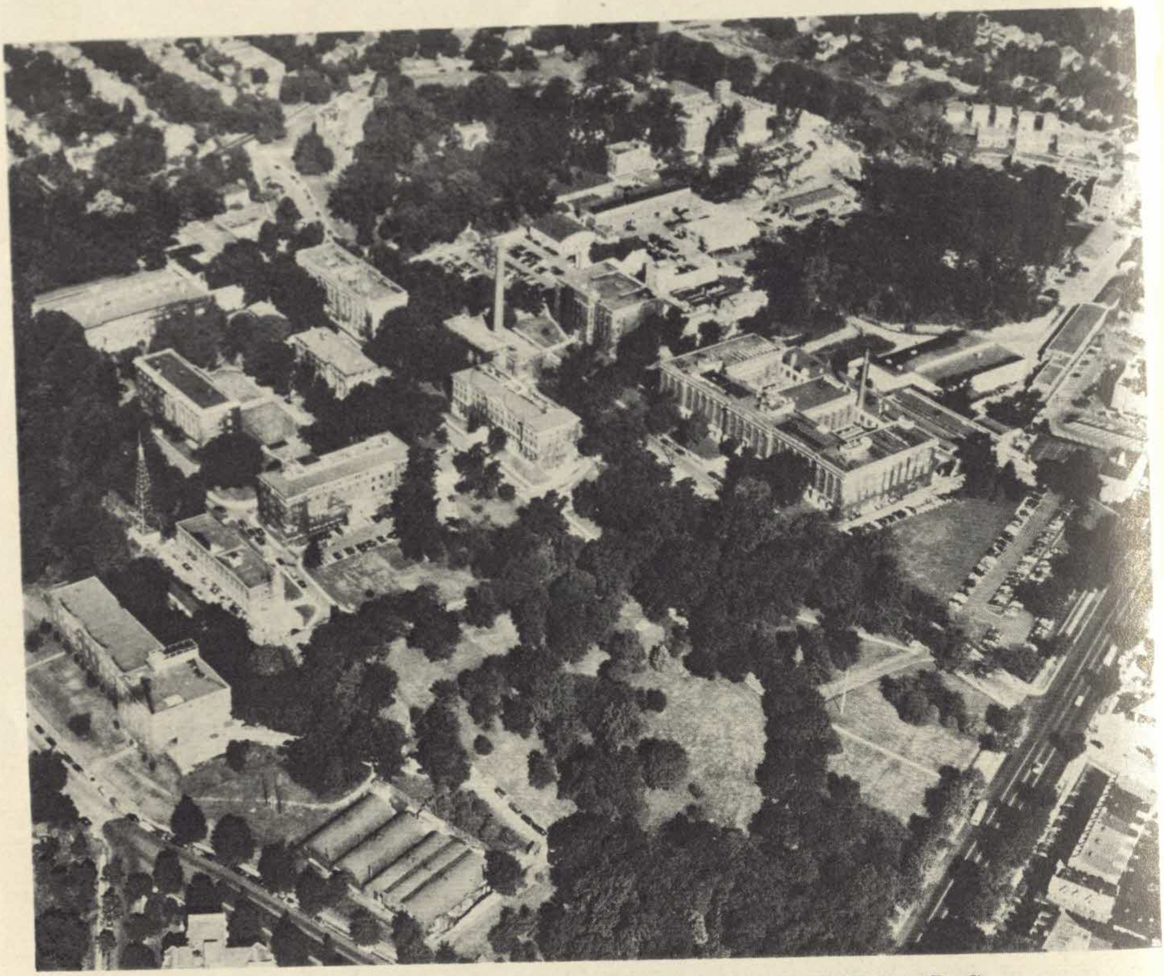
For national security, Bureau scientists made, in cooperation with scientists of the Applied Physics Laboratory of The Johns Hopkins University, the first successful radio proximity fuze for use in explosive missiles. The fuze and its successor models have been acclaimed the second greatest scientific achievement of World War II (second only to the A-Bomb, whose development was also aided by Bureau scientists).

All these and more were certainly not specific in the minds of those who at the turn of the century began their demands for a federal standards bureau. From 1870 to 1901, the government had an office of weights and measures which was operated by the Treasury Department, but the office was extremely limited in the types of standards it governed. It had no official standards of measurement that could be applied to the developing sciences. It had no yardsticks with which to measure the candlepower of light bulbs, no scientific bases for the accuracy of thermometers or barometers. Americans were sending their instruments to Germany or other European scientific centers for laboratory checkings and for the calibrations unavailable to them here.

The National Academy of Sciences took cognizance of the situation in 1900 with a resolution endorsing the movement for "a national bureau for the standardization of scientific apparatus." Prefacing this endorsement were the words: "The facilities at the disposal of the Government and of the scientific men of the country for the standardization of apparatus used in scientific research and in the arts are now either absent or entirely inadequate, so that it becomes necessary in most instances to send such apparatus abroad for comparison."

When the resolution was passed, the government was spending less than \$11,000 a year to guarantee the uniformity of grocery scales, foot rules, flour barrels, and surveyor's chains; the Navy was sending its scientific instruments to Europe for calibration. The United States was ready for the benefits of mass production, but it lacked one of the basic requisites, standards of precise measurement for tooling to mass production.

The congressmen of the day recognized the high caliber of the scientists who were backing the move



Aerial view of the National Bureau of Standards laboratories, Washington, D. C.

for a new federal bureau but, although impressed, were reluctant to further a movement that, to them, looked like unwanted government control over commerce and industry. In addition, the new bureau would require money. A Tennessee representative protested on the floor of Congress that the sponsoring bill would set up "an enormous institution . . . it carries an expense of at least \$250,000." Today, fifty-two years later, "the enormous institution" of that congressman's concept has grown to one with an annual budget of \$6,000,000 for its own operation and together with fund transferals from other federal bodies, largely for defense projects, its working budget for the last fiscal year exceeded \$50,000,000. Recently, the Defense Department announced its intention to relieve the bureau of administering major ordnance research and development programs, but the projects are to be continued in bureau facilities and with bureau personnel.

The National Bureau of Standards was created by a bill passed on March 2, 1901. Nine days later, Samuel W. Stratton, who was to serve for twenty-one years, was named its first director. Twenty-seven thousand dollars were allotted for the payroll of the Bureau for the first year. Although sponsors of the National Bureau of Standards had requested that its director have an annual salary of \$6000, Congress reduced this to \$5000, perhaps because congressmen were being paid the lower salary.

For \$25,000 eight acres of rolling farmlands, in what was then cow pastures of northwest Washington, were purchased, and there two laboratory buildings—still in use—were built. Today the Bureau, one of the world's best-known scientific institutions, consists of some seventy buildings, about twenty of which are major ones, set within a sixty-eight-acre park surrounded by apartment buildings, stores, and private homes. Sixty additional

acres were acquired slowly at great cost to meet an ever present need to expand. The original staff of eight in the small old Office of Weights and Measures has grown to the present Bureau staff of about forty-six hundred.

The Bureau is no longer physically confined to Washington. It operated, until its recent shift to the Navy, a missile development installation at Corona, California. It operates special cryogenic engineering and is building new radio propagation facilities at Boulder, Colorado. It operates numerous small field stations for such things as ionospheric observations, cement testing, and radio time and frequency service.

Even at its present state of expansion, the Bureau is still a laboratory of individual research scientists. Here is scientific freedom for the study of the constants of nature, the physical properties of basic materials, and the fundamental standards of measurement. Here scientists work together, their successes speeded by their cooperative efforts and by the corps of technicians, skilled craftsmen, and administrators gathered together for scientific advancement. Here individual genius is inspired to greater accomplishments than might have come from solitary research.

In the Bureau's laboratories an atomic clock was fashioned, an infallible timepiece calibrated to vibrating atoms instead of to swaying pendulums. This new clock's potential maximum of error is less than the loss or gain of one second in a span of three hundred years. An atomic standard of length has also been developed in the form of a lump of mercury-198, an isotope obtained by bombarding gold with neutrons. Atomic standards of weight, or mass, are also being considered by the scientists working at the Bureau.

In 1901, the Bureau inherited only two standards, the meter and the kilogram. Now it has set more than forty different kinds of scientific standards, and these standards for commerce, industry, and science total more than seven hundred, some five hundred of them in chemicals alone. It has the standard for the length of an inch, the weight of a pound; standards for the range of temperatures from the coldest 459.6 degrees below zero Fahrenheit to 6000 degrees of rock-melting heat. Without its standards for electric power, radio wavelengths, and atomic radiations, American technology would long have remained stifled. American industries today send their gages to the National Bureau of Standards for periodic calibrations. And where industry may require an accuracy of measurement to within one part in one hundred thousand, the Bureau's instruments will check to a degree of one part in one million. By increasing its accuracies over the years, the Bureau has helped make mass production as it exists today. Now, tiny machine parts made in one city will exactly fit other instrument parts made elsewhere, for final assembly in still other locations.

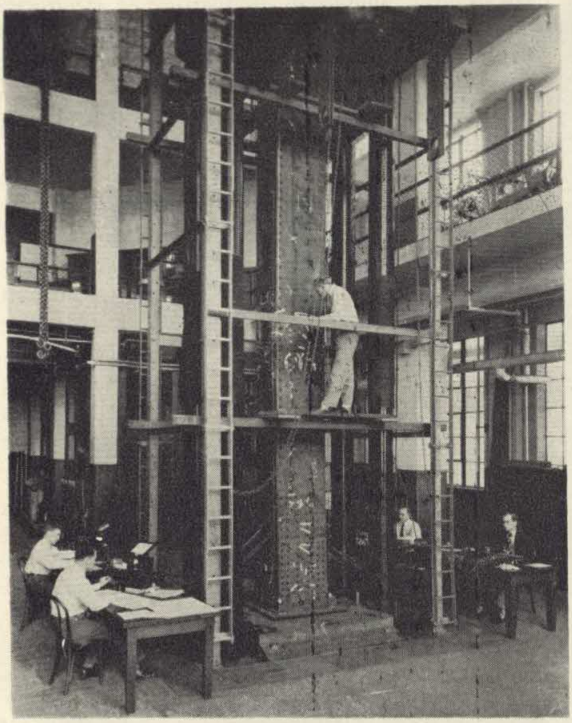
The setting of measurement standards was the primary reason for founding the Bureau, but the standardization work branched to fields undreamed of when the Bureau was started. Government agencies and industries soon began to ask more and more from the National Bureau of Standards. The United States then, as now, was the biggest purchaser in the country and, like any good buyer, wanted its purchases to measure up to the rights of the buyer. The Bureau was called on to test the materials, particularly cement for dam-building projects, for which the government was to pay. Manufacturers first used the services of the Bureau for checking the accuracies of their scientific instruments, but it was not long before other producers also turned to the Bureau for help. In 1907, for example, at the request of the American Foundrymen's Association and the Association of American Steel Manufacturers, it undertook the analysis and distribution of standard samples of steels and irons which industries needed for their own comparative checkings. Later, it cooperated with the American Chemical Society to ensure uniformity in technical analysis for the improved quality of chemical reagents.

These jobs taken on by the Bureau and the almost Herculean tasks it assumed during the wars are not specifically designated in the law which created the National Bureau of Standards. The bill that President McKinley signed in 1901 described the Bureau's *raison d'etre* in these words:

Sec. 2. That the functions of the bureau shall consist in the custody of standards; the comparison of the standards used in scientific investigations, engineering, manufacturing, commerce, and educational institutions with the standards adopted or recognized by the Government; the construction, when necessary, of standards, their multiples and subdivisions; the testing and calibration of standard measuring apparatus; *the solution of problems which arise in connection with standards*;* the determination of physical constants and the properties of materials, when such data are of great importance to scientific or manufacturing interests and are not to be obtained of sufficient accuracy elsewhere.

The all-inclusive "solution of problems" clause became the authority wherever there was doubt that the Bureau could legally take on a research project for other government agencies, or for industry, or private groups such as the baseball leagues.

* The italics are those of the writer.



Experimental arrangement at the Bureau for studying the effects of perforations on the strength of a steel such as used in the Calcasien River at Lake Charles, Louisiana. A ten-million-pound testing machine has provided engineering data on full-scale structural units ranging from steel girders and aircraft components to structural building elements. Accurate calibration of the largest testing machines is possible since the development of four strain-gage dynamometers capable of measuring forces up to six thousand tons.

The Bureau undertook one baseball study to solve the annual complaint that home-runs records were being broken because baseball makers were producing livelier balls. By rigging up a baseball batting device, Bureau scientists found that the complaint was not justified. The balls, old and new, showed the same liveliness.

Much of the work of the Bureau has been done to make the work of others easier. Take the dollar bills you use today. They may not go as far as the dollars of years ago, but they get there in better condition. The paper currency of today seldom tears because Bureau scientists test the paper stock on machines that fold and unfold it thousands of times. If the paper is not strong, it is not used to make money. Bureau scientists, also, invented an electronic machine for the more effortless counting of mutilated and outworn paper money that is to be destroyed.

Occasionally, the Bureau employees are used for some of its testings. Not content to rely upon the results of machine-testing of shoe soles, the scientists outfitted Bureau guards with shoe soles of the material under test and let the guards wear them out normally; when its scientists were testing the new plastics now widely used in dental fillings, the Bureau called for volunteers—employees willing to have their cavities filled—for science and for free.

Some of the major contributions of the National Bureau of Standards to science and technology, not already mentioned, include:

1904 Development of the first luminous (Neon) tube. Luminous script signs in glass tubes containing noble gas excited by electric discharges were first made by Bureau scientists and exhibited at the Louisiana Purchase Exposition in St. Louis, Missouri. It was twenty-six years before the development was commercialized and a new advertising medium was established.

1905 Invention of the deflection potentiometer.

1907 The making of the first optical prism of silver chloride. Since the discovery that rock salt was transparent to infrared energy, a search has gone on for other materials having similar properties. The original silver chloride prism turned black after exposure to light, but recently a successful technique has been developed for polishing that material.

1910 First complete international uniformity of electric units. The Bureau served as host to the International Technical Committee on Electrical Units and for the first time established complete international uniformity in this electric field.

1912 The first comprehensive study of methods of measuring heating values of gases.

1914 Establishment of standards of radiant flux. The standards, used principally for calibrating thermopiles, were first issued by the Bureau in the form of calibrated carbon-filament lamps. The standards have been furnished to national laboratories throughout the world as a uniform basis of radiant energy measurement.

1915 Radio direction finder. Invented by Kolster and Dunmore and improved over the years, it is now in general use by all commercial airlines.

1917 The development of a method for slip-casting large refractory pots for use in melting 1000-pound batches of optical glass. Previously, optical glass was melted in hand-made pots of non-uniform quality, thus producing glass containing numerous flaws.

1918 Infrared detection. Bureau scientists pioneered in the detection of distant objects and signaling by infrared rays. This work contributed significantly to military research for both world wars.

1920 Development of the dental interferometer

for measuring the expansion of dental amalgam, a device for accurately measuring the setting expansion of dental filling materials.

1922 Development of a precision high-speed oscillograph camera.

1922 Invention of the first alternating-current radio set, perhaps the most revolutionary development in radio. This invention put radio in the home.

1925 Beginning of the standard frequency and time interval broadcasts.

1926 Development of a camera for photographing the interior of a rifle barrel.

1929 Engine ignition shielding in aircraft.

1929 Casting of the first large telescope reflector (70 inches) in the United States.

1931 The first x-ray protection code. The Bureau was the first organization to bring about general unification of x-ray and radium protection codes.

1935 A method for determining the fineness of portland cement.

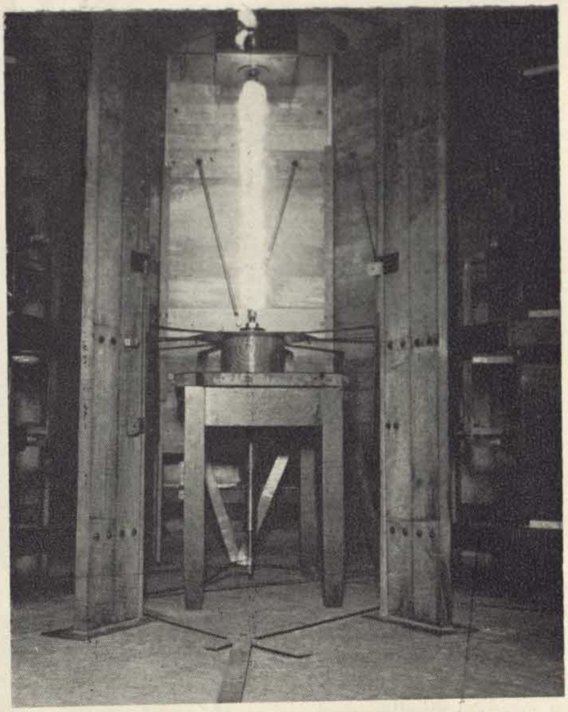
1936 Application of telephoto lenses to eclipse photography.

1936 Development of a radiosonde system, one of the most important contributions to the science of meteorology.

1938 Measurement of the gains of hearing aids for their users by a cavity pressure-testing method.

1940 Discovery of the cause of, and a remedy for, white-coat plaster failures. This was the first comprehensive study of the cause of such plaster failure, and the development of a treatment for limes that eliminates the possibility of expansion failures of plastered walls already in service.

A precision instrument for calibrating airplane mapping cameras, developed by the Bureau, is tested in the optical instruments laboratory. The test involves determining the angular deviation between collimators in the collimator bank beneath the table.

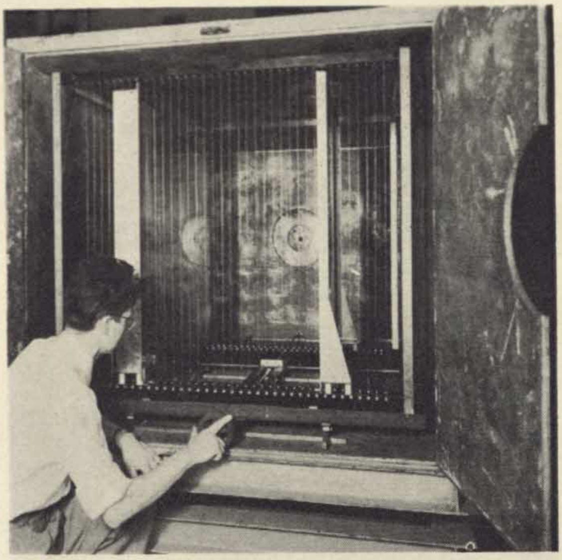


A 100,000-ampere current surge is measured during discharge by means of a special shunt (beneath table) developed in the Bureau's high-voltage laboratory. Improvements in the design of electric power systems and their component parts have greatly reduced power failures from lightning. These improvements are a result of the use of surge generators.

1941 Radio proximity fuze for bombs. As mentioned previously, Bureau scientists developed the first proximity fuze for use in nonrotating missiles such as bombs and mortar shells. Meanwhile, scientists at The Johns Hopkins Applied Physics Laboratories were developing radio-detonating fuzes for rotating missiles such as artillery shells. Radio proximity fuzes were credited by some with shortening the course of World War II.

Bureau scientists have not taken a group monopoly on research in the way that individual scientists, working alone, long held secret monopolies. Instead, as in the instance of the proximity fuze research, the scientists of the Bureau solicited the help of the entire electronics industry and shared their findings within the limits of military security.

In 1941, too, scientists under the direction of Dr. Allen V. Astin, who later succeeded Dr. Edward U. Condon as Bureau director, pioneered the research in telemetering from missiles in flight. If there are any passenger space ships of the future, space travelers can credit the National Bureau of Standards for much of the research that made their transportation possible. At the present time



The Bureau's standard x-ray ionization chamber for use in the 500,000-volt range. Several primary standard ionization chambers that permit the calibration of devices over a wide range of x-ray energies are maintained.

this telemetering is a major tool in the development of guided missiles and other electronic ordnance devices.

Many of the research and development accomplishments of World War II are still muffled by security regulations, but during the war years, Bureau scientists developed a test for weather resistance of porcelain enamels, a way to extract alumina from clays and high-silica bauxites, a T-bend test of the welding qualities of steels, and a way to coat metals with ceramics resistant to high temperatures. They also advanced the development of special-purpose batteries; found out why certain dental fillings backfired on the patients, and produced extreme pain; developed a wind speed and direction indicator used principally at airports in aircraft takeoff tests; and in 1944 produced the first successful guided missile, "The Bat."

Bureau developments and research pushed to successful stages after the end of World War II included studies on the use of concrete as a high-energy radiation shield; printed circuit techniques, for miniaturizing electronic gear; and a spectroscopic lamp containing artificial mercury-198, improving the accuracy and convenience of precise length measurements.

The Bureau's list of accomplishments over the years is seemingly endless, and many that may be of tremendous importance in future developments are not included in this abbreviated listing.

The National Bureau of Standards is a government agency, without independent federal status. It is only one of the divisions of the Department of Commerce. Unfortunately, the nonscientific public has come to regard the Bureau, erroneously, as a testing laboratory for marketed products. The recent furore over a controversial battery additive, AD-X2, aided and abetted this public concept. Once again the Bureau was flooded with requests from consumers, most of them seeking advice regarding the purchase of automobile batteries. The reaction was a discouraging one to the scientists and their fellow workers, who would like the public to know that the primary function of the Bureau is not that of a consumers' research.

When the Secretary of Commerce, Sinclair Weeks, tried to dismiss Dr. Astin as Bureau director, he raised a storm of protests from scientists throughout the United States. Mr. Weeks charged that Dr. Astin was not sufficiently objective, and lacked the business point of view, because Bureau scientists found no value in a commercial battery "dope" which, its maker claims, will lengthen storage battery life. Bureau scientists welcomed the support of their colleagues, but they were irritated by the public's reaction in besieging them for advice as to which products—be they floor wax, soap, or auto batteries—are the best on the market.

To set the nonscientific public straight, only one per cent of the Bureau's work is concerned with product testing, and such testing is for other government agencies. Only one-twentieth of this one per cent deals with "market-place" products, such as the mixture for rejuvenating auto batteries. The other nineteen twentieths concern tests of materials purchased by other government agencies, such as cement for dam-building, lights for government buildings, and tires for government cars.

Because the Bureau is a federal operation, its scientists are justifiably worried whenever the national administration changes hands. Fears and worries hang heavily now, as they did when President Franklin D. Roosevelt swept the Democrats into power in 1932. With that change, about one-third of the Bureau's personnel lost their jobs; many, however, regained them within a year.

The agreement between Secretary of Commerce, Sinclair Weeks, and Secretary of Defense, Charles E. Wilson, to separate the Defense Department's major ordnance research and development programs from Bureau administration (although the contracts were to be continued at Bureau facilities and with Bureau personnel) only increased fears of political "high jinks." Some \$35,000,000 of transferred funds in the Bureau's current fiscal program, and some eighteen hundred of its forty-six hundred employees, were involved. Some described

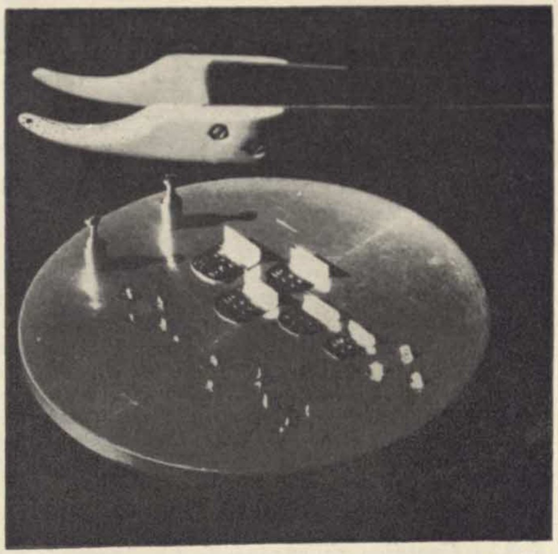
the move as only a paper transfer; others thought it was a beginning of an eventual dispersion of the Bureau's advanced research and development projects to private industry.

When Secretary Weeks first proposed releasing Dr. Astin, it was not the first time that a director of the National Bureau of Standards faced a job-severance. Actually, however, the Bureau has been relatively free of political interference—save, perhaps, somewhat periodic surveillance by congressional committees and the harassment of individual members of Congress who seek favors and services for themselves or their constituents. Dr. Astin took office as the fifth director of the National Bureau of Standards June 12, 1952. The four directors who served the forty-nine years before him did so under nine presidents and eighteen secretaries.

The first director of the Bureau was Dr. Samuel W. Stratton. He took office March 11, 1901, and resigned December 31, 1922, to become president of Massachusetts Institute of Technology. Appointed by Republican President William McKinley, he served under the succeeding Republican administrations of Theodore Roosevelt and William Howard Taft, and the Democratic administration of Woodrow Wilson. He resigned under the Republican administration of Warren G. Harding as the result of differences with Herbert Hoover, then Secretary of Commerce.

George Kimball Burgess became the second Bureau director April 21, 1923, following a brief period during which F. C. Brown served as acting director. Dr. Burgess died in office, July 2, 1932, after serving under three Republican presidents, Warren G. Harding, Calvin Coolidge, and Herbert Hoover.

Lyman James Briggs took office as the third director June 13, 1933, and retired at the age of seventy-one November 5, 1945. Dr. Briggs, an assistant director when Burgess died, immediately became acting director and served in this capacity until his official appointment as director by President Franklin D. Roosevelt. President Herbert Hoover had sent to the Senate Dr. Briggs's name as Bureau director. The Senate, however, took no action on it, presumably in view of the defeat of Hoover and the Republican party in the November, 1932, elections. President Roosevelt sent Dr. Briggs's name to the Senate again in April, 1933, and the appointment was confirmed. It was reported in the press of the period that, in regard to Dr. Briggs's appointment, a reporter asked the President, "What are his politics?" To which the President replied, "I don't know; I never asked him. Everyone says that he's the best man for the job." Dr. Briggs served under Presidents Roosevelt and Truman, and under four Secretaries of Commerce. He was permitted to serve beyond normal retirement age of seventy at President Truman's request, when the country was still at war.



Standard weights used by the Bureau for calibration of standards for modern micro-balances (values range from one gram to 0.05 milligram). Methods have been developed for calibrating standard weights below one hundred milligrams with a precision of one or two ten-millionths of a gram.

Dr. Edward Uhler Condon, now president of the American Association for the Advancement of Science, became the fourth director of the National Bureau of Standards November 6, 1945. He served until the effective date of his resignation, September 30, 1951, when he became director of research and development at the Corning Glass Works. Dr. Condon was appointed by President Truman, and served under three Secretaries of Commerce, Henry A. Wallace, W. Averell Harriman, and Charles Sawyer.

Problems and Parameters of Science Literature*

HAROLD OATFIELD

The author was educated at Reed College, Rice Institute, and Iowa State College, where he received his M.S. (1933) in organic chemistry under Professor Henry Gilman. After working in a control laboratory of Crown Zellerbach Co., he spent 11 years as a member of the Intelligence Division, DuPont Experimental Station. From 1945 to 1947 he participated as a Library Research Fellow in an experimental program at Massachusetts Institute of Technology. He was a professional associate in the Division of Medical Sciences, National Research Council (1947–1951), and served as secretary for the Committee on the Future of the Army Medical Library. Since 1951 he has been a literature research chemist with Chas. Pfizer & Co., Inc.

ACCORDING to an editor of the *Manchester Guardian,* there are in fact no longer four philosophical elements but five: earth, air, fire, water, and *paper*. Even he thinks too much of the last element is used in sound, soporific gobbledygook.

A parameter, I need hardly remind you, in mathematics is a quantity constant in the case considered, but varying in different cases. I view the major parameters of science literature as: ethics, publication, distribution, understanding; and in an effort to clarify for myself what dizziness is involved therein, I have grouped under those headings selected problems for consideration. Many other permutations exist. Let me say at the outset, I have no key to the problem of breasting the tide of scientific publication—I merely suggest channeling it into fewer secondary outlets.

At least four of the six premises† of today's code of ethics for scientists apply to literature: the sharing of new knowledge, the obligation to publish important findings, the necessity for veracity, and the recognition of priority. What is the ethical code for the scientist in respect to the relation of his science and his discoveries to the community and to the race? We still argue without agreement. But it is clear that we should prepare an addendum to the code comparable to the Hippocratic oath of physicians.

Du Bridge[1] has stressed the "problem of converting our expert knowledge of scientific problems into intelligent proposals for political action to meet them," and points out that in this sphere we are laymen though we may not care to admit it. Erskine[2] then steps in to advise scientists not to regard themselves as a caste able to think dogmatically for all. More thoughtfully, Bronowski[3] has described science as a giving, material and spiritual. He shows how its values rest in human fulfillment, in contrast to a standard of ascetic denial promulgated as the basic moral quality of the past and still so considered by many nonscientists today. Bronowski warns us to be alert to defend from misapplication both scientific method and the mind that employs it.

In humanism an earlier civilization gave us the concept of improving culture for ourselves and our descendants by (1) striving for truth or intellectual excellence and the ordering of knowledge—what we call science; (2) placing that knowledge with reference to the rest of life—or the development of wisdom; and (3) applying that wisdom in daily life —or prudence. Today we are neglecting to round out that concept by exercising prudence outside the laboratory.

Let us pass over the hoary problems of how to train the user of scientific information to seek it out for himself, and the unwieldiness in use of the existing bibliographic aids for arranging knowledge due to the volume of material, accrued and accruing, that must be winnowed. What I am concerned with seems to be an ebb in the tide of cooperation among scientists. You may voice thousands of examples to the contrary and mention every committee in the country. We all have taken a vicarious pride or better in the pooling of effort and achievement in a Manhattan project or the development of penicillin. It is not surprising that there is cooperation. It is the lack of it that seems to violate scientific ethics. The multiversity of specialization within the twentieth century has been built upon a

* Based on a paper presented at the 118th meeting of the AAAS, Philadelphia, Pa., December 30, 1951.

† The others: maintenance of integrity; striving for refinement of method.

base of continuing cooperation and interdependence. When one unit fails to do its part, it reflects broadly on the interests of the others. These are a few indications. It must not become a trend.

Some years ago a mathematician abroad sent a manuscript, an original contribution, to an august American institution. Because he was not a member of that institution, his paper could not be published in its journal. Meanwhile, the war cut off communication. Among scientists, no less than for the rest of circulating mankind, word of mouth conveyance of information abounds. During World War II scientist A of the aforesaid institution hinted to mathematician colleague B at another institution that this manuscript might have an important bearing on a problem which B was currently attempting to solve in connection with the war effort. A request was made for a copy of the manuscript. Due deliberation by the governing board (predominantly of scientists) of the institution concerned led to denial of the request. Further appeal proved fruitless until high officials intervened. To what avail custodianship without related use?

Again, an institution had in its files a unique copy of a map of certain waters. During the war that institution was requested to release the map for government use. This request did not meet outright refusal, but was handled by eventually turning the map over to a national repository. The war was nearly won by the time it emerged from "processing" to reach the requesting agency.

A great university's financial resources have dwindled owing to the character of the times and the draining off of potential students by the Korean campaign. It attempts to recoup in part a resulting cut in its library budget by charging for outside use of its library resources. That reversion to nineteenth century society library tactics may be a small price to the individual user for the convenience provided, but multiplied on all sides the practice would spell the end of the interlibrary loan policy, which in this century has aided scholarship and research of all sorts so markedly within the United States through sharing of technical resources.

A zoologist acquired by unorthodox exchange a book published behind the Iron Curtain, a book which contains descriptions of a half dozen new species not yet indexed elsewhere. Did he send a note of apprisal to the appropriate Record, or tell his acquaintance, scientist C, who had worked with organisms of this class for years? Certainly not! Has the stoic sense of civic duty gone out of practicing scientists? No, but science does not alter human nature. Nevertheless, being his brother's notekeeper should not relieve a scientist from responsibility to share knowledge.

There is no need to elaborate upon political restrictions placed on scientific thinking, on exchange of information, and on circulation of scientists themselves. The decline of scientific cooperation on the international level must not become an excuse for loss of the spirit of cooperation among scientists either as individuals or in groups. The next retrograde step would be failure to impart new knowledge via publication. The quantity of publication now pouring forth continually induces vehemence in scientists. How changed their tune of invective would be if a serious decline in publication volume were to occur! Under our present publication setup there will hardly be scientific publication at the expense of security. Even granting the premise of an unwary editor, material, except in notes and dribbles, could not be issued that fast.

A prime problem is what to do with new-found knowledge. When a man has convinced himself that he has found out something worth telling his associates, which he can impart without violating his principles, established policies, or patriotism, and when his compeers, the editor and referees of the journal of his choice, have agreed, Karma, or the inevitableness of the act, enters a little Carnot cycle stage. You will recall that Carnot's classic cycle involves an ideal heat engine, consisting of a conducting cylinder that is closed by a nonconducting piston and containing a quantity of a perfect gas. This working gas goes through four successive operations, alternatively in temperature baths and insulating jackets: (1) isothermal expansion to a desired point (the temperature remains constant but the gas volume increases); (2) adiabatic expansion to a desired point (work is done by using up a part of the intrinsic energy of the system without adding from an outside source); (3) isothermal compression (heat is given off) to such a point that (4) adiabatic compression brings it back to its initial state. In the publishing analogy the original data expressed in a paper represent the gas, and the heat engine components are editors, journals, and scientists: (1) primary publication passes isothermally to secondary publication in due time (no new knowledge is created, but we have more paper dealing with the same facts); (2) adiabatic expansion is paralleled by expansion of subject entries in indexes; (3) isothermal compression occurs as the searching scientist passes from volume to volume (he gives off heat); and (4) the published data come full cycle, as though by adiabatic compression, when they are used in a new publication

or cited in a review. Thus, the world of scientific publication conforms on this plane to the second law of thermodynamics.

There is publishing entropy, too. Entropy is the measure of the unavailable energy in a thermodynamic system. In the scientific publishing system, lost facts and unindexed papers comprise its entropy. Thermodynamically, we realize that the lack of perfect conductors and insulators makes reversibility unattainable in the Carnot cycle. In the analogous cycle of recording scientific findings we also have our imperfect conductors, the abstracts and indexes. I would hesitate to acclaim any scientific paper as an ideal gas. That bit of published knowledge may be applicable toward the solution of another man's problem. This man has not only the personal obligation to become aware of its existence but, once aware, to locate it physically so that he may examine it. Photostating and like means have solved the matter of retaining working copies.

In the problem of location, the man of science tends to fall back on a librarian for help. Libraries have carried out the organization of catalogs of scholarly resources, as well as mapping techniques related to their storage problems, in order to be able to locate material with minimum delay when requested. Librarians continue protesting that there is no coordination among existing abstracting and indexing services and that the resulting duplication runs up their operating costs unnecessarily. They try to render equal service subject-wise to their varied clientele, yet gaps in coverage continue. The multiplicity of record does insure each worthy client the particular assistance he requires. Through his professional society the scientist client must take the initiative and responsibility for secondary publication and for the gradual improvement of its character. He must see that a service of that sort adequate to his group needs is produced, and that once established it is not allowed to languish or devolve into limited correlative usefulness. The fact that your fundamental science has already met this problem, or met it five times over, does not give you the privilege of ignoring it. Next month or next year another society to which you grudgingly pay dues may decide its particular literature interests are not now being well served. Is the bryologist unhappy with the *Index Kewensis?* Not that I have heard. Can the helminthologist make do with *Biological Abstracts?* Or the *Q.C.I.M.?* Where is Echo? Probably buried in the pages of *Helminthological Abstracts.* Does the phthisiologist need a *Tuberculosis Index,* the *Q.C.I.M.,* and the *Current List of Medical Literature,* as well as the abstracts of the *American Review of Tuberculosis, World Medicine,* or *Excerpta Medica* (to choose only within the English language)? Let us cooperate to reduce the number of these secondary publications and strengthen and expand those retained. Then we can riffle fewer pages with a saving in time to locate the same amount of information. If supplementary financial support is necessary to transfuse a weakened index, the scientist collectively may seek assistance from government and industry, who also derive benefit from these tools. But the primary load must be borne by and the drive for having the abstracting or indexing service must come from the learned society itself or several such societies acting jointly. Is it too much to hope for further extension of cooperation between learned societies, as exemplified by the American Physics Society and the American Chemical Society who now agree on periodical abbreviations to be used in their publications? Or as between physicists in England and in the United States for one abstracting tool?

The difficulty of locating unpublished documents, those near-print publications which were spawned in such profusion by governmental research contracts during World War II, has been resolved by key government agencies, such as O.T.S., the Library of Congress, the Atomic Energy Commission, and the Department of Defense. These agencies have acknowledged their responsibility to reduce to order the main body of those cluttered contributions and their continuing postwar stream.

We hear occasionally about the gaps in the organized coverage of knowledge and uncertain figures on the annual amount of published work that does not find its way into the main current of secondary recording for the sake of future reference and potential application or extension. It should be the responsibility of those who raise the issue (and who obviously must feel this lack most keenly) to convey to the scientific public not only the importance of the missed knowledge, but some more adequate conception of what it comprises and how it can be suitably encompassed. Have we crusading critics armed with facts? A solution seems to be evolving in UNESCO for that material coming from workers in geographically isolated areas, through new regional publications for documenting serials. What of foreign patents? Are they being given adequate treatment?

It has been claimed that no communication has lasting value unless it influences and modifies human behavior, either of individuals or of masses. When it does that, it attains social significance. But in science who shall say when that influence will begin to be felt? Will each crucial new contri-

bution have a Meitner to herald at once its applicability, or will it lie dormant or buried like contributions of Gibbs or Mendel until times are more receptive or propitious, or until supporting knowledge has been advanced to a point to meet it halfway? Future generations may scorn or ignore much of what we prize today. If they are solely dependent on the author's original account, they may also have a trying time deciding what it was we had ascertained or what we meant to convey, let alone the merit and utility of our findings. In the time of Isaiah, it was a vexation to understand the report. In the time of the Skeptical Chymist, Boyle pleaded with his Fellows for a clearer style in writing and avoidance of unnecessary subtleties which do not increase knowledge but merely puzzle men. In the time of Andrade, which is today, a plea is still being entered that the scientific account be kept free from unnecessary clumsiness and obscurity. Authors of scientific articles need not disregard syntax to establish their credit. They may safely dust off the copybook virtues of sincerity, clarity, and simplicity in writing, and then resort if necessary to compression. They are again exhorted when so doing to attack one subject at a time and to remember that not all readers have the nimbleness of goats in leaping elliptically from topic to topic without transitional stepping stones. Obscurity in scientific writing has yet to be hailed as a virtue. Recently the Royal Society went so far as to invite authors to write for the hundreds who might be interested in some aspect of a well-written paper rather than for a specialist handful.

Scientific motion picture film literature resources are looming larger as basic records and as texts and are well entrenched as means for imparting techniques. The organization of film into the scientific record constitutes another literature problem. I will not risk further blurring of this outline by discussing film here, but merely call your attention to it.

The solution to some of the problems cited appears to lie in maintaining cooperation in the exchange of knowledge: cooperation between scientists as individuals, as groups in societies and institutions, and internationally; and between scientists and other organized contemporary groups. Let us seek expansion wherever cooperation actively exists today, as in the improvement of secondary literature resources by reducing the numbers of such services, but concomitantly by increasing the specific scopes of those retained, and work to revive cooperation where it is discovered to have lapsed. One of the peripatetic correspondents of the *Lancet* says that "Highly educated people only seem more reasonable." To advance, scientists must do more than "seem" cooperative.

Publications ripple the surface of the sea of knowledge, increasing by greater or lesser degree the solute content. Yet the body remains essentially in repose. Its level never drops but deepens, for knowledge does not evaporate from the record. Items may sink to unfathomed places, but knowledge departs only when the record itself is destroyed, or it fails initially to get entered in that record. Paul Schilder[4] in his monumental book, *Brain and Personality,* remarks, "It's strange that we know so little about the state of mind of a dizzy person." I submit that the scientist has been made professionally dizzy by having to cope continually with waves of that fifth alchemical element, paper. Prudent cooperation in its use, preparation, and dissemination would help to restore normal equilibration in the advancement of knowledge.

References

1. Du Bridge, Lee, "The National Research Program." Sigma Xi address, Cambridge, Mass., November 30, 1945.
2. Erskine, John. In: *The Moral Obligation To Be Intelligent,* pp. 57–58. New York: Duffield (1916).
3. Bronowski, J., "A Proposal for Atomic Peace." *Discovery,* 12, 75 (1951).
4. Schilder, P. F., *Brain and Personality.* N. Y. Nervous and Mental Disease Monograph Series, No. 53, p. 12, 1931.

The Teays River, Ancient Precursor of the East

RAYMOND E. JANSSEN

Dr. Janssen is professor of geology and head of the department at Marshall College, Huntington, West Virginia. He did his undergraduate work in geology at Northwestern University and received his Ph.D. from the University of Chicago. He was formerly on the staffs of the Chicago Natural History Museum and the Museum of Science and Industry in Chicago. He was active in the preparation of geological exhibits for the Chicago Century of Progress Exposition in 1933 and for the Texas Centennial Exposition of 1936. In addition to research interest in coal paleobotany and physiography, he has actively fostered popularization of science and scientific education.

A mighty river, coursing toward the sea, presents a wondrous spectacle of power, strength, and endurance. Its surging waters have cut into the bedrock and stripped away the strata which once lay across its valley. Unceasingly at work, it has become the master of its environment, entrenching itself into the landscape of which it is a part. The stream is the creator of both the valley and the hills; and in creating them, the river inscribes the history of its own eventful past.

The pathway of the river, however, may sometimes be beset with difficulties. Upheavals of the lands, invasions by the sea, advances of glacial ice, landslides, all tend to turn the river from its course. If they be great, the river may be turned aside; if overwhelming, the river meets its end. Such was the fate of one of America's grandest rivers. Unseen by man, it was the master stream of a prehistoric age, a precursor of rivers that flow today.

More than half a century ago, geologists working in the basin of the great Ohio River first noticed certain peculiarities of the river valley. They saw that some portions of the valley seemed to be much younger than others, that some of the river's tributaries appeared to be older than the master stream, and furthermore, that certain confluent valleys showed evidence of former occupancy by torrential currents no longer flowing through them. This led to the conclusion that the Ohio River had not always flowed in its present course, but that during some time in its history it had abandoned portions of its well-established valley and had carved out another route. With this, it was reasoned, had come adjustments in its tributary drainage. Summarizing this accumulated knowledge and adding much of his own, W. G. Tight in 1903 worked out partial details of these changes.[1] Among these was the recognition of a great abandoned valley extending across West Virginia, from Huntington to Charleston, through which the Ohio River was presumed once to have flowed. Averaging a mile and a half to two miles wide and nearly fifty miles long, the valley is occupied today only by minor streams that drain the immediate territory and are incapable of having excavated so great a valley in the bedrock.

To this valley Tight gave the name Teays, from a tiny crossroads station located within it. He also applied this name to the former river which flowed through it to distinguish it from the present course of the Ohio River. He did not know that one day the name he had proposed would become applicable to a greater river—a river which was once the master stream of interior America, with the Mississippi as a tributary. He was unaware that the Ohio River had not yet been born when the Teays flowed across the lands. The story of how the Teays helped to carve a great continent, of how it ultimately ceased to exist, and of how the Mississippi later became the master stream of the interior was not fully realized until nearly half a century later.

The prehistoric Teays, precursor of the present Mississippi, and predecessor of the Ohio, the Illinois, the Wabash, and others, had its source in the Appalachian Mountains of North Carolina (Fig. 1). From there it followed a northwestward course across Virginia and into West Virginia as far as

Charleston, along the same route occupied by the New and Kanawha rivers today. From Charleston it continued due west through the abandoned valley to Huntington, and then swerved northward to Chillicothe, Ohio. Here it resumed a northwestward course past Springfield, Ohio, to the Indiana state line southeast of Fort Wayne. It then turned south and formed a great loop to the north. After reaching its northernmost point in Fulton County, Indiana, the Teays swerved southwestward to Lafayette, proceeded west into Illinois, passing near Champaign, swung down toward Decatur, and then back northwest to Lincoln, Illinois. At this point it was joined by its tributary, the Mississippi, which then flowed considerably east of its present channel. The Teays continued to Beardstown, Illinois, and followed the present lower Illinois River Valley as far as the latter's confluence with the modern Mississippi Valley near St. Louis. Here the Teays received drainage from the western plains through channels which later became identified with the present Missouri River. The Teays continued for a short distance past St. Louis where it emptied into the Gulf of Mexico, an embayment of which formerly extended northward to this point. Here was the mouth of the great Teays River. With its headwaters in the Appalachians in the East and in the Rockies in the West, the tributaries draining the Great Lakes region on the north and the Kentucky terrain on the south, the Teays was the master stream of a primeval America.

The discovery of this ancestral river was not the accomplishment of a single individual. It was the culmination of study and exploration made in recent years by a great many geologists working individually in the scattered territories through which the river flowed. Gradually it became evident that several streams, shown on present-day maps as individual rivers, are really disconnected portions of a former big, single river. Associated underground waters were also found to be moving along definite buried channels. Finally, the entire course of the ancient river became apparent.

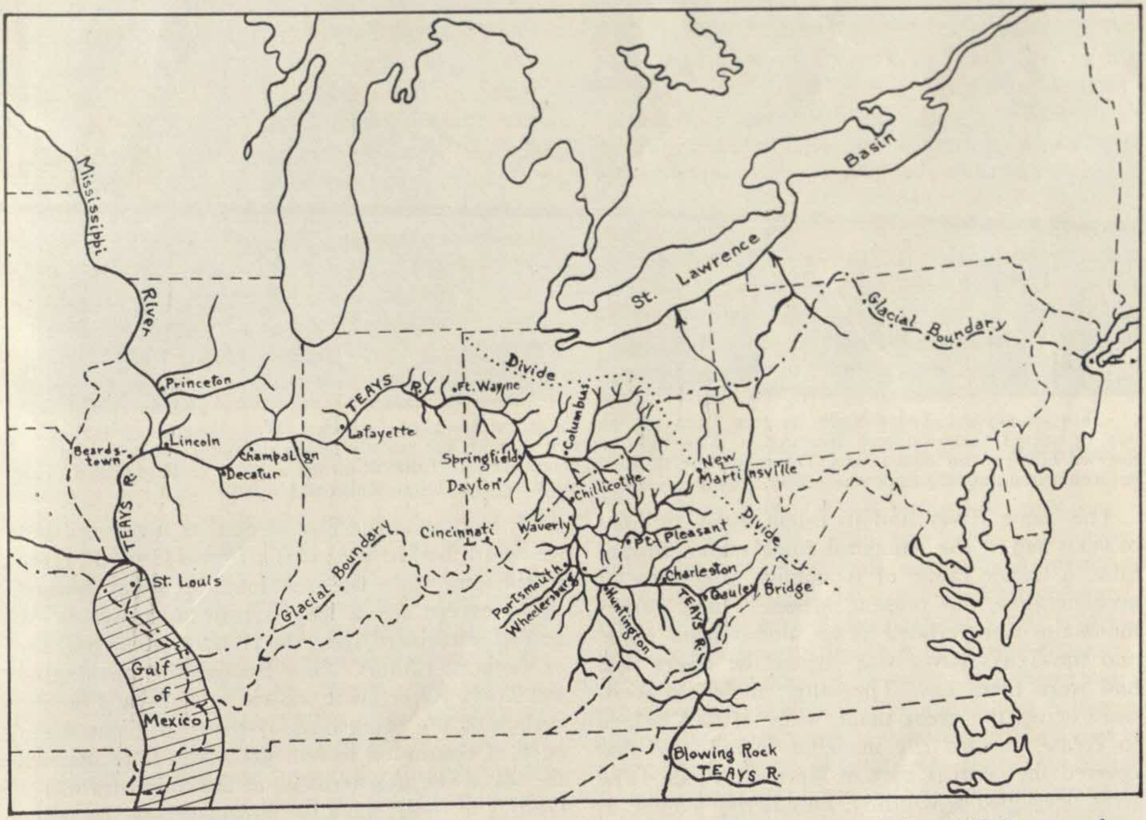


FIG. 1. The Teays River and its chief tributaries in so far as they are known. Some of these still flow as surface streams today; others are completely buried under the glacial debris. Also shown are the northern extension of the Gulf of Mexico and the line of southernmost advance of the Ice Age glaciers. (Adapted from Tight, Fidlar, Ver Steeg, Lamb, and Horberg.)

The abandoned Teays Valley as seen from the air above the town of Culloden, West Virginia, about midway between Charleston and Huntington. The light portion through the middle marks the old river bed, and the forested hills rise on either side. The main line of the Chesapeake and Ohio Railroad and U. S. Highway 60 can be seen extending through the valley. (Courtesy of Chesapeake and Ohio Railroad.)

The Teays River had its origin many millions of years ago in the Ancestral Appalachian Mountains, a higher range of mountains, preceding in geologic time the present ranges. These earlier mountains were eroded to an almost level plain, and the Teays River was one of the rivers that had worn them low. Thereafter, it flowed westward across the great plain, which it had helped to create, toward an immense inland sea that covered the central part of North America. The river developed a winding, meandering course as it crossed the plain.

In the course of time, pressures from within the earth lifted the plain to a high plateau, with the uplift highest in the East so that its surface sloped westward toward the interior of the United States. At the same time, the great inland sea was drained away, except for a long narrow arm which extended northward from the Gulf of Mexico as far as southern Illinois. These changes did not destroy the Teays River, as it was carried upward on the surface of the rising land. With its gradient steepened, it continued to flow down the new slope to the sea at the northern end of the long arm of the Gulf. The uplift gave the stream renewed energy, and it cut its way downward through the uplifted rock layers. The course of the river could not be straightened; hence, it entrenched itself in the

bedrock, while retaining the shape of the meandering course which it had developed previously on the low, flat plain.

Evidence of this can still be seen in the gorge of the New River, which is the present name for the upper portion of the Teays where it flows from North Carolina to central West Virginia. The deep canyon, with its nearly vertical walls and winding course, marks the extent of the river's erosion since the uplift. Similar relationships may be seen throughout the vast Appalachian region wherever other streams have incised their valleys into the great plateau. From some high vantage point, such as those along the Blue Ridge Parkway, one can see that the Appalachian ranges of today are essentially flat topped and of nearly equal elevation in their highest parts. If one imagines all the valleys refilled with the great quantities of rock that once were there, he has reconstructed the vast, rolling plateau surface that existed before the valleys were cut into it. The present Appalachian ranges, with their long, flat-topped summits, are remnants of the former plateau which has been dissected by the stream-cut valleys between them. The Blue Ridge marks the eastern limit and highest part of the former plateau. The steeply tilted rock layers seen in the sides of many of the ranges are the spreading roots of the Ancestral Appalachians, now re-elevated and dissected into numerous parallel ranges. Hence, the Teays, older than the present mountains themselves, actually held its course while the bedrocks were pushed upward from beneath it.

The headwaters of the Teays consisted of at least two main forks. One, rising in eastern West Virginia, is known today as the Gauley River. The other, rising in North Carolina, is the present New River. It is longer, and was the main headwater channel of the Teays. It rises today near the resort town of Blowing Rock, at the summit of the Appalachian Divide. Originally it extended much farther east to the present Fall Line along the eastern base of the mountains. This was before the eastern portion of the Blue Ridge was eroded to become the Piedmont area. Streams flowing down the east side of the Blue Ridge directly into the Atlantic had much steeper gradients than did the Teays and others draining toward the Gulf of Mexico. Consequently, during the intervening ages, the divide has been shifted farther and farther west by erosion, resulting in the disappearance of the uppermost headwaters of the Teays. Contrary to its name, the New River, as the remaining headwater portion of the ancient Teays, is one of the oldest rivers in America. Because it was there long before the mountains were carved, it is the only river crossing the entire Appalachian belt from one side to the other.

The union of the New and Gauley Rivers at Gauley Bridge, West Virginia, forms the present Kanawha River, which was a part of the ancient Teays as far as Charleston, West Virginia. The valley of the Kanawha here crosses the heart of the vast Appalachian coal field. Along the steep valley sides can be seen the entries to coal mines which extend back under the hills on either side. Along the river banks are great chemical plants which process the many products made from coal.

A few miles below Charleston, near the town of St. Albans, the Kanawha suddenly turns out of the old valley of the Teays and pursues an independent course northwestward toward the Ohio River. The Teays Valley proper, however, continues westward across the remainder of West Virginia. It was this valley that Tight long ago recognized as the abandoned course of a great river. Thick beds of sand and gravel, including water-worn boulders up to twelve inches or more in diameter, lie upon the valley floor. Many, composed of rocks quite dissimilar to the bedrock of the valley, show unmistakably that they were washed by river action from the bedrock region of the Blue Ridge. Only a great and powerful stream could have accomplished this. At places where erosion has removed the river gravels and exposed the bedrock beneath, potholes ground in the bottom of the former channel provide further evidence of the river's course. Along the full length of this broad valley floor runs today the main line of the Chesapeake and Ohio Railroad, its right-of-way selected long ago because it provided the only natural avenue of direct travel through the terrain of the West Virginia hills.

Within the city of Huntington, West Virginia, the abandoned valley of the Teays suddenly ends. Here it joins the stream bed of the more recently formed Ohio River, which enters the city from the northeast. The occupied valley continues toward the northwest, past Ashland, Kentucky, and Ironton, Ohio, to the town of Wheelersburg, about ten miles east of Portsmouth, Ohio, where the Ohio River again leaves it. For this distance of about forty miles, the younger Ohio River has appropriated a portion of the old Teays Valley. Within this section, a major tributary of the Teays, the Big Sandy River, entered from the south. It is still in existence and forms part of the state boundary between West Virginia and Kentucky. Rising on the northwest slopes of the Appalachians, it is now a tributary of the newer Ohio River.

Potholes, ground into the bedrock when pebbles are whirled about by streamwater, have been found in a part of the abandoned Teays Valley in Huntington, West Virginia.

At Wheelersburg the Ohio River leaves the Teays Valley and flows westward to the Mississippi; but the old Teays Valley continues almost directly north past the towns of Piketon and Waverly, between which our newest atomic energy plant is now under construction, and thence on to Chillicothe, Ohio. This section of the Teays Valley is now occupied in part by the Scioto River, which flows south to join the Ohio instead of flowing north as did the Teays. During the Great Ice Age, large amounts of sand and gravel were washed into the valley from the north, thereby reversing the slope of the valley floor.

If one drives northward along the present highway to Chillicothe from the south, he approaches the city through the old Teays Valley. The adjacent hills are rather flat topped, and en masse present an even horizon line against the sky. This skyline level is the western continuation of the vast plateau surface that inclines eastward to the top of the Blue Ridge where the Teays had its source. As one continues toward the city, he sees the distant buildings at a slightly higher elevation, although still within the valley. This gradual rise of the valley floor results from the partial filling of its bottom with glacial sands and gravels, poured into it by the meltwaters from the receding continental glaciers of the Ice Age. The city is built on the surface of this valley fill. The great ice sheets which moved into the United States from Canada during the Pleistocene Epoch advanced almost to the northern edge of Chillicothe.

Continuing northward from the city, the traveler ascends the gradually sloping surface of the valley fill until he is on a vast rolling surface composed of gravelly sand, silt, and clay, which completely bury all vestige of the previous land surfaces over which the glaciers moved. The ice, in its movement over the land, planed off the higher hilltops, filled the valleys with this debris and, in place of the former plateau surface, left a broad, rolling topography which continues to the shores of the Great Lakes.

The long valley of the Teays, which can be followed from its source in North Carolina, disappears at Chillicothe under the blanket of glacial drift. Geologists who first traced the course of the old valley as far as Chillicothe had no means of tracing its buried course. Some thought it continued northward to the Great Lakes. Others thought that it turned westwardly into Indiana and met the valley of the Wabash. Indeed, as late as 1943, the lower course of the Wabash was thought to be a continuation of the original Teays Valley.[2]

Knowledge of the actual course of the Teays, from Chillicothe to its former mouth at the Gulf of Mexico, has come from the recent study of thousands of well records. Such records, in the case of water wells, show the elevation at the present ground surface, the depth to ground water, the type of material penetrated, and, if the well goes deeply enough, the depth of bedrock. Because of the greater depths of oil and gas wells, additional information about the bedrock becomes available. By plotting the well locations on maps, and showing the depth to bedrock for each, the topography of the preglacial buried land surface has been determined. Thus, it has become known that the general bedrock surface from central Ohio westward into Illinois represents a continuation beneath the glacial drift of the gently sloping plateau surface. In many places the wells have penetrated the drift to depths of 200 to 300 feet deep before reaching bedrock. The distribution of these deeper wells follows a definite pattern and indicates the long winding course of the Teays River from Chillicothe to southern Illinois.

Not only has the course of the buried river been traced, but the slope of its bed and the width of its floodplain have also been learned with reasonable accuracy. At Chillicothe, where the valley floor first becomes lost under the thick deposits of glacial material, its elevation above sealevel is 620 feet. As it enters eastern Indiana it is 508 feet, and near Lafayette in western Indiana it is 384 feet above sealevel.[2] It continues to drop gradually across Illinois until a minimum elevation of 280 feet is reached where the valley of the Teays is again exposed and is occupied today by the Illinois River. The elevation of the Teays bed at its point of discharge into the Gulf of Mexico south of St. Louis is not yet definitely established. However, at a point fifteen miles south of St. Louis,

where the surface elevation is about 400 feet above sealevel, bedrock was reached at a depth of 277 feet,[3] which would be about 123 feet above sealevel. Whether this represents the actual bed of the Teays or the bottom of the Gulf of Mexico, which was then becoming filled with delta deposits, cannot yet be definitely stated. Sealevel just prior to the Great Ice Age was higher than at present, so that this indicated elevation of the Teays near its mouth might coincide with the possible sealevel of that time. On the basis of the known elevations of the river bed between Chillicothe and western Illinois, however, the Teays River had an average drop of seven inches per mile,[4] which coincides closely with the average gradient of the Mississippi today between Cairo, Illinois, and its mouth.

The width of the exposed Teays Valley between Charleston, West Virginia, and Chillicothe, Ohio, averages about one and one-half miles. The buried portion gradually widens to about four miles near the Indiana-Illinois line. Near Decatur, Illinois, it is about five miles wide, and near Lincoln, where the Teays was joined by the Mississippi, the valley floor broadens to almost fifteen miles.[4] The Teays was a massive and well-established river, as the size and gradient of its buried valley indicate.

The Teays River was dismembered into several smaller rivers and partly buried by direct action of the great glaciers of the Ice Age. The massive sheets of ice which moved into the northern United States from Canada traveled farther south in western Ohio, Indiana, and Illinois, than elsewhere. The ice extended into northern Kentucky in a few places and made its farthest advance in southern Illinois where the lowest elevations were. The entire lower course of the Teays River below Chillicothe was overridden by the ice. The Teays was overridden and all its tributaries within the glaciated area were covered.

The well records which have shown the location of the buried Teays Valley have indicated the courses of many of the buried tributaries. In so far as these are known, they are shown on the accompanying map. Most interesting of these was the primeval Mississippi which flowed southward from the border of Canada. Originally it did not make the big bend around west-central Illinois. Instead, its course diverged at Clinton, Iowa, and flowed near the middle of Illinois to meet the Teays near Lincoln. Farther south, the Teays received at least one major tributary from the west, the predecessor of the Missouri River, which had a somewhat different course than at present and was not so long.

Originating, perhaps, in the days of the dinosaurs, the ancient Teays established its course from

The New River Gorge as seen today from Hawks Nest State Park, West Virginia. Here the river follows the same winding course that it had in ancient times. The nearly level skyline marks the surface of the elevated plateau into which the river has since entrenched its channel. (Photo by U. S. Geological Survey.)

the Blue Ridge to the Gulf. With its great network of tributaries, it helped carve the landscape of a large portion of the continent. The amount of sediment—mud, silt, sand, and pebbles—which it eroded and carried to the sea must have been tremendous. The sea into which it poured those sediments was the long narrow arm of the Gulf of Mexico. This long seaway, from southern Illinois to New Orleans, has been completely filled, and the great delta now juts far into the Gulf proper.

The building of the delta has been attributed to the Mississippi River, which now follows its entire length. This part of the Mississippi, however, has been in existence for a brief time in comparison with that of the former Teays. It seems evident that the greater bulk of the delta was built by the Teays, with the Mississippi adding only the latest portions. Hence, the immense delta, more appropriately, might be called the delta of the Teays.

It is possible that the Teays may have extended its course considerably beyond the point near St. Louis which was originally its mouth, just as the Colorado River has filled the northern end of the Gulf of California, and must today flow over this extended land. If the Teays had accomplished as much as the Colorado in this respect, its lengthened course over the filled land would have extended well beyond Cairo, Illinois. This would mean that it had as an additional tributary the ancestral Ohio River, which then was a relatively small, short river with its source near Cincinnati. If the Teays had extended its course much farther before becoming extinct, it may have had essentially the same additional drainage as does the lower Mississippi today.

The great Teays River ceased to exist as a surface stream with the coming of the vast glaciers of

the Ice Age. For some reason, as yet unknown, the subarctic climates in the North began to deepen. The snows fell more frequently and lasted longer. The temperatures were lowered to the extent that the long winter snows did not all melt in the short summer months. The unmelted snows packed into ice, and as the years passed, the ice fields grew larger. Great mountains of solid ice took form in Greenland, Labrador, central Canada, and the Canadian Rockies. The great weight of the thickening ice caused it to sprawl outward from these centers of accumulation. These behaved like gigantic mounds of stiff molasses, slowly spreading in circumference as more ice continued to gather at the tops of the domes.

Eventually, the ice moving outward from one dome merged with that from another. Finally, all Canada was blanketed with a continuous ice sheet from sea to sea. In the north, the ice moved toward the pole; in the south toward the United States. Covering hill and valley, the blanket of ice grew thicker as it slowly pushed forward. Attaining thicknesses of 10,000 feet, or possibly more, the sliding sheet of ice advanced along an irregularly scalloped front, with great lobes protruding ahead of the main mass. Inching forward over the land, it wrecked everything in its path. The topsoil and underlying mantle of weathered rock were churned and plowed. Chunks of broken rock became frozen into the bottom of the ice, and as these were dragged along by the advancing glacier, they scratched and gouged the barren bedrock over which they moved.

Slowly the vast sheet of ice moved across the region of the Great Lakes. The lakes were not present then, for they were born of the Ice Age. It was then a region of hills and valleys, probably similar to that of southeastern Ohio and West Virginia today, with the streams draining toward the St. Lawrence River. The valleys were deepened and widened and changed in shape; and the materials gouged out of them were carried forward by the advancing ice.

By this time the great mass of ice was overriding the upper Mississippi and encroaching upon other northern tributaries of the Teays. The mammoth, the mastodon, and the musk ox, which had ranged far northward along the shores of Hudson Bay, found themselves migrating ahead of the towering ice sheet. They crossed the Teays River in great numbers. Today we find their fossil bones in Kentucky and West Virginia, and some as far south as Florida and Mexico.

Eventually, the advancing ice reached the banks of the Teays and slowly moved across it, burying both the river and its wide valley beneath a blanket of debris. The ice mass inched its way almost to the tip of southern Illinois before it stopped. Its irregular front tapered backward in each direction, into New England on the East, and into the Montana Rockies on the West.[5] The lower course of the Teays below Chillicothe lay buried beneath the vast sheet of glacial ice.

The long wall of ice, which now covered the upper course of the river, became a great natural dam beyond which the headwater flow could not penetrate. Consequently the upper waters became ponded, converting the stream into a long narrow lake confined within the valley walls. Thick layers of finely laminated clays deposited in this lake bottom indicate that it stood several thousands of years before the ice melted and released the ponded waters.[6] Perhaps, when the lower course of the Teays had been uncovered, stream waters again surged through its partially filled valley. On the other hand, if its valley had by then become completely filled with glacial debris, it is unlikely that the river ever returned to its previous surface course.

This much, at least, we do know. The great glacier, which had moved southward over the Teays and had later melted, was only the first of four which followed each other in geologically rapid succession. Each of these advanced along its own individual front, so that the points of farthest advance do not coincide. In many places, the later ice sheets extended beyond the limits of the earlier ones. Each had the effect of partially or completely obliterating the deposits dumped by the preceding ones. In only a few places can all four successive beds of glacial deposits be found on top of one another. At least two, probably three, and possibly all four of the great glaciers of the Ice Age overrode some portion of the lower Teays Valley. What the first glacier may have failed to accomplish was completed by one or another of the others. When the last of these glaciers had melted back for the final time, the entire lower valley of the Teays was deeply buried. The valley was filled with deposits of gravel, sand, and silt, and the glacial debris covered the entire landscape, the highlands and the former valleys. The average thickness of the material which now blankets the north-central United States is in the neighborhood of fifty or sixty feet.[7] In some places it is thinner, in others it is thicker by a hundred feet or more. Below this general depth are the buried valleys whose bedrock bottoms sometimes extend several hundred feet deeper.[8]

The ponding of the river waters, which were

Aerial view of the Ohio River where it cuts diagonally across the valley originally occupied by the Teays River near the outskirts of Russell, Kentucky. (Courtesy of Chesapeake and Ohio Railroad.)

blocked by the ice during one or more of its advances across the valley of the Teays, flooded the main valley above Chillicothe and those of the entering tributaries. Such valleys became long finger lakes. Nearly all the tributaries in southeastern Ohio and adjacent regions of Kentucky and West Virginia, in their lower courses, then held standing water instead of flowing streams. As a result, the bottoms of these temporary lakes, like that of the main Teays Valley, received layers of fine silts and muds that settled out of the water in great thicknesses over the coarser streambed sands and gravels. Eventually, some of these lake waters overflowed their rims, cutting through low divides in the enclosing hills. New systems of drainage evolved, and when these lakes were finally drained by the melting of the glacial dam, stream patterns bearing little resemblance to the former Teays system were in effect.

Portions of the Teays Valley, such as the abandoned section between Huntington and Charleston, West Virginia, as well as similar abandonments in various tributaries, were thereafter cut off from direct connection with the new stream systems. In other cases, the drainages were completely reversed because the glaciers had left the new land surfaces sloping generally southward from the Great Lakes. For example, the Allegheny and Monongahela rivers in Pennsylvania were originally northward-flowing tributaries of the St. Lawrence River, whose headwaters extended into the region of the Great Lakes before the glaciers gouged out those tremendous basins. The direction of their flow was reversed by the advancing ice, and they were made to merge at Pittsburgh, thereby sending a flood of water southward—the start of the modern Ohio River. These waters poured southward toward Huntington where they found the now-abandoned valley of the Teays, which they followed as far as Wheelersburg, Ohio. Additional waters, pouring southward all along the melting front of the glacial ice, added their torrents to the new river. Since the river was prevented from continuing to Chillicothe in the old valley, which now

had a slope in the opposite direction, the waters broke over the low divide to the west and poured into the previously existing lower Ohio Valley which then had its source near Cincinnati. Thus the Ohio River, as we know it today, came into existence, replacing in part the surface system of the Teays.

A million years have passed since the advancing ice of the first great glacier slid down over the valley of the Teays. This represents a mere fraction of the much greater length of time that the age-long Teays had dominated the drainage of preglacial interior America. But during this relatively much shorter time, the ice sheets completely changed the face of the lands over which they moved. They established the Great Lakes, they left 10,000 smaller lakes in Minnesota, they turned the headwaters of the Missouri southward, they pushed the lesser Mississippi to the west, and sent the combined waters of a new river system down across the old delta of the Teays.

In spite of these tremendous changes, the Teays River is not totally extinct. Its headwaters, between North Carolina and central West Virginia, still flow, under different names, along the identical age-old channel. At St. Albans, they were simply diverted to add their flood to the new Ohio. But much more important is the fact that the greater, buried portion of the Teays still carries its waters across Ohio, Indiana, and Illinois. Because it is much easier for rainwaters and melted snow to percolate between the loose sands and gravels that fill the buried valley than it is for them to seep through the bedrock on either side, there remains an avenue for the movement of ground water along the old channel. The Teays River is not really gone; its waters still flow slowly underground.

The discovery of the buried Teays Valley as a carrier of subsurface water has greatly advanced the cause of geologists whose task it is to search for adequate supplies of ground water. In many parts of the United States our expanding economy and increasing population have drawn so heavily upon the supplies of water that many communities and areas have found themselves dangerously short of this basic necessity even when no drouths exist. In the future, geologists concerned with such problems will search for buried river channels with the same diligence that they now search for hidden pools of oil and gas.

References

1. Tight, W. G. *U.S. Geol. Survey, Profess. Papers,* No. 13, 1903.
2. Fidlar, M. M. *J. Geol.,* **51**, 411 (1943).
3. Flint, R. F. *Ibid.,* **49**, 626 (1941).
4. Horberg, L. *Ibid.,* **53**, 349 (1945).
5. Antevs, E. *Bull. Geol. Soc. Amer.,* **40**, 631 (1929).
6. Janssen, R. E., and McCoy, G. P. *Proc. West Va. Acad. Sci.* **53**, 1953.
7. Ver Steeg, K. *J. Geol.,* **46**, 654 (1938).
8. ———. *Ibid.,* **44**, 918 (1936).
9. Janssen, R. E. *Sci. American,* **186**, 74 (1952).
10. Stout, W., and Lamb, G. F. *Ohio J. Sci.,* **38**, (1939).
11. Stout, W., Ver Steeg, K., and Lamb, G. F. *Ohio Geol. Survey,* 4th Series, *Bull.* 44, 1943.
12. Ver Steeg, K. *Ohio J. Sci.,* **46**, 297 (1946).

150 YEARS OF ATOMIC PHYSICS

One hundred and fifty years ago (Oct. 3, 1803) John Dalton, in a paper on the solubility of gases in water, made a remark which started the "Modern Atomic Theory." "An inquiry into the relative weights of the ultimate particles of bodies is a subject as far as I know entirely new. I have lately been prosecuting the inquiry with remarkable success. The principle cannot be entered upon in this paper but I shall subjoin the results as far as they appear to be ascertained by my experiments." He then gives a "table of relative weights of the ultimate particles of gases and other bodies," the first table of atomic weights.

Dalton's researches were made available in 1808, when he published his *New System of Chemical Philosophy.* "Therefore, we may conclude that the ultimate particles of all homogeneous bodies are perfectly alike in weight, figure, etc. In other words every particle of water is like every other particle of water: every particle of hydrogen is like every other particle of hydrogen, etc.," (*New System of Chemical Philosophy,* p. 142). (*Discovery,* Vol. 14, No. 10, October, pp. 318–320, 1953, also gives a reading list on the history of the Atomic Theory.)

BOOK REVIEWS

Eugenics: Galton and After. C. P. Blacker. 349 pp. $5.00. Harvard University Press, Cambridge, Mass. 1952.

FOR more than twenty years Dr. Blacker has been General Secretary of the Eugenics Society of London and a frequent adviser to the British Government on population problems. He has written or edited eight previous books, of which *The Chances of Morbid Inheritance* (1935) is perhaps the best known. His long association with and leadership of the eugenics movement in England eminently qualify him to discuss the past and evaluate the present trends in this field.

This book is divided into two parts, of which the first (126 pages) is a brief account of the life and work of Sir Francis Galton, founder of the Eugenics Society. Numerous pertinent quotations from Galton's writings are given. Blacker is particularly interested in describing Galton's personality and his attitudes toward religion, evolution, and eugenics in the Victorian world, in which there were many sharp controversies on these subjects. Galton's many and varied interests in scientific and social fields are discussed, and their startling variety is underscored by an appendix which lists the 227 titles in Galton's personal bibliography.

The second part of this book, entitled "After Galton," describes the development of eugenics after the death of Galton in 1911. The treatment is largely historic, describing developments as a logical outgrowth of Galton's views and emphasizing the debt to these of much current thought on eugenic and population topics. The chapter on population growth is well done, with a simplified description of the demographic cycle and pertinent remarks concerning the "demographic dilemma" which confronts the world today, particularly in reference to the problems of overpopulation in Asia. The discussion of recent developments in testing procedures leaves much to be desired, although the writer reminds us of reservations, qualifications, and difficulties encountered in various types of tests of intelligence, aptitudes, and skills that are all too frequently overlooked, even by those particularly trained in the use of such tests. The chapter on developments in genetics is the longest in the book (50 pages) and gives a brief review of some of the outstanding mileposts reached in the past 50 years, but it is more concerned with a discussion of philosophical aspects of genetic theory than with a presentation of historic fact. Fifteen pages of this chapter are devoted to discussion of "biological science in the USSR," with a description of the development and tenets of Lysenkoism and its opposition to Mendelian genetics. The author exhibits the expected revulsion for the determination of "correct" scientific attitudes by political edict, but seems hopeful that some compromise position based on fact and not on political expediency may eventually be reached in this controversy.

In the two final chapters, Dr. Blacker summarizes his views on eugenics under the title "Eugenics Today." There is discussion of standards of eugenic value, the uses of para-medical services such as marriage guidance, health examinations and birth control, problems of identification of eugenically favored and unfavored families, and of economic measures which could promote the well-being of children. Blacker's conclusions and recommendations for future eugenic progress should probably be accepted as representative of British thought and of the position of the Eugenics Society. His opinions are in many ways similar to those that have been expressed in this country by Frederick Osborn (*Preface to Eugenics*, 1951), and which largely represent the opinions of the American Eugenics Society. As the quality and quantity of its future citizens are certainly among the most valuable natural resources of a nation, these conclusions and recommendations are of outstanding importance and deserving of wide and thoughtful discussion.

C. NASH HERNDON

The Bowman Gray School of Medicine of Wake Forest College
Winston-Salem, North Carolina

Introduction to Logical Theory. P. F. Strawson. 266 pp. $3.50. Wiley, New York; Methuen, London. 1952.

THIS book has two main aims: to bring out the relations and contrasts between ordinary discourse and formal logic, and to clarify, at an introductory level, the nature of formal logic. It is also intended to include enough elementary material to provide a basis for the study of its philosophical aspects, and to serve as an introduction to more advanced technical treatise.

It begins with a discussion of the logical appraisal of statements in terms of the concept of inconsistency, and continues with an explanation of the object of formal logic, the use of formulae, the notions of logical form and logical system. Then we have a description of the propositional logic in terms of truth functions and truth tables, with some indication of the formulation of this logic as a deductive system. The author passes to the Boolean logic of classes as an alternative interpretation of the same abstract system, and as a part of the logic of propositional functions of one variable. He then treats the Aristotelean logic of classes and shows its consistency and its interpretability in terms of the Boolean logic. After a very sketchy introduction to propositional functions of several variables and the theory of relations, he analyzes in detail the relations between formal logic and the logic of ordinary discourse. He concludes with a chapter on inductive reasoning and probability.

The author falls between two stools in attempting to write simultaneously an introductory book for beginners and an original contribution to the logical analysis of ordinary speech. His effort to take care of the complications due to the vagueness, ellipticity, exceptions, and anomalies of ordinary discourse leads to

an awkwardness and obscurity of style, making the exposition unnecessarily difficult for the uninitiated, while the detailed discussion of some obvious matters in very elementary terms is often rather tedious for the better prepared reader. It leads him to write such sentences as: "Then we can frame our desired general entailments on the following model: any statement made by the use of a sentence which could be obtained by substituting a certain word or phrase for the variable in the formula 'x is twenty-nine years old' entails the statement made by the use in the same context of the sentence obtained by making the same substitution in the formula 'x is under thirty years old'" (p. 30), and "The standard or primary use of an 'if . . . then . . . ' sentence, on the other hand, we saw to be in circumstances where, not knowing whether some statement which could be made by the use of a sentence corresponding in a certain way to the first clause of the hypothetical is true or not, or believing it to be false, we nevertheless consider that a step in reasoning from that statement to a statement related in a similar manner to the second clause would be a sound or reasonable step; the second statement also being one of whose truth we are in doubt, or which we believe to be false" (p. 83).

At the same time, the limitation to an elementary exposition prevents him from developing the technical machinery needed to deal with the logical problems of language adequately. The author makes a valiant attempt, and has buried in his book some suggestive ideas. But the reader who patiently tries to dig them out will often be disappointed when the author raises an interesting problem, points out its importance and difficulty, and then makes no serious effort to tackle it.

The book is unsatisfactory as a preparation for further study because of its frequent use of nonstandard terminology and very meager references to the literature. Its insistence on the inadequacies of modern logic and the sins of most other writers on the subject, together with its concentration on matters which, from a beginner's common-sense point of view, would appear to be quibbling over trivialities, will hardly give him an appreciation of the beauty, fascination, and power of modern logical theory, and would, indeed, discourage him from pursuing the subject further.

The working scientist in a nonmathematical field usually becomes interested in logical theory for one of two reasons. He often wants to know the techniques of theory construction—how to organize his ideas into a deductive system, the uses and limitations of such a system, the things to look for and to avoid in such a theory. The author gives no example of a deductive system, nor does he indicate in any way the central importance of such systems in formal logic.

The experimental scientist usually wants the help of the mathematician or logician in the design of experiments or the interpretation of empirical results. He wants, for example, to know whether he is justified in drawing a certain conclusion from his data, or how to test a given hypothesis, or whether he can estimate a certain probability on the basis of his observations. The author, in his last chapter, is primarily concerned with the question: "What reason have we to place reliance on inductive procedures?" (p. 249). He decides that the question arises from confusion, that all previous answers are spurious, and that "induction is rational (reasonable)." He does not discuss the problem of how to assess the degree to which a given set of data support a given proposition.

It is unfortunate that the author has overburdened an elementary introduction with matters which more properly belong in a research publication. We hope that he will at least salvage his valuable insights on the logic of language in some future exposition, for experts, of a fully detailed technical treatment.

PAUL C. ROSENBLOOM

U. S. Department of Commerce
National Bureau of Standards
Los Angeles, California

Along the Great Rivers. Gordon Cooper. 159 pp. + plates. $4.75. Philosophical Library, New York, 1953.

NO matter how many books are written on the world's great rivers, there always seems to be room for one more. Especially is this true when an author speaks from the standpoint of a traveler and reporter, as Gordon Cooper does. His latest work appears to be something of a sequel to *Dead Cities and Forgotten Tribes* published a year ago. Both volumes feature people and the things they do, rather than places and statistics.

Name the important rivers that come easiest to mind on each of the continents and the chances are you'll hit most of the ten covered in this book—missing perhaps only one, the Murray, mighty life-line of South Australia and Victoria. The name probably should be hyphenated to include the Darling, for this tributary is as important to Southeastern Australia as the Missouri contribution to the Mississippi-Missouri system is to our own mid-continent states. The author tells the story of how Mildura, Victoria was reclaimed from a desert waste by dispossessing the rabbits and putting to work the millions of gallons of water flowing seaward. All of this happened within about the same time-span wherein U.S. pioneers performed similar miracles in areas of parched America.

On the other hand, the story told in the chapter on the Mississippi is one of floods only partially controlled; of Spanish and French influences still apparent; and of a strange community called Gee's Bend, a remote river spot uninvaded by automobiles or the tax collector until recently.

No one could possibly leave out the Nile in considering rivers that have meant most to men and civilization, nor the Yangtze Kiang, so vital to other untold millions for centuries before Marco Polo saw it, nor the Ganges, Holy River worshipped by India's multitudes.

With Cooper and David Livingston the reader invades Darkest Africa along the Zambezi to Victoria Falls, camps out in the African veldt. The Danube, Highway of Races, is another inescapable topic—Iron Curtain or no. Blue is its color, with poetic license—

muddy grey or dull green in actuality, according to Cooper. History, legend, and stories of commerce help round out the author's personal experiences, mostly in Vienna, the native or adopted home of Strauss, Schubert, Augustin, Gluck, Mozart, Haydn, Beethoven, and Brahms.

In the USSR the Volga was chosen over the longer Ob and Lena rivers, perhaps because almost everyone can appreciate how much the Volga means in the affections of the whole Russian people. Those who are footloose and fancy-free will lament with Cooper that "It seems strange today to realize that only 20 years ago foreigners were welcome visitors to Russia . . . that they could travel about more or less freely, in most parts of the country. You could, for instance, go to Cook's and buy a ticket right through from London to Tokyo, by way of the Trans-Siberian Railway, and you could likewise make a trip down (or up) the Volga."

Include also the St. Lawrence on your list and South America's Amazon, mightiest of all earth's rivers, and you will have duplicated the author's choices for chapter headings. The little volume is an excellent one for inclusion on the reference shelves of school and public libraries, even though it doesn't offer much to scholars and serious students of river lore, river commerce, or science.

HERBERT B. NICHOLS

U. S. Geological Survey
Washington, D. C.

Atoms, Men and God. Paul E. Sabine. x + 226 pp. $3.75. Philosophical Library, New York. 1953.

A research scientist, emotionally conditioned toward religion by having been raised in the home of a Methodist preacher, tries to answer the question, "Can I be intellectually honest in believing what, as a Christian, I profess to believe and at the same time accept the teachings of modern science and psychology regarding the nature of man and God and the physical world?"

While the answer is unquestionably affirmative, the precise nature of the reconciliation achieved is not too clear. In some places the author seems to adopt a Spinozistic position which conceives of the "final stuff" of the world as exhibiting itself under two aspects, physical and psychical. Such a view, of course, makes a place for spiritual values. In others he argues for a conception much like that of Weyl and Jeans, who find God in the world because of the remarkable "fit" which mathematical formulae have. In still other places he seems to accept a somewhat disguised Berkeleyan idealism, "that God and the atoms and the human soul are one in essence, a spiritual trinity, three expressions of the unity of the living Soul of a living universe" (p. 219). And one finds suggestions of Eddington and Compton when he employs the principle of indeterminacy to argue that "choice" is not unmeaningful when applied to the physical world.

The author is obviously sincere, and struggling with what is for him a vital problem. There is little of originality in the work; the "arguments" for the existence of God and the justifiability of religion are those which have been repeatedly discussed in the history of philosophy, and such as one can find, somewhat more carefully formulated, in any good introduction to philosophy.

A. CORNELIUS BENJAMIN

Department of Philosophy
University of Missouri

The Itinerant Ivory Tower. G.E. Hutchison. xi + 261 pp. $4.00. Yale Univ. Press, New Haven, Conn.; Oxford Univ. Press, London. 1953.

A mutual friend tells me that Mr. Hutchison toyed with the idea of calling his book "From a Plastic Tower." I rather wish he had, because a plastic tower sounds like a fresh and personal vantage point from which to view the world. Ivory is anachronistic, even when itinerant. To be sure Mr. Hutchison is anachronistic in some ways in this book. It is a book of essays, and in our plastic world the essay has given way to the article—a substitution that seems more deplorable than the change from ivory.

This is, then, an unusual book. Books by scientists tend to be either summaries of knowledge of a particular field or (rather rarely) popularization hopefully aimed at a wide audience. This is a wide book aimed at a narrow audience. At least the trade publishers would regard it as a relatively narrow audience—literate people with an interest in science and ideas and able, occasionally, to read meditatively. Maybe this audience is larger than the publishers think.

Most of the materials of the book were originally published as "Marginalia" in the *American Scientist*, but they seem to me to read better in book form than in the journal, and I don't think anyone should overlook the book just because he has followed "Marginalia." When the snippets are all strung together they give a much more adequate impression of an unusual and stimulating mind; and the diverse ideas, in close juxtaposition, gain considerably in force.

The word "biogeochemistry" doesn't occur in the book (at least it isn't in the index), though the holistic, biogeochemical point of view is implicit everywhere. There is as much anthropology as biology here, and perhaps more philosophy and criticism (in the old-fashioned meaning of the word). Many stretches of text are pleasantly enhanced by a haze of erudition—at least I found the erudition pleasant. I don't think the author was using the erudition to cover up flaws in his argument, but I don't really care.

A review should probably make some mention of contents; but there is such a diversity here that I don't know how to go about it. There is discussion of bird behavior in relation to psychoanalysis; analysis of Kroeber's too-little-known *Configurations of Culture Growth* and of Huntington's too-well-known *Mainsprings of Civilization*; some rather cutting remarks about the loves discovered in the Yale index cards by Ford and Beach; unreserved tribute to Ruth Benedict and D'Arcy Thompson; reflections on religion as a taboo subject.

My humanist friends often make snooty remarks

about narrow-minded and illiterate scientists. I am going to lend them this book. I think our graduate students often are pretty narrow-minded, and I am going to recommend it to them, too. But I hope I don't lose my copy because I'll be needing it in my own daily living—there are many things here that I aim to crib and pass on to my captive undergraduate audience.

MARSTON BATES
Zoology Department
University of Michigan

Succulent Plants: Other Than Cacti. A. Bertrand. 112 pp. Illus. + plates. $4.75. Philosophical Library, New York. 1953.

THIS small and unpretentious work of A. Bertrand has much to recommend it to the amateur who desires to learn something about succulents as well as to grow them. One only wishes he had elaborated a bit more upon the culture as well as the control of pests and diseases. This last, all too often overlooked by the would-be grower, frequently proves costly and disappointing in the loss of plants, and causes many gardeners to give up the growing of succulents as too difficult. Confining the descriptions to the more readily available plants is a wise step, and one which this reviewer wishes would be followed by more writers on similar subjects. The illustrations are excellent and well-chosen and give a good overall view of succulents in general. The descriptions are concise and would serve well to help one recognize the plants to which they refer. In the attempt to bring the nomenclature up to date, a few errors occur, but these need not detract from the unmistakable value of this little book for the beginner who wants to learn about succulent plants.

EDWARD J. ALEXANDER
The New York Botanical Garden

Our Neighbour Worlds. V. A. Firsoff. 336 pp. Illus. + plate. $6.00. Philosophical Library, New York. 1953.

IT is a pity that such entertaining style and generally lucid exposition should be wasted on a book of so little real worth. The author has tried to produce a popular account of present-day knowledge and theories about the solar system, combined with a discussion of the possibilities of space travel. He has read most of the elementary books and a few of the more advanced ones, but has not always understood what he has read. The reader to whom Mr. Firsoff has addressed his book would be well advised to consult the author's sources instead.

Examples of the author's lack of real understanding are to be found in his statement of Kepler's Second Law (p. 26), and his discussion of the sun's orbital motion in the galaxy (p. 18). The phrasing of Kepler's Second Law apparently is original, and that is probably why it is wrong. The velocity of the sun relative to the stars in its neighborhood is said to be its orbital velocity. The actual value is about 10 times larger. The references to "astigmatic lenses" (p. 60), "General Vanderburg" (p. 95), and the "MacDonald Observatory" (p. 292) are probably due to carelessness and not ignorance.

The first four chapters give a general astronomical introduction and a discussion of theories of the origin of the solar system. Firsoff has his own theory, but his discussion of this includes calculations based on a figure for the number of stars in the galaxy (p. 41) which is too small by a factor of 10. The next four chapters are devoted to the possibilities of space travel, and these are followed by six chapters giving a survey of the planets and their satellites, and the asteroids. Comets are ignored, and meteors are discussed in connection with problems of space travel. The final chapter is a mathematical appendix.

Other books by the same author are listed as follows: *Arran with Camera and Sketchbook, The Cairngorms on Foot and Ski, The Tatra Mountains, Ski Track on the Battlefield,* and *The Unity of Europe.* In preparation is: *In the Hills of Breadalbane.* The "blurb" on the jacket of the book describes him as a "fully qualified and practical astronomer." This is a mislabeling of an intellectual product comparable with the mislabeling of worthless, but harmless, drugs and chemicals.

FRANK K. EDMONDSON
Goethe Link Observatory
Indiana University

A Free Society. Mark M. Heald. xii + 546 pp. $4.75. Philosophical Library, New York. 1953.

THE future of democracy may indeed be an article of faith, but it is by no means a certainty. In an ever more complex world where the individual is ever less secure, democracy is being required to repel attacks and dispel doubts under conditions that will tax it to the utmost. The extent of its success will in the long run depend on the capacity of those who cherish it, to re-examine its problems and reconsider its very essence in the light of the new conditions that mark society at mid-century.

These tasks Professor Heald has undertaken in *A Free Society,* not as a contribution to political science but in order "to provide the average citizen of a flourishing democracy with some guides for his thinking with regard to an admittedly confused and paradoxical popular understanding of a vital and practical political concept."

This is a large order and a commendable enterprise to which he brings a thoughtful analysis and a considerable talent for condensation. The book is comprehensive, if necessarily somewhat superficial in places. Attention is given in about equal proportions to the philosophical and historical aspects of democracy and to a description of its present state, its challenges, and the way it can be made to function better and to flourish. Heald has a laudable and understandable conviction about the superior merits of democracy. He is also aware of its imperfections and inconsistencies, its vulnerabilities and dilemmas. Many of the points he makes are well taken; for example, his comments on the prevailing pre-

occupation with security as the prime value in society, the negative attitude of Americans toward political power and the institution of government, or the unsolved problems of economic democracy and the challenge of the welfare state.

Like most of mankind, however, the author is better at describing problems than at offering solutions. Perhaps it is unreasonable to expect more in the way of prescriptions than the exhortations and generalizations he often provides. As a guide to the average citizen the book would be more interesting and of greater value, in any case, had he made more frequent use of specific instances and illustrations throughout. *A Free Society* is nonetheless a thoughtful and a useful general treatment of a vital topic.

William E. Diez

Department of Government
University of Rochester

Heredity in Health and Mental Disorder. Franz J. Kallmann. 315 pp. Illus. $6.00. Norton, New York. 1953.

INTEREST in medical genetics is increasing all the time, and nowhere is that interest more intense than in the area of mental health and mental disorder. The author of this volume has spent twenty-five years in research on schizophrenia and other forms of mental anomalies, and for sixteen of those years has developed the Department of Medical Genetics at the New York State Psychiatric Institute. He is best known for his formulation of the twin-family methods of investigation.

The book is in three major parts. The first section deals with the general principles and methods of human heredity. It is clearly and interestingly written and provides a valuable introduction to the more technical matter of the following sections. Part Two discusses heredity in relation to specific mental disorders. Many case histories from the personal files of the author are presented, to illustrate clearly the principles and conclusions at which he arrives. The author rightly insists on a sharp differentiation between schizophrenia and manic-depressive psychosis, which have been thought by some to be either clinically or genetically related. Evidence is presented for the dependence of manic-depressive psychosis on a dominant gene substitution with incomplete penetrance. In schizophrenia, on the other hand, a recessive gene appears to be involved. The chance of developing schizophrenia increases in direct proportion to the degree of blood relationship to a schizophrenic individual: about one per cent if no relatives are known to have the disorder, 7 per cent for a half brother or sister, 14 per cent for a full brother or sister, or a fraternal twin, or one parent; and 86 per cent for a schizophrenic identical twin.

Also discussed are involutional psychoses, senile psychoses, epilepsy, and neurological disorders such as amaurotic idiocy, paralysis agitans, Huntington's chorea, and diffuse cerebral sclerosis. The author is not convinced that the dominant gene hypothesis for cerebral dysrhythmia, as shown on electroencephalograph records for epilepsy, is fully substantiated.

The final section is devoted to a very fine discussion of the applications of genetics in mental health planning. All counselors, psychiatrists, and others who are called on for family guidance should read this part carefully. It is very well done. The book is copiously illustrated throughout and is finely printed and bound.

Laurence H. Snyder

Graduate College
University of Oklahoma

The End of the World: A Scientific Inquiry. Kenneth Heuer. 220 pp. Plates. $3.00. Rinehart, New York. 1953.

SEVERAL years ago a Moscow astronomer, A. Vorontzoff-Veliaminov, criticizing the description of the possible future explosion of our sun given in a book by L. Goldberg and L. H. Aller, wrote: "The purpose of such realistic descriptions in the capitalistic world is to prove the futility of the life on the Earth, and to undermine the will of the people to rebuild their social order." Judging by these standards, the recent book by Kenneth Heuer represents supercapitalistic propaganda, since from the beginning to the end it discusses all thinkable, and a few unthinkable, ways in which our little planet might meet its doom. Heuer does so in very vivid, realistic language supported by a number of impressive illustrations. The book contains a large amount of interesting historical information concerning historical predictions of the end of the world. We learn, for example, that, according to Bernard, a hermit of Thuringia, the end of the world was due in A.D. 992, the year when the Annunciation of the Virgin fell on the same day as Good Friday. This year passed, and the world still continued! According to Nostradamus, the king of astrologers, the end of the world had to come on the day when Easter Sunday would fall on St. Mark's day (i.e., April 25th). It is amusing to learn that, according to the calendar accepted in Nostradamus' time, such a coincidence was absolutely impossible, but it became possible according to the new (Georgian) calendar introduced in 1582, twenty-six years after Nostradamus' death. The next danger-day to watch is April 25th, 2038!

Having covered the historical background of the problem, the author turns to the world catastrophes which could be expected on the basis of strictly scientific astronomical data. He considers the possibility of a collision of the Earth with the head of a comet, which, without destroying our globe, might cause damage comparable to an atomic bomber's attack in intercontinental warfare. Heuer refers to astronomical calculations which show that in some year A.D. 50,000,000,000+ the moon will approach very close to the Earth, and, being broken into a thousand pieces, will stone the surface of our planet by rocks as large as mountains. He also considers the eventuality of a star encountering our solar system and either colliding with the sun itself, or at least kidnapping and carrying away with it our poor Earth. Although all such perils of collision possess exceedingly small probabilities, they are all in principle possible,

and might happen either within the near or the distant future.

In subsequent chapters, the author turns his attention to the future of the Earth that would result from the future evolutionary history of our sun. He gives a vivid description of boiling oceans and melting rocks in the case of a nova-like explosion of the sun, and paints a severe picture of ice-bound Rio de Janeiro in the case of the sun's thermal death.

K. Heuer's book came out a little too early to include the most recent views on solar evolution, according to which, in a few billion years from now, our sun is bound to expand into a red super-giant, gradually engulfing in its slowly swelling body the system of inner planets to which it once gave birth. It seems, in fact, that such an expansion will precede the explosion itself, and subsequent cooling.

Having dealt with all natural causes that may terminate life on the surface of our planet, the author turns his attention to the "man-made end of the world." He quotes the experts in atomic explosions to the effect that all life on the earth can be completely destroyed by a number of hydrogen bombs that could be produced by a major industrial nation in the course of five to ten years, and at a cost of only $40,000,000,000. This certainly looks like a much more reasonable way of destroying humanity than all the astronomical stuff!

On the whole, the book represents a very amusing, instructive, and exciting reading.

G. Gamow

Department of Theoretical Physics
George Washington University

Books Reviewed In SCIENCE

November 6

The Human Senses. Frank A. Geldard. New York: Wiley; London: Chapman & Hall, 1953. 365 pp. Illus. $5.00.
Reviewed by William R. Amberson.

Starch: Its Sources, Production and Uses. Charles Andrew Brautlecht. New York: Reinhold, 1953. 408 pp. Illus. $10.00.
Reviewed by Roy L. Whistler.

Psychiatric Dictionary. With encyclopedic treatment of modern terms. 2nd ed. Leland E. Hinsie and Jacob Shatzky. New York: Oxford Univ. Press, 1953. 781 pp. $15.00.
Reviewed by G. N. Raines.

November 13

Human Behavior: Psychology as a Bio-Social Science. Lawrence E. Cole. Yonkers-on-the-Hudson, N. Y.: World Book, 1953. 884 pp. Illus. $5.50.
Reviewed by Melford E. Spiro.

Praktische Arbeitsphysiologie (Applied Physiology of Human Work). Gunther Lehmann. Stuttgart: Georg Thieme, 1953. (U. S. distrib.: Grune and Stratton, New York.) 355 pp. DM 33.
Reviewed by Josef Brožek.

November 20

Undtersuchungen über die Tiergemeinschaften des Bodens: Die Oribatiden und ihre Synusien in den Böden Norddeutschlands. Karl Strenzke. *Zoologica,* Band 37, Heft 104, Stuttgart, 1952. 172 pp.
Reviewed by Edward W. Baker.

The Sulfapyrimidines. Lawrence H. Sophian, David L. Piper, and George H. Schneller. New York: A. Colish, for the Lederle Laboratories, 1952. 180 pp.
Reviewed by E. E. Campaigne.

Visceral Circulation. A Ciba Foundation Symposium. G. E. Wolstenholme, Ed., with assistance of Margaret P. Cameron and Jessie S. Freeman. Boston: Little, Brown, 1953. 278 pp. Illus. + plates. $6.50.
Reviewed by Frederick P. Ferguson.

Deformation and Flow in Biological Systems. A. Frey-Wyssling, Ed. Amsterdam: North-Holland Pub.; New York: Interscience, 1952. 552 pp. $11.50.
Reviewed by L. J. Mullins.

November 27

Symposium on Chromosome Breakage. (Suppl. to *Heredity,* 6 [1953]). Held at the John Innes Horticultural Institution, June 9–11, 1952. London-Edinburgh: Oliver & Boyd, 1953. 315 pp. Illus. + plates. $7.50.
Reviewed by Karl Sax.

Stochastic Processes. J. L. Dobb. New York: Wiley; London: Chapman & Hall, 1953. 654 pp. $10.00.
Reviewed by D. ter Haar.

Inorganic Thermogravimetric Analysis. Clément Duval. Amsterdam-Houston: Elsevier, 1953. 548 pp. Illus. $11.00.
Reviewed by Thos. De Vries.

LETTERS

LESS AIMLESSNESS IN EDUCATION*

First, I wish to mention two peripheral items in the article. One is the rigging of questionnaires in such manner that unwanted responses are not possible. Unfortunately it is a trick that is not unknown in other educational surveys and, of course, it is not to be condoned. Indeed, the questionnaire method itself, even when honestly used, has its limitations in serious research. There is frequently a temptation to regard as true and right anything that a majority of respondents to a questionnaire believe. But "truth cannot be manufactured from error, no matter how often it is repeated."

The other preliminary comment is on Dr. Bestor's castigation of the "educational bureaucracy," composed of state departments of education, colleges of education, and some voluntary organizations like accrediting agencies. It is true that some of these have arrogated to themselves unwarranted extra-legal powers to set up educational requirements, including such things as standards for teacher certification. For example, there is a teacher who secured a temporary permit from the state department of education to teach safe driving classes. After teaching such classes successfully for three years, he could secure a renewal of his permit only by taking summer school courses in fire prevention and in the prevention of industrial accidents! Ridiculous regulations like this should soon bring an exposure of the intricate operations they resort to for increasing their bureaucratic control.

To turn now to the main theme of the article—aimlessness in education—it should be noted that the appearance of aimlessness results not so much from lack of purpose as from uncertainty in the incomplete process of adapting secondary education to changing conditions. Besides this, the contradictions posed by Dr. Bestor are more apparent in educational theory than in classroom practices.

A number of historical developments in secondary education contribute to this present puzzle. For the purpose of brevity and simplicity, only a few of them will be treated. It is an oversimplification to regard these developments as having occurred in a brief space of time, near the turn of the century. But for the purpose of making clearer their interaction one upon another, they may be regarded as nearly contemporary occurrences.

The first in importance and the longest continuing is the gradual increasing enrollment in secondary schools. In 1900 less than half of the boys and girls aged fourteen to seventeen were in school. Now, after fifty years of expanding secondary enrollment, close to 80% of this age group are in school. The most rapid increase came early in the century. It was not caused by a sudden thirst for knowledge on the part of numerous adolescents but by the rising standard of living. Higher personal incomes now enabled more families to give leisure to their young people and to excuse them from wage earning or from helping on the farm while they went to school.

This great expansion of the secondary school population was more than an increase in numbers. It was also the coming of a different type of pupil. Up to this time the secondary school had existed chiefly for only one purpose, namely, preparation for college. Only the very brightest and ablest of the young people, whose parents at the same time had the means to contribute to their support during their student years, were encouraged to continue their education into high school. Those who could not maintain the pace of Latin, mathematics, history, and science for four years dropped out. Those who survived were the screened best, acquiring that "intellectual discipline" of which Dr. Bestor writes so wistfully. For two and a half centuries secondary education was of this sort—college preparatory. In the course of the years it became standardized and formalized. It was this kind of education to which some of our country's ablest leaders and greatest thinkers attributed their success. But probably it was more consequential that the educational system had screened and selected the top level of intellectual ability. Now with the big expansion of secondary education, the screening was less fine and more young people stayed in high school even though they had no plans to go to college. Their parents demanded for them something besides preparation for college. They demanded "practical subjects."

This was the second factor contributing to the present confusion. A "new curriculum" was developed, mostly under popular pressure. The demand was not only for utilitarian subjects but also for less difficult academic disciplines, such as general mathematics, general history, general science, and citizenship. In 1917 the Smith-Hughes Act passed Congress, providing for federal aid for instruction in manual training, agriculture, home economics, and other vocational courses. Thousands of public schools in even the small towns across the country took advantage of the new law. As the percentage of teen-agers that stayed in school increased, the average intelligence quotient of the school population declined. Other practical and easy courses were added to the curriculum until today there is proposed "life adjustment education." The old uniform college preparatory curriculum gave way to the elective system in most high schools, where the offerings were arranged in groups of required and elective subjects. Now the secondary school assumed a dual purpose. In addition to preparing for college, it also undertook to educate pupils who were not destined for higher education. There was uncertainty about what this second purpose should be. Prep-

* A comment on the article, "Aimlessness in Education," by Dr. Arthur E. Bestor, Jr., *The Scientific Monthly*, 75, 109 (1952).

aration for life came to be the concept preferred over preparation for a vocation. Later it was citizenship that received wide emphasis as the new purpose, or character training. Recently it has become life adjustment.

The third development was new knowledge about the psychology of learning. In support of the old curriculum there had flourished the "theory of formal discipline." It was the name given to the belief that the mind, like a muscle, could be strengthened or developed by exercise on hard subjects, and that then the newly acquired power could be transferred to new and different problems. It was also known as the "transfer of training." The mind was to be disciplined and made strong on school subjects and the strength was in later life to be transferred to any kind of problem. The harder the school subjects, the stronger the mind was supposed to grow.

When psychological research and testing were applied to this theory it was found wanting. Experiments did not uphold it. Some transfer of training was indeed found to occur, but scarcely any was found in pupils with low intelligence. The brighter the pupil, the greater was the amount of transfer. Thus it became more profitable for those of lower ability to concentrate on specific learnings rather than to strive for general intellectual discipline. This new point of view further promoted the offering of vocational and utilitarian subjects with their specific knowledge and skills. And the high school was confirmed in its dual purpose, offering intellectual discipline to abler pupils who could affect a transfer of training, and life adjustment education to the less able.

The final development to be considered is the influence of John Dewey and the "Progressive Education" movement. Briefly, the emphasis was to be turned to the child instead of the subject, to the importance of interest in learning, and to learning-by-doing. The forces that seemed to be forming to divide educational interests into a classical party and a vocational party were now greatly accelerated. Soon a conservative party supplied vigorous opposition to the "Progressives," upholding formal discipline and the traditional curriculum. One faction later went in for the "great books." Through the years the slogans of the two parties varied more than their steady vehemence. Once they were "indoctrination" vs. "freedom"; at another time "competence in subject matter" vs. "the child centered school"; or "essentialism" vs. "the activity curriculum"; more recently they have been "the three R's" vs. "life adjustment education." The spokesmen in the controversy pulled no punches, not stopping short of overstatement and exaggeration and thus causing the apparent aimlessness. To the confusion of many laymen, they often spoke and wrote as if they expected that their views were to prevail in all education, regardless of the level of pupils' intelligence. In the article under consideration, Dr. Bestor seems to wish all pupils, the bright and the dull, to have sound "intellectual discipline." The writer of the Illinois Secondary School Curriculum Program,* as well as A. H. Lauchner,† both of whom Dr. Bestor refutes, write as if they expected that all pupils, both the bright and the dull, are to be given such pap as the 55 problems listed in the Illinois curriculum bulletin.

The key to the reconciliation and interpretation of these two points of view is the dual purpose of American secondary education. It is also the key to less aimlessness. The same school has the brilliant youth who seeks intellectual discipline adequate for his coming college studies and his later life career, as well as those with meager mental endowment, some of whom can scarcely learn to read and for whom even life adjustment problems will be a challenge. To be true to its dual purpose the school must adjust its curriculum to both levels of ability. In Europe the accepted way of meeting this problem has been to establish separate schools for different abilities, and secondary education has there remained highly selective. But in the United States it has been considered more democratic to provide one school for all pupils and within that school to make the needed adaptations to varying abilities. As exceptions, in the larger centers of population some technical and vocational schools are operated, as is also the Bronx High School of Science for the mentally gifted. Undisturbed by educational controversy, the private secondary schools pursue their single aim, select their students, and prepare them for college. But for the most part, the public secondary schools throughout the land continue to receive in democratic mingling all the children of all the people and must continue to apply the principles of both sides of the controversy in the effort to adapt their instruction to those who have received ten talents and to those who have received one talent.

FRED J. KLUSS

Roosevelt High School
Cedar Rapids, Iowa

* Illinois Secondary School Curriculum Program, Bulletins Nos. 9, 10, 11, 13.

† Lauchner, A. H. "How Can the Junior High School Curriculum Be Improved?" Bulletin, National Association of Secondary School Principals, Vol. 35, No. 177, March 1951.

THE QUAESITUM

THE very able article entitled "On Absolute Measurement" by N. Ernest Dorsey and Churchill Eisenhart consists of extracts from a book by Dorsey, selected and arranged by Eisenhart. It is full of sound advice to the experimentalist but is founded upon notions of an absolute that have been foreign to the thinking of many people for half a century.

To begin with, the ordinary number system in terms of which measurements are made is a creation of man. To give it a position more fundamental than any other part of man's language is to overlook its basic nature. It was made by man for man's necessities; first counting, then measurement, and then the solution of algebraic equations.

What do we mean by the velocity of light? As commonly understood, we mean a number which we get when we do so and so. It is read as centimeters per second, if you will. What meaning can possibly be assigned to the term apart from human action? A value is used by us which has been reached by a chain of experi-

ments acceptable to present-day scientists. To talk of velocity of light apart from man's operations is simply making a noise. The absolute vanishes when viewed critically here as in other places in science. Physicists commonly speak of the temperature "absolute zero." Here the term absolute must be regarded as a name in every way similar to centigrade. No matter how the physicist may define absolute zero, its determination will lead back to measurements by man, and consequently is not only subject to what we may call experimental errors but subject also to the general mutability of human ideas.

The assumption that any kind of average of a sequence of numerical measurements will approach a limit when the number of experiments is increased seems naive in the extreme. In fact, the very word "limit" when we are considering experiments in science is subject to challenge. Certainly the man who uses it must state what meaning he is attaching to the term. Its use according to the accepted mathematical definition when applied to a set of experiments is non-sense.

The value of the mathematical limit in statistical studies of experimental data may be clarified to those who have not carefully considered the matter by the following brief remarks on the applicability of numbers and mathematical processes to physical science. Here the term "physical science" is to be construed very broadly and is to include much not usually described by the word, physical. A mathematical system starts with a set of undefined terms and certain postulates about them. There follow strings of theorems formed according to a certain logic, that is, according to an agreed set of rules. Now if a physical system consists of a set of objects which can be put into one-to-one correspondence with the undefined terms of a mathematical system and if these objects obey certain laws which can be put into one-to-one correspondence with the postulates of the mathematical system, we say that the physical system is isomorphic to the mathematical system. Applications of mathematics step into the picture. Theorems say: If you do thus and so, you will get so and so. Now are physical systems ever truly isomorphic to mathematical systems? Certainly not, if the mathematical system contains the infinite in any form and this includes the continuous. The simplest physical law, such as $pv = \text{constant}$, is at best an approximate description for certain experimental results and then its usefulness is assumed for limited ranges of the variables only. The assumption that p and v as continuous variables "really" obey this law assumes a fundamentalist definition of p and v apart from human action. "Faith" of this type may have its place in religion but is strictly without meaning in physics. Of course, formulas may be set up to cover a finite set of data. It may then prove to be more convenient to use limiting forms for these formulas obtained by strictly mathematical means. This is commonly done and has been most useful in many types of study. The worker should keep in mind that he is working with a mathematical construct only. For example, the entire subject of calculus in applied science is of this nature. No person in his right mind when working with calculus should assume that its rules are obeyed by a "divine" quaesitum.

It is not proposed to say to what degree it may be profitable for a worker to talk about limits in other than a mathematical sense and about quaesita. He should, however, have a clear realization of what he is doing and of the fact that physical science is not absolute.

TOMLINSON FORT

Department of Mathematics
University of Georgia, Athens

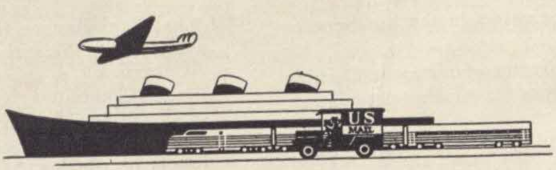
FATHER OF AMERICAN ANTHROPOLOGY

READERS of the very interesting diary kept by William Fellowes Morgan of a portion of his anthropological trip to the Southwest in 1878, edited by Professor Temple R. Hollcroft in THE SCIENTIFIC MONTHLY for September, 1953, may be interested to learn that a journal of the same trip kept by William Fellowes' "Uncle Lewis", Lewis Henry Morgan, was edited and published by the undersigned in *American Antiquity*, **8**, 1, 1942. The elder Morgan's journal was begun on June 21 at Canyon City on the way to southwestern Colorado. It ended at Taos Pueblo on August 7th, the day after William Fellowes' Journal began. The two journals thus provide an account of almost the entire trip.

It may be of interest, also, to note that Lewis H. Morgan, the "Father of American Anthropology," and President of the AAAS in 1880, made four ethnological field trips into the Great Plains and to Hudson's Bay territory between 1859 and 1862. The fourth of these trips was made by steamboat up the Missouri River as far as the Rocky Mountains. A brief account of these expeditions, based upon Morgan's journals, was published by the undersigned in *American Anthropologist*, **53**, 11, 1951: "Lewis H. Morgan's Western Field Trips."

LESLIE A. WHITE

Department of Anthropology
University of Michigan



ASSOCIATION AFFAIRS

PREVIEW OF THE 120TH MEETING, AAAS, BOSTON, DECEMBER 26–31, 1953

From advance registrations and Boston hotel reservation data, it is already evident that the 120th meeting of the American Association for the Advancement of Science will be both diversified and well attended—in the latter, quite possibly second only to the record-breaking New York meeting of 1949. Not only will all parts of the continent be represented, but a larger than usual number of distinguished foreign scientists will participate.

An inspection of the General Program-Directory, which is being sent advance registrants by first class mail at this time, shows that the 120th meeting of the Association will combine many traditional aspects and will also have several new features.

Special Sessions. One of the characteristic and most important features of the annual meetings of the Association is the series of outstanding general addresses by distinguished authorities, sponsored by organizations that meet regularly with the AAAS. These special events are joint sessions with the Association and are open to the general public of the city in which the meeting is held.

I. **Sunday evening, Dec. 27, Ballroom, Hotel Statler; 8:00 p.m.** American Association for the Advancement of Science and the Society of the Sigma Xi.

Speaker: A. V. Hill, Foulerton Research Professor of the Royal Society, University College, London, London, England; past president, British Association for the Advancement of Science.

Subject: The Design and Mechanism of Muscle (Illustrated).

Detlev W. Bronk, president, Rockefeller Institute for Medical Research, and chairman of the Board of Directors of the Association; and Lewis J. Stadler, professor of field crops, University of Missouri, president of the Society, will serve as cochairmen.

II. **Sunday evening, Dec. 27, Grand Hall, Mechanics Building; 8:30 p.m.** National Geographic Society.

Speaker: Luis Marden, member, Foreign Editorial Staff, National Geographic Society.

Subject: Sicily, the Forgotten Island (Illustrated).

Meredith F. Burrill, vice president for AAAS Section E, will preside.

III. **Monday evening, Dec. 28, Ballroom, Hotel Statler; 8:00 p.m.** AAAS Presidential Address.

Speaker: Detlev W. Bronk, president, Rockefeller Institute for Medical Research, and retiring president of the Association.

Subject: The Role of Scientists in the Furtherance of Science.

Edward U. Condon, director of research, Corning Glass Works, and president of the Association, will preside.

Preceding the address, Earl P. Stevenson, president, Arthur D. Little, Inc., and general chairman, seventh Boston meeting, will speak briefly.

Following the address there will be an informal AAAS Presidential Reception in the adjacent Ballroom Assembly. All registrants and members of local committees are cordially invited to attend.

IV. **Tuesday evening, Dec. 29, Ballroom, Hotel Statler; 8:00 p.m.** Scientific Research Society of America.

Joseph W. Barker, Research Corporation, president of the Society will preside.

Speaker: David B. Steinman, consulting engineer, New York, New York.

Subject: Suspension Bridges—The Aerodynamic Problem and Its Solution (Illustrated).

V. **Wednesday evening, Dec. 30, Georgian Room, Hotel Statler; 8:30 p.m.** United Chapters of Phi Beta Kappa.

Speaker: Leonard Carmichael, secretary, Smithsonian Institution.

Subject: Science and Social Conservatism.

Kirtley F. Mather, professor of geology, Harvard University, will preside. Warren Weaver, president elect, will represent the Association.

The Scientist in American Society. Early in the year, a committee of Section K and the AAAS Symposium Committee, without knowledge of each other's plans, both decided that there should be a program on some of the social and political problems confronting American scientists at the present time. The following 2 sessions, combined by mutual consent, are sponsored by the Association as a whole:

Sunday Afternoon, December 27

2:30 p.m.; Talbot Hall, Mechanics Building; *Symposium: The Scientist in American Society, Part I: Freedom for Scientific Inquiry.* Arranged by a committee of Section K-Social and Economic Sciences, Conrad Taeuber, assistant director, Bureau of the Census, secretary.

Detlev W. Bronk, *presiding*

1. The Beliefs and Expectations of the Public. Clyde W. Hart, Herbert Hyman, Paul B. Sheatsley, and Shirley A. Star, National Opinion Research Center, Chicago, Ill.
2. The Social Psychology of Political Loyalty in Liberal and Totalitarian Societies. Raymond A. Bauer, lecturer on social psychology and research associate, Russian Research Center, Harvard University.

Tuesday Evening, December 29

8:00 p.m.; Paul Revere Hall, Mechanics Building; *Symposium: The Scientist in American Society, Part II.* Arranged by a subcommittee of the AAAS Symposium Committee: Charles D. Coryell, professor of chemistry, Massachusetts Institute of Technology, chairman, P. M. Morse, and V. F. Weisskopf, professors of physics, Massachusetts Institute of Technology, and Bart J. Bok, associate director, Harvard Observatory.

EDWARD U. CONDON, *presiding*

1. The Need for and the Production of Scientists. HAROLD C. UREY, distinguished service professor of chemistry, University of Chicago.
2. Scientists and Other Citizens. GERARD PIEL, publisher, *The Scientific American.*
3. The Legal Basis for Intellectual Freedom. MARK DE WOLFE HOWE, professor of law, Harvard University.
4. Scientists and Political Action. EDWIN C. KEMBLE, professor of physics, Harvard University.
5. Discussion, led by EDWARD U. CONDON, director of research, Corning Glass Works.

The AAAS Science Theatre, a permanent feature of the Association's annual meeting, presents showings of the latest domestic and foreign scientific films—nearly all with sound—throughout the meeting period.

The Science Theatre is a feature for the pleasure and information of all registrants attending the annual meeting. It cannot be for the casual passerby; thus **admission is restricted to those who wear the AAAS convention badge,** or who show an Association registration receipt.

PROGRAM 1

Sunday Afternoon, Dec. 27, 2:00 p.m.–6:00 p.m.

1. CHEMICAL BRUSH CONTROL. American Museum of Natural History. Color. Sound. 23 min.
2. DEMONSTRATIONS IN PERCEPTION. United States Navy. Black-and-white. Sound. 30 min.
3. DECISION FOR CHEMISTRY. Monsanto Chemical Company. Black-and-white. Sound. 35 min.
4. LOCOMOTION OF SNAKES. New York Zoological Society. Color. Sound. 11 min.
5. GENETICS AND BEHAVIOR. Joseph J. Antonitis and J. P. Scott. Color. Silent. 16 min.
6. RADIOISOTOPES: THEIR APPLICATIONS TO HUMANS. Medical Film Guild, Ltd. Color. Sound. 32 min.
7. THE CHAIN OF LIFE. Pictura Films Corporation. Color. Sound. 11 min.
8. PROMINENCE ACTIVITY. Sacramento Peak Station of Harvard College Observatory, Sunspot, N. M. Black-and-white. Silent. 15 min.
9. LIVES OF THEIR OWN. Pictura Films Corporation. Color. Sound. 11 min.
10. MAN TO MAN. Mental Health Film Board. Black-and-white. Sound. 30 min.
11. BETTER AND SAFER HIGHWAYS. The Firestone Tire and Rubber Company. Black-and-white. Sound. 7 min.

Repeated as PROGRAM 4, **Dec. 29, 9:00 a.m.–1:00 p.m.**

PROGRAM 2

Monday Morning, Dec. 28, 9:00 a.m.–1:00 p.m.

1. WARNING SHADOW. National Cancer Institute and American Cancer Society. Color. Sound. 21 min.
2. LEONARDO DA VINCI. Pictura Films Corporation. Color. Sound. 68 min.
3. ANTARCTIC VIGIL. Australian News and Information Bureau. Color. Sound. 10 min.
4. SEE HOW THEY SWIM. Pictura Films Corporation. Color. Sound. 11 min.
5. TARGET NEVADA. Department of Defense. Color. Sound. 14 min.
6. WHICH FATE. National Society for Medical Research. Color. Sound. 28 min.
7. WATERS OF COWEETA. Forest Service, U.S.D.A. Color. Sound. 20 min.
8. WHITE SPLENDOR. Pictura Films Corporation. Color. Sound. 11 min.
9. HIGH QUALITY SPICULES AND CHROMOSPHERE. Sacramento Peak Station of Harvard College Observatory, Sunspot, N. M. Black-and-white. Silent. 15 min.
10. PROJECT TINKERTOY. National Bureau of Standards. Black-and-white. Sound. 27 min.

Repeated as PROGRAM 5, **Dec. 29, 2:00 p.m.–6:00 p.m.**

PROGRAM 3

Monday Afternoon, Dec. 28, 2:00 p.m.–6:00 p.m.

1. THIS IS MAGNESIUM. Bureau of Mines. Black-and-white. Sound. 15 min.
2. AUTONOMIC NERVOUS SYSTEM, PARTS III AND IV. J. E. Markee and R. F. Becker, Duke University. Color. Sound. 42 min.
3. SEE HOW THEY FLY. Pictura Films Corporation. Color. Sound. 11 min.
4. OAK WILT. National Oak Wilt Research Committee. Color. Sound. 22 min.
5. VOICES UNDER THE SEA. British Information Services. Black-and-white. Sound. 19 min.
6. THE EFFECT OF ELECTRO-CONVULSIVE SHOCK ON "CONDITIONED ANXIETY." H. F. Hunt and J. V. Brady. Color. Silent. 14 min.
7. BIRTH OF AN OIL FIELD. Shell Oil Company. Color. Sound. 11 min.
8. KING OF THE RIVER. Pictura Films Corporation. Color. Sound. 11 min.
9. LIFE STORY OF A WATER MOLD. Arthur T. Brice-Phase Films. Black-and-white. Sound. 11 min.
10. "A" IS FOR ATOM. General Electric Company. Color. Sound. 16 min.
11. NEW FRONTIERS IN SPACE. McGraw-Hill Book Co., Text-Film Dept. Black-and-white. Sound. 25 min.

Repeated as PROGRAM 6, **Dec. 30, 8:00 a.m.–noon**

PROGRAM 7

Wednesday Afternoon, Dec. 30, noon–4:00 p.m.

1. LIVING WATER SERIES, PART I: NATURE'S PLAN. Conservation Foundation. Color. Sound. 30 min.
2. RADIOISOTOPES, PART XII: AGRICULTURAL RESEARCH. Department of the Army. Black-and-white. Sound. 40 min.
3. THE SEA LAMPREY. Fish and Wildlife Service. Color. Sound. 13 min.
4. BATTLE OF THE BEETLES. Forest Service, U.S.D.A. Color. Sound. 16 min.
5. SAND AND FLAME. General Motors Corporation. Black-and-white. Sound. 20 min.
6. FLYING DOCTOR. Australian News and Information Bureau. Black-and-white. Sound. 11 min.
7. THE MECHANICAL INTEREST AND ABILITY OF A HOME-RAISED CHIMPANZEE. Keith J. Hayes and Catherine Hayes, Yerkes Laboratories of Primate Biology. Black-and-white. Silent. 60 min.
8. THE QUESTING MIND. General Motors Corporation. Color. Sound. 20 min.
9. WOODCOCK. Fish and Wildlife Service. Color. Sound. 14 min.

INDEX TO VOLUME LXXVII (JULY–DECEMBER 1953)

Title Index of Articles

Subject Index

Contributors

Books Reviewed

Book Reviewers

Verse

Letters